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# HISTORY OF SCIENCE.

J'AI TOUJOURS PENSÉ QUE LA MEILLEURE MÉTHODE DE POPULARISER LES ÉTUDES SCIENTIFIQUES, EN GÉNÉRAL SI PEU ATTRAYANTES, CONSISTAIT À EXPOSER COMME DANS UN PANORAMA, LES DIFFÉRENTES PHASES QU' UNE SCIENCE A PARCOURUES DEPUIS SON ORIGINE JUSQU' À L'ÉPOQUE OÙ ELLE SE PRÉSENTE À LA COUCHE TRANSITOIRE DES CONTEMPORAINS.—*Ferdinand Hoefer, Histoire de la Chimie, 2me ed.*

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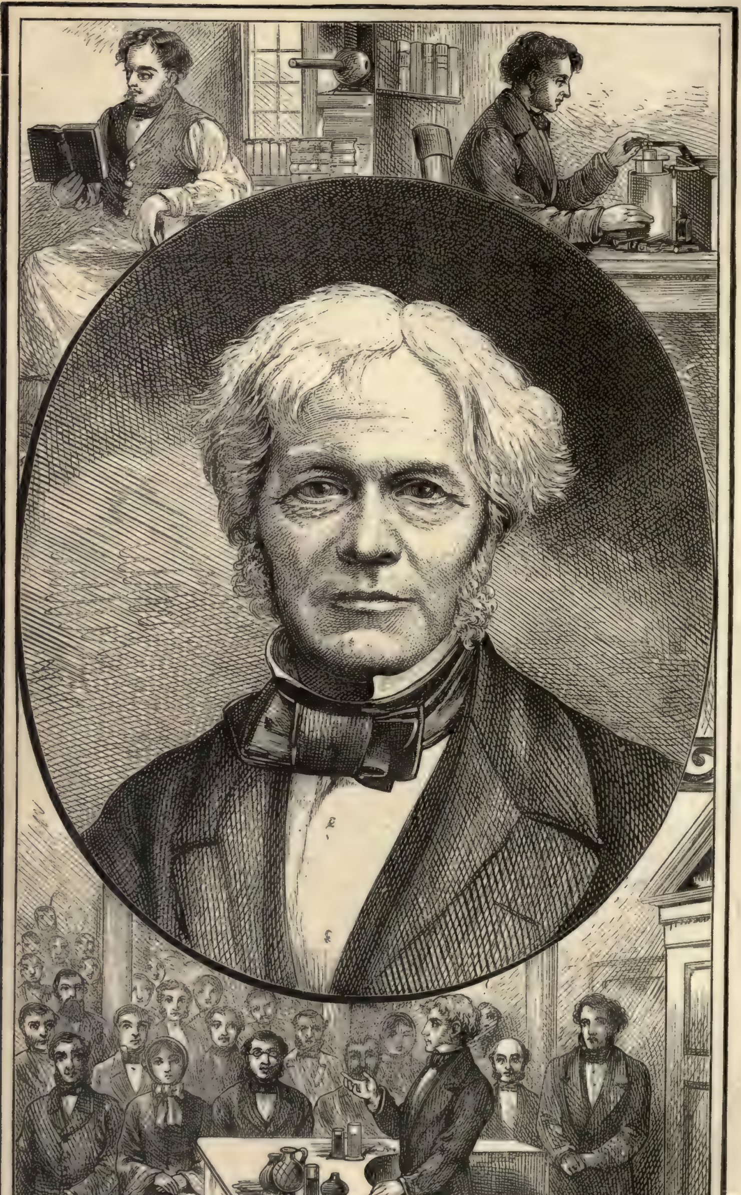

*Front.* FARADAY.

# A POPULAR

# HISTORY OF SCIENCE

BY

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*Author of*

"DISCOVERIES AND INVENTIONS OF THE NINETEENTH CENTURY"

"SCIENCE IN SPORT MADE PHILOSOPHY IN EARNEST," ETC.

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# PREFACE.

THROUGHOUT the following pages it has been the Author's aim to present the subjects of which they treat in a manner entirely compatible with the requirements of a book intended for popular use. Necessarily, therefore, the work has features very different from those which might be expected in a history of graver pretensions. The general reader is here supplied with simple illustrations of laws and principles, and with ample details of experiments and observations, which illustrations and details would in no wise be needed by the accomplished student. An expert in this or that branch of science might perhaps complain that we have not always brought into view those highly generalized conceptions of modern thought with whose value and importance he is familiar. But the highly general and abstract character which gives philosophic value to such conceptions render them unsuitable for popular presentation, for they must ever remain unmeaning and unintelligible to minds not prepared for their reception by profound and systematic study. Without, however, assuming the possession on the part of the reader of other attainments than those possessed by the average schoolboy or schoolgirl, who has had the ordinary opportunities of witnessing experiments at popular lectures, we believe that it is possible to place before him or her such an account of the principal steps in the progress of science as will be intelligible throughout. The whole body and matter of any department of knowledge cannot of course be all passed in review; but those researches, discoveries, and theories which have contributed to furnish its form and principles may be traced in their inception, progression, and completion. In the following chapters the endeavour has been to follow the main lines along which each science has developed, by adducing such specific experiments, observations, and reasonings as support or illustrate the most important results. The intermixture with the scientific matter of more or less brief biographical notes concerning the great men who have made science, and the introduction of the portraits of many of them, will, it is hoped, import into the present work some of that element of "human interest" in which books of pure science are often accused of being deficient.

As *science* is a word with a wide extent of application, it will of course be readily understood that it is used in the title of this work with a limitation of its sense, and this limitation will best be defined by an enumeration of the branches of science treated of in the present volume. These are: Astronomy; the Physical Sciences—light, heat, electricity, etc.; Chemistry, and the group which we venture to call the Natural History Sciences—botany, zoology, geology, etc.; and, to a certain small extent, Mathematics. As the last is a study popularly supposed to offer few attractions, we hasten to add, that it is only because they have proved mighty instruments in advancing the other sciences, that we have given some brief and very simple illustrations of the nature of such inventions as logarithms, the Cartesian geometry, and the infinitesimal calculus; but, as it is not to be expected that the general reader will most commonly be a votary of

> "The hard-grain'd Muses of the cube and square,"

no reference is made to the subsequent development of mathematics, and the recondite methods of modern analysis. It may here be proper to observe that as a science advances to perfection it is more and more concerned with processes of mathematical deductions. There are some sciences which have long consisted chiefly of deductions made from a few simple principles established by experience once for all. Such are mechanics, hydrostatics, parts of optics and of astronomy. The abstract mathematical forms of such parts of science make them less generally attractive, and where in the following pages these sciences are entered upon under the designation of "The Mathematical Sciences," it is usually some of the experimental or inductive particulars that are discussed.

The plan upon which the subject matter has been arranged is to give the first three chapters to ancient science, divided mainly into ancient Greek, Alexandrian Greek, Arabian, and Mediæval schools. A chapter is then apportioned to the science of the sixteenth century generally, while for each succeeding century separate chapters are assigned to the several sciences or groups of sciences. Three chapters, specially devoted to the respective labours of the epoch-forming men, Galileo, Bacon, and Newton, take their places among the rest and carry on the narration. The division of the matter according to centuries has of course no intrinsic significance, being adopted merely for convenience of reference; and the classification of the sciences into mathematical sciences, natural history sciences, etc., suggested by the titles given to some of the chapters, is to a great extent also arbitrary. Two more remarks we have to make for the reader's avoidance of misconceptions. He must not assume that the subjects which he finds touched upon in the following pages under the name of each philosopher necessarily represent the whole of that philosopher's useful scientific activity. Still less will he be entitled to suppose that the names of the few living men of science which happen to find a place in this volume are introduced by reason of the pre-eminence of their owners above the rest of the many distinguished cultivators of science, whose splendid labours also are advancing the boundaries of knowledge in every direction; it is only the limitations imposed by the scope and aim of our pages that hinder us from concluding with the display of a long roll of the honoured names and the inestimable works that add lustre to an age already rich

> "With the fairy tales of science, and the long result of Time."

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## ERRATA.

Page 9, line 9, *for* Airey *read* Airy; page 152, line 5, *for* quantities which admit *read* quantities which admits; page 529, line 30, and page 246, line 15, *for* Sir W. Thompson *read* Sir W. Thomson.

# HISTORY OF SCIENCE.

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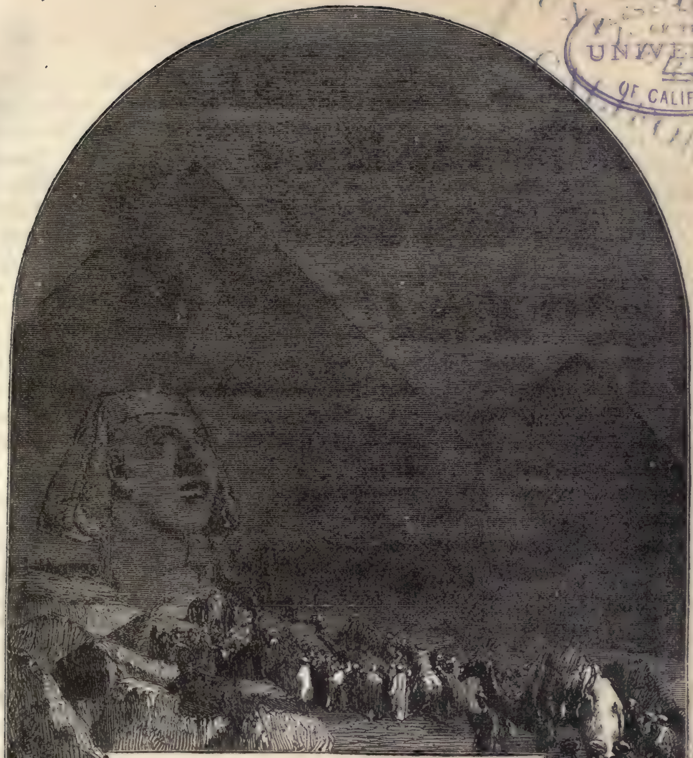

# HISTORY OF SCIENCE.

◆

## CHAPTER I.

### ANCIENT SCIENCE.

IF Science meant simply knowledge, its history would begin where that of the human race begins. For only by his capacity for acquiring knowledge could man ever have maintained his existence on the face of the earth. He is by nature unprovided with those means of attack and defence which other creatures possess; he is even unprotected from the vicissitudes of the seasons by the furry coat which covers the beasts of the field. But man soon learnt to fashion for himself weapons of attack and defence: he chipped the shapeless flints into spear-points; out of pebbles he formed sharp arrow-heads; between the extremities of the elastic bough torn from the trees he stretched a cord made of the twisted fibres of their bark, and thus found a means of sending winged messengers of death after the fleetest prey. He would soon disdain the accidental shelter of thickets and caverns when, by the exercise of his reason, he had learnt to construct habitations for himself. Rude indeed must those primitive dwellings have been; but, unlike the swallow which age after age continues to build her nest in the same way, man progressed in knowledge, until at length he was able to rear the solemn temple and the stately palace. Again, such food as the spontaneous bounty of nature might supply in fruits and roots, with which the uncertainties of the chase might be supplemented, would soon prove inadequate for the sustenance of the increasing tribes, and they must soon have learnt to sow and to reap, and to know seed-time and harvest. And when the knowledge of primitive man had extended to the management of fire, he greatly enlarged his powers, and acquired that specially human distinction—the art of cooking his food. We see, then, that at the earliest period the very necessities of life must have compelled men to observe the properties of the things around them; to study the appearance and habits of animals; to note the effects of heat; to recognize the signs which announced the return of the seasons for sowing their grain, and for gathering in the produce.

But however necessary such knowledge may be for existence, or in a more advanced state for comfort, it is not this which we mean by science. Take, for instance, the art of making bread, which was probably practised by the earlier races in some such manner as that represented in Fig. 1, wherein is depicted the process employed by certain savage tribes at the present day. Rude as the process is—and it consists only in spreading the paste, made of flour and water, on a series of flat stones which have been heated in a fire—its employment betokens the knowledge of a certain number of the facts of nature. It required the experience of perhaps many ages to impart the knowledge of other facts by which the originally rude process became improved. This progress of an art, from its rudest to its more advanced state, does not necessarily imply an advance in science. Arts originally improved by advantage being taken of such facts as accident revealed, or by experiments designed to discover facts, not for the sake of the knowledge itself, but in order to find better means of attaining practical ends. Science, on the other hand, seeks a knowledge of the truths of nature, not as the means to an end, but as itself the end. Men lived and acted for ages before they began to indulge in scientific inquiries, and the vast body of natural truths, first acquired in the endeavour to supply necessities and obtain comforts, formed the raw material, as it may be termed, from which the first philosophers attempted to con-

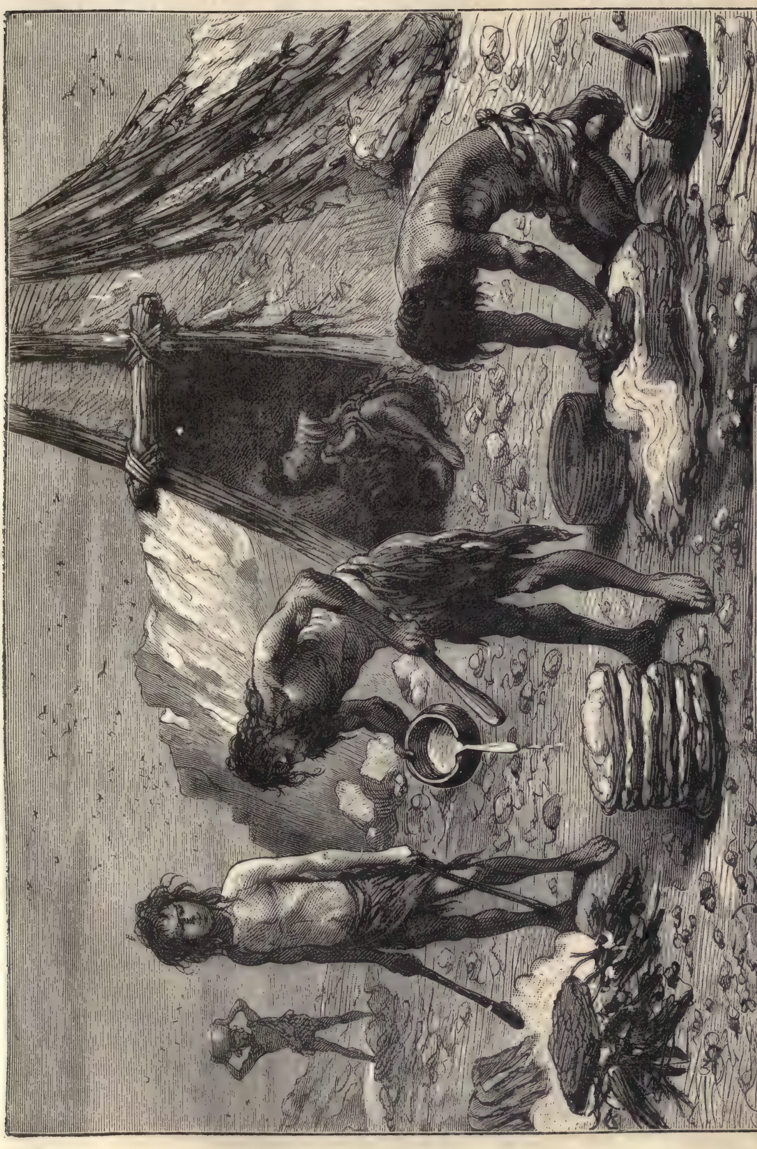
J. ANSSEAU
J. FÉRAT

FIG. 1.—PRIMITIVE BREAD-MAKING.

struct the fabric of science. The object of science then, as now, was, and is, to embrace in the smallest possible number of general truths the whole of the facts of nature. Hence a current definition of the word—"Science is the knowledge of many, methodically digested and arranged so as to become attainable by one." Thus science is concerned with the highest or most general truths, and regards these as its end and aim; while, on the other hand, arts are contented to occupy themselves with the lower or particular truths, and that only as the means of accomplishing some practical end.

Before the rise among a people of the scientific spirit which seeks the causes of phenomena in nature itself, it is observed that men have everywhere regarded natural phenomena as depending on the action of personal will. They have always supposed that the movements and changes they observed in the world were produced by the immediate action of powerful but invisible beings:

> Did raging storms o'er ocean's bosom sweep?
> 'Twas angry Neptune smote the troubled deep.
> Did clouds condensed emit electric fire?
> 'Twas Jove's wide-wasting instrument of ire.
> Did crops luxuriant fertile fields adorn?
> 'Twas Ceres decked the vales with wavy corn;
> Or Bacchus bade the high-embowering vine,
> Loaded with clusters, round the elm entwine:
> But if they perished by untimely blight,
> The Furies tainted the cold dews of night.

This transference to nature of the same kind of will and action which man knows so well in himself, has in all ages been the conception of the world formed by minds in the state of intellectual infancy. We can trace in the development of a child's mind the gradual substitution for this order of ideas, of the notion of a world governed by invariable laws. Now, the general mind of mankind has passed through the same stages which may be traced in the growth of the individual mind; and just as it would be impossible in the case of an individual to specify some particular moment of his life, at which it could be said that his earlier impressions of the world then gave place to others derived from experience and reflection, so it would be impossible—even if we possessed a record of all the thoughts of past ages—to specify that period in the history of mankind at which science had its origin. As far back as history carries us we find indications of the cultivation of certain studies, which were the forerunners of some of our sciences, even if these last retain little or nothing of the ancient lore. We know, for instance, that at a very remote period astronomy was diligently pursued by some among the Chinese, Indians, and other Eastern nations; and the Pyramids attest the geometrical knowledge possessed by the Egyptians at a far distant time. But we may here well dispense with any discussion of what is vaguely known concerning the scientific acquirements of various ancient nations, and commence our history at a period sufficiently remote, but with a people whose records enable us to follow with a certain amount of distinctness, the growth of scientific ideas, and from whom we may trace the progress of science in a direct line to our own times. We refer to that wonderful race whose antique relics afford us still our grandest models in poetry and arts—the ancient Greeks.

It must not be supposed that the scientific and philosophical doctrines which were inculcated by this or that sage, among the Greeks or other races, represent ideas which were generally accepted by men of his time or country, or were even known to them. The number of originating minds in any community is always very small, and though individuals of a given people contemporaneously exhibit every phase of intellectual development, the position from which the philosopher surveys the world is often one of solitary elevation; but it seldom happens that the people at large are ready to listen to his voice as to that of a prophet descended from a sacred mount. The utterances of the philosopher may even be direct contradictions of the accepted and cherished beliefs of his contemporaries: indeed, it is upon record that some of those who first among the Greeks sought to explain the phenomena of nature by physical causes, were in no small personal danger of suffering the terrible penalty attached to the conviction of impiety towards the gods. It was the action of these divinities which the ordinary Greek of Hesiod's and of Homer's time saw in every movement of nature, and to seek for physical causes where he clearly recognized the operation of personal wills would appear to him at once impious and absurd. Or, to borrow the expression of an acute writer, the question for the men of ancient Greece was not—*What are the causes* of rain, thunder, and earthquakes? but—*Who* rains and thunders? *who* shakes the earth? And they would be quite satisfied to be told that it was Zeus or Poseidon.

The writings of the earlier philosophers of Greece have not come down to us. It is only from the accounts of their doctrines given by later writers, or from criticisms on their works, that we are made imperfectly acquainted with their systems. The first name we find in the roll of Greek philosophers is that of Thales, who propounded certain doctrines displaying that search after generalization which has been mentioned as characteristic of science.

THALES (B.C. 640—548) was a native of Miletus, the chief city of Ionia, a flourishing colony planted by the Greeks on the shores of Asia Minor. The Ionians were more enterprising than the natives of the mother country, and by actively engaging in trade and navigation they became a wealthy people, while as yet Hellas proper remained comparatively poor. At the commencement of authentic Greek history, these Ionians were also noted for their intellectual culture and artistic taste. Their temples were celebrated for their size and splendour, and their cities vied with each other in the production of the most magnificent works of art. Of all these cities Miletus was the most prosperous and the most powerful; her ships were to be seen in every port of the Mediterranean and of the Euxine, and her citizens established themselves in numerous colonies on every shore. While the merchants of Ionia were making voyages to every part in pursuit of wealth, it was inevitable that the philosophers of Ionia, hearing vaguely of the inventions and learning of other lands, should also seek by visits to these to increase their store of knowledge. Thales was fortunate beyond his predecessors—for although he is called the originator of philosophy, it is probable that there were in Ionia seekers after knowledge even before him, *vixere fortes ante Agamemnona*—in having the opportunity of visiting the wonderful land of Egypt. It was only shortly before the time of Thales, or about B.C. 670, that Egypt was for the first time thrown open to the friendly access of the Greeks; for the country had for ages been previously closed against all foreigners, the Egyptians maintaining that policy of exclusion and isolation which until lately separated China and Japan from intercourse with the rest of the world.

Thales spent a portion of his life in Egypt, where he was initiated by the priests of Memphis and Thebes into the mysteries of their science. It would be useless to speculate upon the antiquity or extent of the scientific knowledge possessed by the inhabitants of the valley of the Nile. Egypt we know was an ancient land at the very dawn of our extant histories. The Great Pyramid may perhaps have been built more than a thousand years before Joseph was brought down to Egypt, and it is certain that at the epoch of its erection the starry heavens over it presented an aspect very different from the present one, for at that epoch the constellation of the *Southern Cross* shone in the skies of Northern Europe. That a high state of perfection in arts, the result of a long settled condition of civilization and prosperity, must have prevailed in Egypt at a very remote period is testified by buildings, statues, and other remains which yet exist, and by the accounts which historians have left us of the public works, systems of agriculture, canals, plans of irrigation, etc. These things imply a great knowledge of practical mechanics, of engineering, of the arts of computation, and of surveying land. It was doubtless to the practical necessity for methods of measuring land that the science of geometry owed its origin, as indeed its name implies. Be that as it may, it appears certain that at a very remote period the priests of Egypt were in possession of some knowledge of geometry, astronomy, and other sciences. It is also certain that Thales and others of the earlier Greek philosophers resorted to Egypt in order to acquire a knowledge of science.

The journey of Thales to Egypt marks an epoch in the progress of the development of scientific thought. We do not mean by the mere importation of scientific knowledge into Europe, but by showing us one of the great steps in the division of intellectual labour. The priests

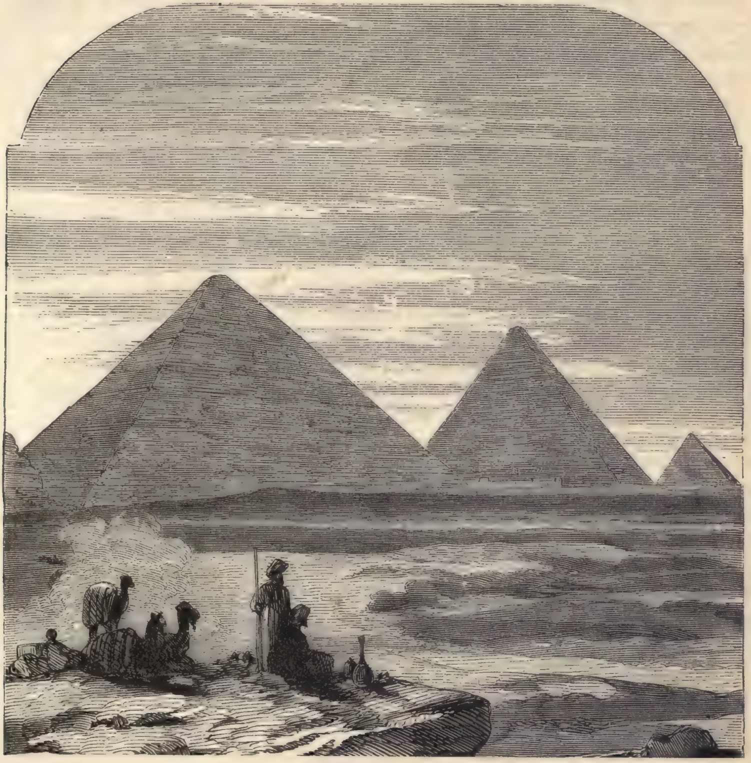

FIG. 2.—THE PYRAMID OF CHEOPS.

were in Egypt, as in all other countries before the appearance of the Greek philosophers, the sole depositaries of the existing scientific knowledge. Thus it has been in Chaldæa, in India, in China, in Japan, and among the ancient races of Mexico and Peru. Among the Greeks we first recognize the existence of a class of men who devoted themselves entirely to the cultivation of knowledge, while detaching themselves altogether from the sacerdotal character. These were the *philosophers*, or Grecian sages, whose lives, as free from political ambitions as they were from theocratic pretensions, were solely occupied in the endeavour to extend the domain of human intelligence.

Thales appears to have been somewhat advanced in years when he went to Egypt, yet, if we may believe the Greek historians, he made so rapid a progress in mathematics that his instructors were astonished by his acquirements. · It is expressly related that the Egyptians were particularly struck by his ascertaining the height of an obelisk or pyramid by merely measuring the length of its shadow. This may have been accomplished by Thales measuring the length of the shadow of the obelisk, at that particular instant of the day when the length of his own shadow was equal to his own height, and when, of course, the like equally holds with regard to the shadows of all vertical objects projected in a horizontal plane. Perhaps, however, he might make the determination at any time of the day, knowing that the shadows of different vertical objects have always the same proportions to the objects themselves. Simple as this matter now seems to us, it is the first suggestion we meet with of the problem of measuring inaccessible distances. Thales is also said to have been able to measure, by a like application of the proportionality of similar triangles, the distances of vessels from the shore. It is indeed probable that Thales would soon surpass his teachers, for the subsequent rapid progress of geometry in the hands of the Greeks proved the special aptitude of the Greek understanding for scientific deduction. When Thales returned to Ionia, he imparted to his countrymen such knowledge as he had acquired by his journeys, or had discovered by his own reflections, and thus he laid the foundation of true geometrical science. One of the most elegant of his own geometrical discoveries is that property of the circle by which two lines, one drawn from each extremity of the diameter, to meet at any point whatever in the circumference, always form a right angle with each other. (*See* Fig. 3.)

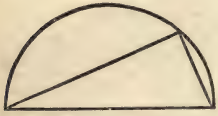

FIG. 3.

As Thales taught his countrymen the true foundation of geometry, so he appears also to have done them a like service in astronomy. He introduced some reforms into the Greek calendar, then in great confusion owing to very incorrect estimates of the length of the year. He also determined the periods at which the equinoxes and solstices occur. The equinoxes are, as everybody knows, the periods of the year at which the day is the same length as the night over all the world. The solstices are literally the periods at which the sun appears to stand still, in the following sense:—If the height of the sun be observed at noon every day, as for instance by marking the position on which the shadow of an obelisk's apex falls on a level plain, it will be seen that from midwinter to midsummer he daily attains a greater noon elevation. But about midsummer the increase of noon elevation ceases, and for several successive days the shadow will fall on the same place. This indicates the period of the summer solstice. After this, the height obtained by the sun diminishes each noon, until in midwinter another period occurs at which the noonday height remains stationary, and this is the winter solstice.

It is stated also that Thales was acquainted with the rotundity of the earth, and the true cause of lunar and solar eclipses. Nay, more, it appears that he actually predicted the occurrence of an eclipse, and that the prediction was fulfilled. The ancient historians assert that this eclipse interrupted a great battle which was commencing between the Medes and Persians, and from this circumstance it has been inferred that the eclipse in question must have taken place in the year B.C. 585. The truth of this ancient statement having been doubted, our present Astronomer-Royal, Sir George Airey, not long ago calculated back the dates of the solar eclipses which have occurred about the period in question, and he found that on the 28th of May, B.C. 585, there was a solar eclipse which was total for the region of Asia Minor. The calculation of the exact period of a solar eclipse requires the knowledge of a great number of facts which could not have been known to the Egyptians or to Thales. It is therefore surmised that Thales had in the East acquired a knowledge of a certain *cycle*, or long period, in which eclipses occur with a certain regularity; such a period was, it is said, known to the Chaldeans, among whom also Thales spent some time. By his knowledge of the cycle he may have been able to predict an eclipse, and he would no doubt have allowed some considerable limits between the dates within which the prediction might be fulfilled.

There is a fact which it is expressly stated Thales was acquainted with, and which for centuries remained an isolated fact, although it attracted some notice from the curious. It is the remarkable property of amber to attract little bodies after it has been rubbed with a cloth. This circumstance is one of the fundamental facts of that extensive branch of science we now call electricity, from the Greek name for amber (ελεκτρον).

But perhaps the most remarkable doctrine of Thales was one which displays in an extraordinary degree that seeking after generalization which has been already referred to as the great aim in science. Thales was the first on record who referred *all things* to a common origin, or supposed them to be produced from one material. He said, "*all things are produced from water.*" This appears to us at the present day a strange conclusion, for we are unacquainted with the facts upon which the philosopher based his opinion; and the sense intended to be attached to the words is by no means certain. Some moderns think that Thales is entitled to very little credit for this famous maxim, and they suggest that he acquired the notion in Egypt, from the common people, who, seeing that the water of the Nile made their soil fertile, and, in fact, furnished them with their means of subsistence, would attach the greatest importance to water. On the other hand, it has been supposed that Thales had observed that the earth itself showed signs of having been deposited from water, as we now know by the teachings of geology. It is of course quite possible that the philosopher may have acquired such geological truths as are here implied; but we are inclined to think that his maxim did not embody them, nor did it merely reflect the superstitious veneration of the Egyptians for the Nile. We regard the maxim as really a generalization of facts observed by the philosopher, but too hasty and too wide a generalization. We may take for granted that Thales could not have overlooked the fertilizing effects produced by the annual overflow of the Nile, and that in his native city of Miletus the sea would furnish the people with an unfailing supply of food. Thales, however, probably remarked that no seeds germinate in the absence of moisture, while on the other hand, plants grow without requiring any other visible material than water.* Again, Thales may have noticed that the bodies of animals are built up as it were in water, and are saturated with it. He recognized, it is said, the importance of water as an element of the human organization.† Thales went beyond such facts as these, and concluded that even heat was a product of humidity, for seeing that the sun and stars daily rose from the ocean, and with equal regularity sank into it again, he supposed that their fires were in some way nourished by the sea. The selection by Thales of water as the primordial element harmonizes curiously with the love of the old Greeks for the sea—that sentiment which prompted Xenophon's soldiers to raise a great shout when, after their long journey, they at last beheld the sea from the Thechian mountain, and embracing each other with tears in their eyes, they cried aloud, "θαλασσα! θαλασσα!" ("the sea! the sea!")

The disciples and successors of Thales sought, like him, the single or fundamental principle out of which, as they conceived, everything had been produced. This notion of a primordial principle distinguishes indeed the Ionian school of philosophers. Thus, while ANAXIMANDER (B.C. 611—545), the friend and pupil of Thales, continued to teach many of his master's doctrines, he regarded the fundamental principle out of which he supposed everything had been made, as something more subtile than water, though not so thin as air. To this philosopher the invention of the sundial has been attributed, but more probably he derived the knowledge of it from the Babylonians. It is impossible, amid the conflicting statements made by ancient writers, to determine always the original authors of scientific doctrines and inventions. In many cases, perhaps, the pupil was only promulgating the doctrines or making known the inventions of his master. Anaximander gave a very judicious answer to a question which seems to have occupied the thinkers of his time, who asked him how it happened that the earth was sustained in its position. He said that the earth being in the centre of the universe, there could be no cause to solicit its movement in one direction rather than another, for everything was uniformly disposed around. Anaximander thought that the sun was a ball of fire, and that its actual size was less than that of the earth.

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* To make this view clear to the young reader we need only remind him of the familiar expedient of growing hyacinths in glasses filled with water.

† Modern physiologists tell us that water constitutes six-sevenths of the weight of the human body.

ANAXIMANES (B.C. 557—504), who in his turn was successor to Anaximander in the school of Ionian philosophers, had his own solution of the problem regarding the origin of things. He said, "*All comes from air, and into air returns.*" Even water, which to Thales had appeared the primordial substance, was regarded by Anaximanes as originating in air, since it is in air that the clouds are formed, and these are the source of the rain which feeds the springs and rivers that supply the ocean itself. Anaximanes held that the air was a life-giving principle, and that even the soul of man was in some way derived from it. It is curious that these words may be interpreted as expressing the modern discovery of the invariable dependence of all animals upon the oxygen of the atmosphere; but it is certain that the old Greek could not have been possessed of a knowledge of such facts as are now so familiar to us. Yet it is hardly possible that this doctrine could have been a mere conjecture. Is it not more likely that some well-founded observation suggested the theory, which was at once carried to that extreme degree of generalization which we shall find to have been a special feature of scientific thought among the Greeks? Anaximanes supposed that the air extended to an infinite distance from the earth; and this idea was entertained for centuries after his time. It was a very natural one, for there is no limit to the atmosphere visible to the senses.

At first sight the doctrine of Anaximanes, which declares that *all things come from air, and return to air again*, appears a very extravagant assumption. But observe the words in which an eminent chemist of the present century sums up the facts of his science as relating to organized beings. "Plants and animals are derived from the atmosphere, and are but condensed air. They come from the air, and they return to it."—(*Dumas.*) These are almost the very words of the ancient philosopher! Has modern science, then, only come to confirm a truth guessed at four and twenty centuries ago? It is not possible that the ancient philosopher could have used the words with the full import which the modern chemist attached to them. The latter sums up in his statement the results of hundreds of refined experiments, which had been expressly contrived to elicit the truths of nature; but to the former such experimental demonstrations were quite unknown. We may well suppose, however, that the old Greek observed some facts which first suggested his theory, and this having been confirmed by a few further observations, he generalized it by applying it to all objects whatever.

Like the other Ionian philosophers, Anaximanes appears to have occupied himself with certain astronomical problems; but it is now impossible to determine the discoveries made in this department of science by the several individuals of the Ionian school, or how much of their science was derived from foreign sources. The adjustment of the calendar was a subject which received much attention from the Greek astronomers. The necessity of some arrangement by which the divisions of the year used in the civil reckoning of time should always correspond with the same seasons, will be obvious to every reader. The difficulty of making this adjustment is occasioned by the fact that the real year corresponds with no exact number of days, nor does the month, *i.e.*, the time required for the moon's revolution, form any aliquot part of the year. Suppose, then, that the year and the first month commence together at a given instant, the beginning of the next year cannot possibly coincide with the beginning of a lunar period. The Greek astronomers at length succeeded in finding a practically useful solution of the problem of arranging the calendar. It was discovered that two hundred and thirty-five lunar revolutions occupied almost exactly the time of nineteen solar years. Consequently every nineteen years the beginning of the year would coincide with the beginning of a month, and therefore the calendar could be arranged simply and conveniently for a cycle of nineteen years. This was known as the Cycle of Meton, and its first epoch, when it was definitely adopted by all the Grecian States, corresponds with the year B.C. 432. On this cycle our ordinary calendar is still based, as may easily be seen by any one who will refer to the tables usually prefixed to the Book of Common Prayer of the Church of England. He will there observe that in order to determine the dates of certain festivals which depend upon the lunar periods, it is necessary to ascertain the *Golden Number* for the year in question, the golden number being simply the ordinal position of the year in the Metonic cycle of nineteen years.

We pass on now to a philosopher of another school. His was a name greatly renowned in the ancient world, for he was the founder of the famous sect of the Pythagoreans.

PYTHAGORAS (B.C. 570—500) was born in Samos, an island belonging to the Ionians, and he appears to have visited every land where any information on things human or divine was to be obtained. In Egypt, where he lived for many years, he gained the confidence of the priests and acquired a knowledge of their geometry. The Chaldeans made him acquainted with their astronomy, the Phœnicians taught him arithmetic, and to the Indians he was perhaps indebted for his theology. After many years of absence he returned to Samos, only again to quit it for political reasons and settle at Cretona, a city founded by the Greeks in the most southern part of Italy. It was here that Pythagoras established the school of philosophy which was afterwards called the Pythagorean or Italian.

The scientific contemplation of nature appears to have interested Pythagoras chiefly as a basis for his moral and religious views. He also had his answer to the question concerning the universal principle of things, and his doctrine appears at first sight much stranger than those of his predecessors. For him, this universal principle was order, harmony, proportion. His disciples held that *number* was the essence of all things, and that its power pervaded all the works of art and of nature. We cannot suppose that they believed that numbers were the causes of things; the meaning probably was that all things must be considered under the condition of number, quantity, or proportion. Possibly, therefore, the Pythagorean doctrine contains a recognition of the necessity of studying the laws of nature as expressible by quantitative relations. The Pythagoreans, however, attached extraordinary importance to some numbers, merely as numbers. Thus, 10 was held to be above all others the perfect number, because it is formed by the addition of the first four numbers (that is, $1+2+3+4=10$), and probably also because it was connected with a mysterious symbol in the religion of the Chaldeans and Egyptians. Great significance was also attached to the number 3, which was called "the Number of the Universe," because everything, it was said, must have three parts, namely—the beginning, the middle, and the end. Reasons were found for also appropriating to the numbers 5, 7, and 9, certain symbolical or mystical meanings. It is curious to remark how general has been the tendency to attach significance to certain numbers, and the observant reader will not fail to notice the existence of this tendency even at the present day.

Pythagoras was perhaps the first of the Greeks who taught arithmetic and geometry on a scientific plan. He invented the multiplication table, and the abacus which is still used in schools to impart the elements of arithmetic. He is reputed to have been the first to introduce among the Greeks a system of exact weights and measures. Some important theorems of geometry are considered to be his discoveries, and among these is the famous proposition about the squares described on the sides of a right-angled triangle. As one of the most simple, elegant, and useful of geometrical propositions, we may express it here for the benefit of the non-mathematical reader, leaving him to test its truth.

Let A B C (Fig. 4) be any right-angled triangle, having its right angle at B. If squares be formed on each side of the triangle, it will be found that the area of the square on the side opposite the right angle is exactly equal to the sum of the areas of the other two squares. It was said that Pythagoras was so delighted by the discovery of this truth that he offered up a sacrifice;

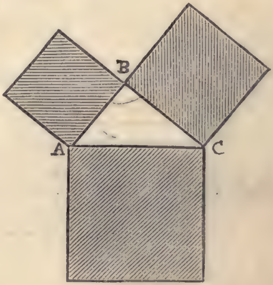

FIG. 4.

but this statement has been observed to be inconsistent with the philosopher's known objection to all shedding of blood, even on the altars of the gods.

There is a story of Pythagoras one day passing a smith's workshop, and noticing that the ringing sounds given out by two of the hammers as they struck the anvil formed certain musical harmonies with the note emitted by a third hammer. The story goes on to relate how this led him to think of the cause of those differences of pitch, and how he proceeded to make certain experiments on the subject. Although this story is open to doubt, it is tolerably certain that Pythagoras did actually make experiments on the relations between the several lengths of a stretched cord which would sound the notes of a musical scale. The story about the blacksmith appears to be wholly unnecessary, for so obvious a fact as that a stretched cord will give out a sound more and more acute when we shorten its length, leaving its tension unchanged, could not have escaped the notice of any one who thought about musical sounds at all. It was the scientific conception of *measuring* the lengths of the cord, which has made the name of Pythagoras memorable in connection with acoustics as the founder of the science. He discovered that when a stretched cord is stopped in the centre, each half will, when plucked, give out a note which is precisely the octave above that given out by the full length of the cord; and that two-thirds and three-fourths of the length of the cord correspond respectively to the musical intervals we call *the fifth* and *the fourth*. So that the proportions of the lengths of the cord which will sound a note, octave, fifth, and fourth, are severally 1, $\frac{1}{2}$, $\frac{2}{3}$, and $\frac{3}{4}$. The apparatus by which these relations are demonstrated in every course of lessons on acoustics is represented in Fig. 5, and as it consists essentially of a single stretched wire, it is called the *monochord*. The figure shows at B a hollow case or box, the top of which is made

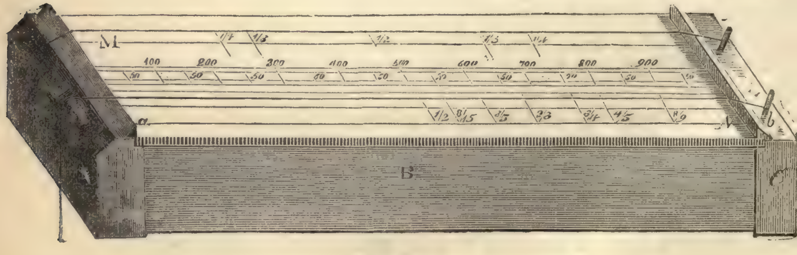

FIG. 5.

of thin deal, and forms a sounding-board. The wire M is stretched between two pegs, and below it is placed a stop or bridge, which can be moved to any required point under the wire, the lengths of the segments being shown by the figures marked on the sounding-board. The other wire represented in the apparatus is used for the purpose of showing the effects of different tensions in altering the note. This also appears to have been a subject of investigation by Pythagoras or some of his disciples. The experiments are performed by attaching known weights to the end A of the wire, and in this way it is found when the wire is stretched by a weight of 9 lbs., it will yield a note a *fifth above* that given out with a tension of 4 lbs.; and the octave is reached when the wire is stretched by a weight of 16 lbs. It will be noticed that these numbers are inversely as the squares of those expressing the relative *lengths* of wire for the same intervals.

The reader can hardly have failed to observe the circumstance of the relative lengths of the same cord which yield the harmonious intervals of *octave*, *fifth*, and *fourth* being expressed by the first four numbers, 1, 2, 3, and 4. Whatever mystical importance Pythagoras had before this discovery been accustomed to attach to numbers we may well suppose would be vastly increased when he found that music itself depended on numbers. The people of ancient Greece were perhaps more susceptible to the influences of music than the moderns, although it is capable of rousing even in these last the wildest enthusiasm. Music, it is said, produces states of feeling which are incapable of being awakened by words, and are inexpressible by them. By the very absence of distinct ideas or images, it seems the more directly to address the heart, lifting from it the burden of ordinary existence, so that the listener feels as if admitted into the innermost sanctuary of being, where life itself is ready to reveal its mystery. Can we, then, wonder that Pythagoras should have supposed that he had at length discovered the world-secret, and that he should have taught that harmony, proportion, number, ruled all things? Pythagoras applied this doctrine in his famous conception of the music of the spheres. His ideas on astronomical subjects appear in some respects to have embodied certain truths; for example, by his placing fire in the centre of the universe we may infer that he intended to represent the position of the sun. He conceived the planets to revolve around the central luminary, and he observed, as he thought, that their different velocities had the same proportions to each other as he had discovered in the musical intervals. But as these velocities were great, Pythagoras could not think of them otherwise than as necessarily accompanied by a sound—a mighty rushing noise. All things he had ever observed in rapid motion,—the horse, thundering with hoof of speed; the chariot, with flashing wheels raising the Olympian dust; the ship, with raging prow urged by a keel-compelling gale through the waters,—failed not to loudly impress the ear. But the vast velocities of the planets being exactly in the harmonic ratios, the result could not, he concluded, be noise, but music, and music excelling in sweetness as well as in power all earthly music. We may here parenthetically remark that these

Pythagorean notions were largely entertained by the philosophers of the Middle Ages, and that even at the present day there are not wanting persons who occupy their minds with speculations of this kind. The "music of the spheres," false as it is in science, is often appropriately enough alluded to by our poets. Thus Pope, in the "Essay on Man":

> If Nature thundered in his opening ears
> And stunned him with the music of the spheres,
> How would he wish that Heaven had left him still
> The whispering zephyr and the purling rill!

But as the music of the spheres, notwithstanding its sweetness and loudness, was inaudible to mankind, Pythagoras had to explain why it was unheard. His explanation was ingenious, and indeed it indicates a real truth as to the nature of our perceptions. The music of the spheres is unheard, said Pythagoras, because it is ever and unceasingly resounding in men's ears from the moment of their birth. It is certainly true that we recognize no condition of sensation except by the change from another condition, and that we could never have been conscious of sound and silence except by their contrast.

Pythagoras had many disciples, and it is impossible to distinguish the doctrines held by him from later developments of these doctrines by his followers. His predecessors had arrogated to themselves the title of wise men, but he first called himself a lover of wisdom (φιλο-σοφος), and thus we have the name of philo-sophy. Pythagoras never committed his doctrines to writings, and oral instruction became the tradition of the school. It was also part of the Pythagorean plan to veil the higher truths of their philosophy in mysterious language, which was intended to be understood only by those fully initiated. Their maxim was that not unto all should all things be taught, and therefore on many subjects they openly professed certain doctrines—the *exoteric*—while they secretly inculcated others—the *esoteric*. It is believed that among these last Pythagoras taught his disciples something very like the astronomy which places the sun in the centre of our system, and recognizes in the fixed stars suns which are the centres of other systems. Even that which filled the popular mind with terror—the comet, that

> from his horrid hair
> Shakes pestilence and war,

by the Pythagoreans could be contemplated calmly; for the philosopher of Cretona seems to have been aware that the comet was also a body revolving about the sun, like a planet visible in only a part of its orbit.

It would appear from the account given of the Pythagorean doctrines by Aristotle that these philosophers had a notion of both the diurnal and the annual revolutions of the earth, as well as of its rotundity. He tells us that in the centre of the universe the Pythagoreans placed fire, as the watch-tower or outwork of Zeus, and about this central fire they supposed that ten bodies revolved; namely, the earth; another earth (*αντιχθον*) opposite to this, making day and night; the sun which reflected the central fire; the moon; the five planets; and the starry sphere.

Some of the Pythagoreans appear to have clearly observed evidences of those changes on the surface of the earth which it belongs to the science of geology to consider. The Roman poet Ovid, who wrote many centuries after the time of Pythagoras, has introduced into the fifteenth book of his "Metamorphoses" a sketch of the doctrines of the Pythagoreans. Pythagoras himself is here represented as speaking in his own person, although it is evident that some of the illustrations put into his mouth refer to events that occurred after his age. We quote, from Addison's translation, a few lines describing geological phenomena:

> The face of places and their forms decay,
> And that is solid earth which once was sea;
> Seas in their turn retreating from the shore,
> Make solid land what ocean was before;
> And far from strands are shells of fishes found,
> And rusty anchors fixed on mountain ground.
> And what were fields before, now washed and worn
> By falling flood, from high to valleys turn,
> And crumbling still descend to level lands;
> And lakes and trembling bogs make barren sands.
> The parchèd desert floats in streams unknown,
> Wondering to drink of waters not her own.

He then gives examples of streams having changed their courses and their characters, naming among historical instances of sea changes:

> Antissa, Pharos, Tyre, in seas were pent,
> Once isles, but now increase the continent;
> While the Leucadian coast, main land before,
> By rushing seas is severed from the shore.
> So Zancle to the Italian earth was tied,
> And men once walked where ships at anchor ride,
> Till Neptune overlooked the narrow way,
> And in disdain poured in the conquering sea.
> Two cities that adorned the Achaian ground,
> Buris and Helice, no more are found;
> But, whelmed beneath a lake, are sunk and drowned,
> And boatmen through the crystal waters show
> To wondering passengers the walls below.

The reader will not fail to remark how distinctly the occurrence of fossil shells, etc., is pointed out in Ovid's lines. Such phenomena were, indeed, known to several of the early Greek philosophers, and were discussed by them. Thus, we know that the remains of marine animals embedded in rocks in inland situations attracted the attention of Xenophanes, who flourished about five centuries before the Christian era.

The name of LEUCIPPUS (*c.* B.C. 495—428 ?), of Abdera, an Ionian colony on the coast of Thrace, has an interest for us, as that of the founder of a philosophy which has provided the theoretical basis for all the physical science of the present day. Leucippus declined to admit that fire, air, earth, and water were the true elements; and he substituted the notion of all things being formed of particles so small, that they could individually be neither seen nor felt. These particles he conceived to be, nevertheless, of various forms and sizes; and he supposed that their different arrangements and movements gave rise to all the various properties of bodies. It would appear, therefore, that Leucippus regarded matter as consisting of particles of various kinds, and intervening spaces in which these particles moved in various ways.

These doctrines were greatly developed by DEMOCRITUS (B.C. 470—361), the pupil of Leucippus; he was also a native of Abdera, but, like Pythagoras, he extended his opportunities of acquiring knowledge by long residence in Persia, Syria, and Egypt. Though he left behind him many treatises, not one has survived; but the principles he taught have been transmitted to us in a few distinct propositions. One of these asserts that *nothing can be made from nothing*, and that all change consists only in the composition and separation of particles. This is identical with the modern principle of the indestructibility of matter. Democritus asserted also that there exists nothing but atoms and empty spaces, and that the atoms have no internal actions, but affect each other simply by impulse and pressure. At the present day the most general formulas by which we express the facts of nature involve the supposition of the existence and motions of atoms. Such are the theories of sound, heat, light, and chemistry. Strange that, three and twenty centuries ago, the fundamental theories of modern science should have in a manner been anticipated by the philosopher of Abdera! The reader may wonder how it happened that a theory which has proved so fruitful in the hands of the moderns did not lead in ancient times to results of corresponding importance. It should be observed that we are in possession of an immense store of facts of which the ancients had no conception, and that for many branches of science it is the power of our modern mathematical analysis which has given strength to the atomic theory. Besides, in the history of Grecian philosophy we observe that a remarkable change was some time afterwards given to the direction of men's thoughts, by a few master-minds who appeared among the Athenians.

The reader may remark, by what we have related of the speculations of the earlier Grecian philosophers, that one great question with them was concerning the *stuff* from which things are made. One thought this universal principle was water, another air, and so on, but all agreed that the key to all knowledge lay in its discovery. EMPEDOCLES of Agrigentum (B.C. 460) introduced a new idea into philosophy. He adopts the hypothesis of invariable and indestructible particles, but attributes a special action to fire or heat as the active principle. He affirmed also that matter was governed by two distinct *forces*, which respectively presided over combination and decomposition. These he named *love* and *hatred*, and they appear to be identical with our modern conceptions of *attractive* and *repulsive* forces. The introduction of this notion of *force*, or the prominence Empedocles gave to it, marks a new era in scientific speculation, for we have now the conception of something acting upon *matter*, and this conception has pervaded scientific thought more or less down to the present day. Not long ago each class of phenomena was supposed to be controlled by a special "*force*," the term used being not a mere figure of speech, but as the name of an agent controlling matter, which was thought of as something passive or inert, merely moved by "force." For instance, it was supposed the phenomena with which chemistry is conversant were explained by the supposition of the existence of a force called "*chemical affinity*" governing the combinations of matter. Later it was discovered that "chemical affinity" was, after all, nothing but an expression for a group of facts—a mere trick of words.

The notion of a world of "*brute matter*" pushed or pulled hither and thither by "forces," is derived from our own personal experiences. A stone lies on the ground, and continues motionless until we kick it, or push it, or lift it up. The effort we put forth in these actions is the foundation of our idea of force, and as in general the stone moves not without our efforts, we think of it as entirely passive. When, therefore, we see the stone dropped from a height moving rapidly to the ground, we say that a *force* is acting on the stone, we picture it to ourselves as pulled towards the earth by the "*force of gravitation*," and as being simply an inert mass upon which this force acts. But reflection shows us that in reality the matter in the stone is as active in producing the result as the matter of the earth, and that no ground whatever can be found for declaring that there exists, apart from the earth and the stone, *a tertium quid*, an agent called gravitation, acting upon the otherwise inert masses of stone and earth. In reality "gravitation," like the phrase "chemical affinity," is simply a sign we employ to express a certain class of phenomena. These remarks will perhaps serve to indicate our tendency to explain the phenomena which the world presents to us by notions derived from our own consciousness. There is in the philosophy of Empedocles a plain expression of this tendency, but in later philosophers it became the acknowledged foundation of their system. Indeed, the Grecian philosophy soon became so entirely *subjective* and concerned itself so little about the facts of the external or *object* world, that the study of these last came to be a distinct branch of intellectual pursuit, and ancient science and philosophy finally parted company. Until about the age of Plato, the philosopher and the scientific inquirer were so commonly united in the same individual, that it would not be easy to give an account of the development of scientific ideas without referring to doctrines which perhaps more strictly belong to the domain of philosophy. Besides, in all ages science and philosophy have reacted upon each other, so that it would be impossible to trace the history of either as an independent line of intellectual advancement.

The first philosopher who took a point of view entirely subjective was PROTAGORAS (*c.* B.C. 480—411), an Abderite, and a pupil of Democritus. His starting-point was not the material or object world, but the mental or subjective world. He regarded matter as something in itself quite indefinite, something which in its perpetual change and flow appeared now to be this, now to be that. He took no account of the *atoms* of his master. He declared that things are but what they appear, and that they have for us no other meaning. His great maxim was "man is the measure of all things." By this expression he gave prominence to the view that we can know of the external world nothing beyond its phenomena, and that it is impossible to arrive at any knowledge of things absolutely, or as they are in themselves.

We may here notice the opinions of the great moralist SOCRATES (B.C. 470—400) on physical science, to which in his younger days he had given some attention. He declared that it was impossible for men to arrive at any satisfactory knowledge of such things; for, said he, those who have devoted themselves to investigations of natural laws have come to the most contradictory conclusions. He held also that all such knowledge was vain and useless, for even if the natural philosophers should succeed in discovering the laws of natural phenomena, they would not be able at will to produce wind or rain or changes of the seasons. He considered, too, that such inquiries were not free from impiety towards the gods. Socrates made "man the measure of all things" in a sense different from that of Protagoras, for he believed that the motions of the sun, moon, and stars are arranged for the convenience of man, for whose benefit also animals, such as goats, sheep, horses, and oxen, are specially designed.

PLATO (B.C. 427—374), whose name is so illustrious in philosophy, has directly and indirectly largely influenced the course of intellectual development and scientific thought. Before Plato had become the disciple of Socrates, he had been a student of the philosophy of Heraclitus, one of whose prominent doctrines was that all things are in a state of ceaseless change, so that, for example, no one could ever be twice on the *same* river, inasmuch as the water is ever changing. About the age of twenty Plato became a disciple of Socrates, and continued so until the death of the latter, nine years afterwards. Plato then visited various countries, as Egypt, Persia, Sicily, and Italy. On returning to Athens he established his renowned school of philosophy amid the groves of Academus, near Athens; and this place has given a common title to schools of art, learning, and science throughout the world. Plato lived to an advanced age, and left behind him many writings, now esteemed amongst the most precious legacies that antiquity has bequeathed to us.

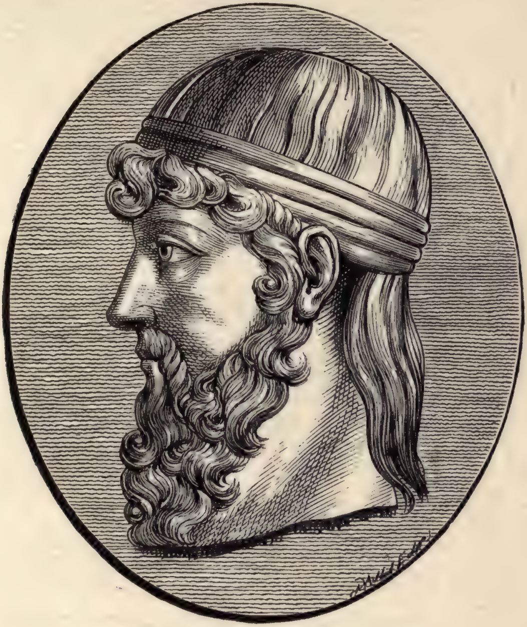

FIG. 6.—PLATO (*from an antique gem*).

It was the practice of Socrates to constantly seek for definitions of justice, beauty, and so on, and this of course implied that he thought that in some things at least there was something permanent. Plato managed in his famous doctrine of Ideas to reconcile and combine the conflicting views of Heraclitus and of Socrates. This doctrine gave rise afterwards to endless disputations, which for the most part diverted men's minds from the observation of nature. Although in the region of strict physical science the Platonic doctrines, with all their attractive brightness, would have proved guides as misleading as *ignes fatui* playing over a marsh, yet Plato's search after the One and the Eternal in the multiplicity and changefulness of things has not been without fruit in scientific conceptions. Perhaps the highly imaginative nature of Plato's philosophy appeals to some element in our mental constitution, which rigid science and the mere contemplation of phenomena fail to satisfy. Certain it is that the soaring aspiration which distinguishes the philosophy of the great Academic sage has, irrespective of creeds and systems, attracted to his pages the best minds of every subsequent generation of men.

Plato considered the objects perceived by the senses as mere changing and fleeting shadows of certain eternal and unchangeable realities, which possessed in a higher sphere of being an existence independent of the things of sense. These realities, which alone could be the objects of true knowledge, and could be taken cognizance of by the mind only, Plato called *The Ideas*. By ideas Plato meant not mere thoughts, or images of the things in the mind, but substantive existences that were in being before matter, or man, or the objects of sense came into existence, and that constitute the patterns or archetypes to which the sensible objects, more or less, conform. Things, according to Plato, are perceived to be what they are only by their participation in the *Ideas*, and it is only so far as they participate in these unchangeable realities that phenomena are contemplated by the philosopher. As an illustration of the manner in which this theory of Plato's reconciled the notions of change and permanence, consider the diversities presented by the individuals of the human race, in age, conformation, sex, and numberless other particulars. Yet in the midst of the endless diversity we recognize the possession of something in common, by which each individual appears to more or less agree with a certain model, which in ordinary language is sometimes called our *idea* of humanity. The word *idea*, as here used, would express simply the set of qualities which we consider as essentially human, or, expressed in words, it would be the *definition of humanity*. But Plato conceived of *ideas* as things existing absolutely, apart altogether from the world of phenomena.

In the dialogue of Plato's called the "Timæus" we have the earliest account of a physical theory of the universe, which has reached us in its author's own words. To the modern reader, whose view of nature is very different from that which presented itself to the minds of the older Greeks, the "Timæus" appears a confused and bewildering speculation. In this dialogue, theological, philosophical, mathematical, and physical considerations, which now are clearly separated and treated apart, run together without distinction. These speculations carry us back to a period at which the intellectual domain had not yet been parcelled out into distinct fields, or, to borrow a phrase from the language of the evolutionists, the now recognized classes of notions had not as yet become *differentiated*. It may be here remarked, in order to guard readers unacquainted with Plato's writings from obtaining a false impression concerning them, that Plato's philosophy does not turn upon physical science, to which, indeed, he attaches little importance, apparently regarding it rather as a recreation than a serious or profitable study.

Inquiries into facts of natural science presented no attractions for Plato, and he seems to have believed that no certain results were attainable. It is noticeable that he makes Timæus speak with a diffidence and hesitation, contrasting strongly with the confident manner in which the interlocutors in other dialogues expound such doctrines as those of the

"*Ideas*." At the outset of his discourse Timæus is made to speak thus: "If, then, Socrates, amid the many opinions about the gods and the generation of the universe, we are not able to give notions that are in every way exact and consistent with one another, do not wonder at that. If only we adduced possibilities as likely as any others, that ought to be enough for us, when we remember that I who am the speaker, and you who are the judges, are only mortal men, and we ought to accept the tale which is probable, and not inquire further."* He proceeds, however, with his subject by supposing that the Creator of the universe, desiring that all things should be good, made the world, or rather He set in order a world which He found in turmoil and disorder. He planted an intelligent soul in the world, which thus became a living thing like an animal. Between fire and earth, the necessary elements of a visible and tangible creation, God placed air and water as their bond of union, for these elements form a continuous proportion; that is, as fire is to air, so is air to water, and as air is to water, so is water to earth. He desired that the created living thing (*i.e.*, the universe) should be perfect and not liable to decay, and therefore He gave to it the most natural and suitable form, which is that of a sphere. This is the most perfect of all figures, everywhere similar to itself, and every way equidistant from the centre. There was no need of limbs or organs, for nothing existed outside of the universe, to be perceived, or approached, or seized. Hence the universe-animal was finished smooth and round on the outside, and the motion assigned to it was that appropriate to its shape, namely, a revolution upon itself.

Timæus then goes on to say that before God made the body of the universe He made its soul, from a compound of an unchangeable essence, and a corporeal one, with a third or intermediate essence. The creation of the sun, moon, and five planets had for its object the distinction of days and nights, months and years, or time divided by uniform motions according to number. To the moon was assigned an orbit nearest the earth. Next beyond that was the orbit of the sun, in which God had lighted a fire to illuminate the whole. The motions of the Morning Star, and of the star sacred to Hermes, are mentioned, but the movements of the other wandering stars are said to be numerous and intricate, so that their comparative length had not been estimated by number. The Creator formed within the universe four races: one of the gods, another of birds, a third of aquatic animals, a fourth of land animals. The divine race were, for the most part, made out of fire, and, after the likeness of the universe, in a circular form, and they were distributed over the heavens. The Creator then delegated to the gods He had made the task of forming mortals. But He Himself provided for these a divine and immortal part, for when the universe was formed He had assigned to each star a soul, placing them as in chariots. He showed them that each must be received into a body, and that the soul which had lived righteously during its appointed time would return to the habitation of his star. But if he failed in doing this, he would in a second generation pass into a woman, "and should he not cease from evil in that condition, he would be changed into some brute who resembled him in his evil ways, and would not cease from toils and transformations until he followed the original principle of sameness and likness within him, and overcame by the help of reason the latter accretions of turbulent and irrational elements, composed of fire and air, and water and earth, and returned to the form of his first and better nature."

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* The quotations in inverted commas are from Jowett's translation.

The divine offspring of the Creator, to whom He had committed the task of forming mortals, having received at His hands the immortal principle of the soul, constructed for it a mortal body, the materials of which they borrowed from the fire, air, earth, and water of the world. These they joined in not indissoluble union, for the elements were to be restored to their original forms. For the reception of the more divine and immortal principle they prepared the head, making its shape round in imitation of the spherical form of the universe. The rest of the body was formed to serve the head, to bear it aloft, and carry it honourably and safely about the earth. In the head they placed organs by which the soul might perceive. The gods formed within the body a soul of an inferior and mortal nature, and this they placed in the breast, divided from the habitation of the higher principle by the narrow isthmus of the neck. The dwelling-place of the inferior soul was itself partitioned by the midriff. The space between this and the neck was the seat of the emotional or irascible part of the mortal soul; while below the midriff was seated that part of the mortal soul which is concerned with the bodily appetites. This position the gods assigned to the lowest creature, in order that his habitation might be as far away as possible from the head, where, as in a council-chamber, the higher principle might, removed from all disturbance, consider calmly what was best for the whole. In order to awe and restrain the turbulence of this lower nature, the gods made the liver to dwell with it, and made this compact, bright, smooth, and bitter. The liver was made smooth and bright "in order that the power of thought, which originates in the mind, might be reflected as in a mirror, which receives and gives back images to the sight." The spleen is for the purpose of keeping the liver bright and clear; it acts like a sponge for cleaning the mirror. In the same strain, Plato points out the design of other parts of the body. The flesh, for instance, is merely a sort of padding, for it was intended, he says, as a protection against heat and cold and falls. He discusses the causes of diseases, which he attributes to a reversal of the natural order in which the structural elements of the body pass one into another. He supposes that sensations and movements are conveyed by the veins; the existence of nerves is unknown to him. It is, indeed, only of the most obvious anatomical facts that he takes any notice.

Plato and other ancient philosophers applied to physical questions the abstract relations of number and figure, in a manner very different from that of the modern mathematician. The properties of numbers had been demonstrated irrefragably, and had so possessed men's minds with the conviction of their absolute certainty, that numbers were even supposed to contain the secret of the universe. Thus we read in the "Timæus," that when the Creator had compounded the material of the universe, "He began to divide it in this wise:—first of all, He took away one part of the whole, and then He separated a second part, which was double the first, and then He took away a third part, which was half as much again as the second and three times as much again as the first, and then He took a fourth part, which was twice as much as the second, and a fifth part, which was three times as much as the third, and a sixth part, which was eight times as much as the first, and a seventh part, which was twenty-seven times the first." The proportions thus assigned (represented by the figures 1, 2, 3, 4, 9, 8, 27) were then filled up in two series by placing in the intervals certain intermediate terms which Plato indicates, pointing out the relations between the several terms, so that finally these two series emerge, viz.:—

$$1 : \tfrac{4}{3} : \tfrac{3}{2} : 2 : \tfrac{3}{8} : 3 : 4 : \tfrac{16}{3} : 6 : 8.$$

$$1 : \tfrac{3}{2} : 2 : 3 : \tfrac{9}{2} : 6 : 9 : \tfrac{27}{2} : 18 : 27.$$

These numbers probably represent musical scales, or have some connection with Pythagorean doctrines of harmonic relations which were current in Plato's time.

The elements are themselves formed of triangles, as four of the regular geometrical solids may be formed of certain combinations of triangles. To each element Plato assigns a geometrical solid: to earth, the cube; to fire, the pyramid; to air, the octahedron; to water, the icosahedron. The elements may be transformed into one another, as one of these figures may be changed into another. This susceptibility of transformation illustrates one characteristic of Plato's scheme,—a view of nature which is very noteworthy,—and that is, the conception of *unity* which underlies it. Another example would be his notion about living things. These he imagines are descended from man by a process of degradation from the higher forms into lower. The race of birds was created out of innocent and light-minded men; land animals were the habitations of those souls which had no philosophy and never looked up to heaven; while the fishes were formed from only the lowest and most ignorant, who were thus made to respire in water, instead of in the finer element, air.

These brief notes of Plato's speculation will serve to show that physical science, mathematics, physiology, psychology, philosophy, and theology had not as yet been considered as offering distinct fields for investigation. Perhaps the most noteworthy thing about the "Timæus" is the fact that in it we find for the first time described in the author's own words *a conception of the universe as a whole.* Until the idea of unity underlying the multifarious phenomena of the world had taken possession of men's minds, there was nothing to prompt observation or to direct research. That these earlier efforts to understand the world should have produced only erroneous schemes was inevitable. The real materials for the edifice of science were not yet at hand. They were, in fact, almost altogether unknown, reposing in secret stores, whence long and patient toil was required to draw them forth and to lay them upon sure and firm foundations. But it was essential to the work that the possibility of the structure should be foreseen, and that some previsional design should set forth the grandeur and beauty which such an edifice might assume. The design necessarily took the tone and colour of the materials which presented themselves to the eye and lay nearest to the hand. The scheme of science which Plato has left in the "Timæus" may be regarded as little more than such a sketch dashed off in a half-playful mood. Serious attempts to build, before the right materials had been obtained, were not wanting, as we shall see in the case of some doctrines of Plato's great disciple, who oftentimes used *à priori assumptions* in his foundations, and supported his superstructure with pillars and buttresses made of false or merely verbal analogies.

ARISTOTLE (B.C. 384—322) is the most famous representative of the science of the ancients. He was born at Stagyra, an unimportant town on the coast of Thrace. His father was a physician to the Macedonian King Philip II., a position which not only determined the direction of Aristotle's studies towards natural science, but led to his subsequent appointment as tutor to Alexander the Great. Aristotle, after his father's death, went to Athens, where he became one of the most diligent pupils of Plato. When Aristotle was forty-one years of age and Alexander a boy of thirteen, Philip committed the education of the young prince to the care of the philosopher. The pupil, who afterwards became so famous, received with deference the instructions of his preceptor, and retained great respect for his learning. Aristotle left many treatises, a considerable number of which are still extant.

Until the rise of modern science in the fifteenth century, Aristotle exercised an unbounded influence over the course of scientific thought, and his authority in all matters relating to the knowledge of nature was regarded as final. Aristotle has often been represented as the exact opposite of Plato in his method as well as in his matter. Thus in Raphael's great fresco painting in the Vatican, entitled "The School

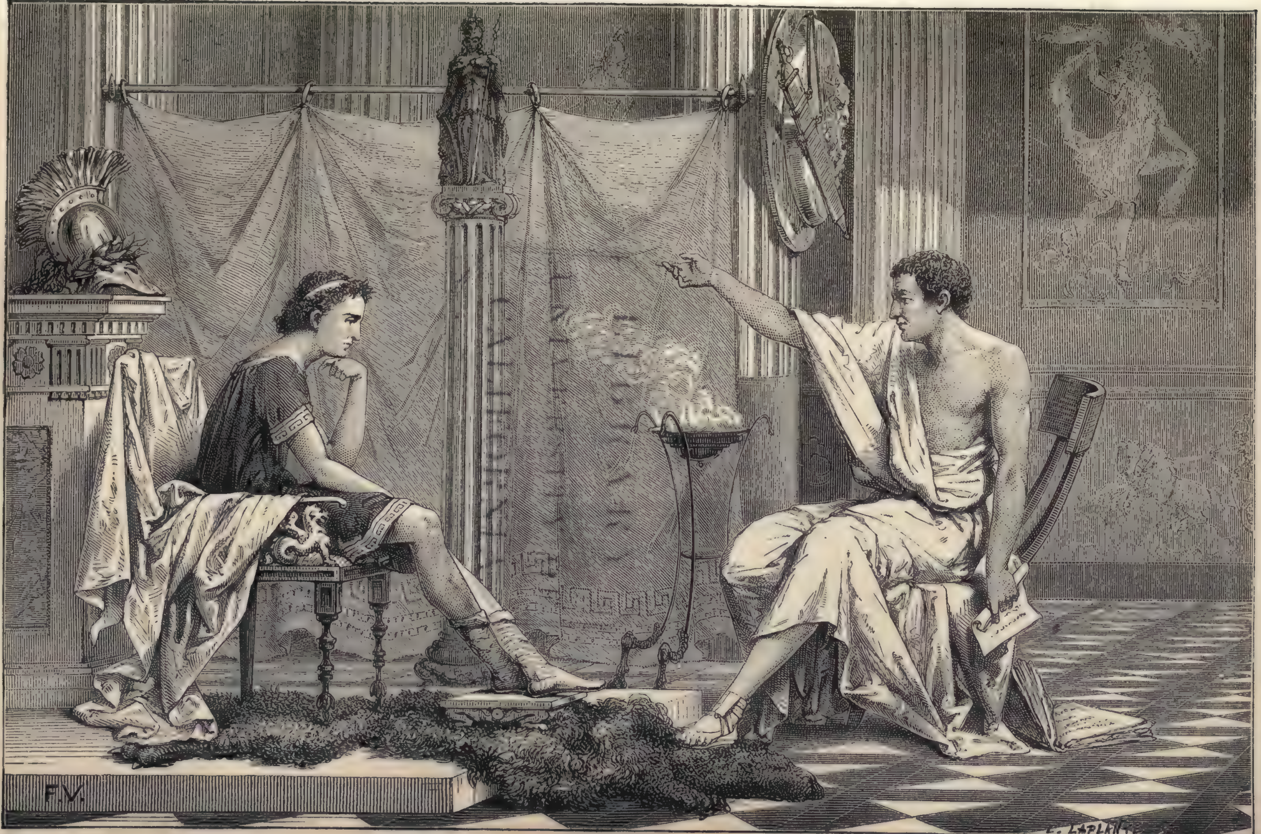
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PLATE II.—ARISTOTLE AND HIS PUPIL.

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of Athens," Plato is represented raising his hand towards heaven as the region of his *Ideas*, while Aristotle indicates his field of research by pointing to the earth. But, however great may have been the difference between the two philosophers, Aristotle appears to have adopted a system as inherently subjective as that of his predecessor. Thus, for example, he teaches that in order to know the nature of a thing we must first consider the *end* for which it is made. The results of his speculations and researches present a strange intermixture of truthful observations and unfounded opinions. How much some of the speculations of Aristotle on physical subjects consist of merely verbal distinctions and deductions from preconceived dogmas, may be seen from the following short *résumé* of some of his reasonings on astronomical subjects:

He begins by stating that as planes are measurable in *two* directions, and lines in only *one*, so bodies have magnitudes in *three* directions. Body is therefore made complete by *three* magnitudes, and three is specially the number of perfection, being used in the observances by which honour is paid to the gods. We speak also, says Aristotle, of *three* things as we do of all. (The allusion here seems to be to the separate grammatical forms used for the dual number by the Greeks, as their plural number does not, as with us, indicate two or more, but *three* or more persons or things. This last argument is an example of the assumption that the facts of nature must correspond with merely verbal distinctions.) As matter is then made complete by three, the same must be the case with motion; and, in fact, there can be but three simple motions—motion *towards* the centre, motion *from* the centre, and motion *round* the centre. The first two are rectilineal motions, and these two are motions natural to the four elements, since it is the nature of earth and water to move downwards towards the centre, and of fire and air to move upwards from the centre. The heavenly body which revolves in a circle cannot therefore be fire, as some have supposed; nor can it be any other of our four elements, since to these only the rectilineal motions to or from the centre are natural. By violence, indeed, a body may be moved contrary to its natural motion, as when a stone is thrown upwards; but such motions being contrary to nature speedily cease. It would, therefore, be unreasonable to suppose that the continuous and eternal circular motion of the heavenly body can be contrary to nature. And it would be inconsistent with Goodness that any being should be employed in constraining by violence the heavenly body to circular motion contrary to its own nature. The perpetual task of thus providing violent and ceaseless rotation would condemn the moving soul to pain and an unhappy fate. It must, therefore, be the nature of the heavenly body to revolve in a circle, and it was a mistake on the part of the ancients (it seems curious to find Aristotle speaking of the ancients) to think that the heavens must be supported by the feigned

Atlas. Empedocles also was wrong in supposing that the heavens are sustained by the extreme velocity of their rotation in the same way as water sustains itself in a vessel rapidly whirled round.

Aristotle's argument to prove that the heavens are incorruptible is conducted after a similar fashion, somewhat as follows. It was one of his doctrines that *the natural motions of contraries are contrary.* Thus, of the pair of opposites earth and fire, the natural motion of the former is downward towards the centre, that of the latter upwards from the centre. He urges that to motion *round* a centre there can be no opposite motion, inasmuch as there being but three simple motions, viz., towards a centre, from a centre, and round a centre, and the first and second modes of motion forming a pair of contraries, there remains no kind of motion to which the circular motion can be opposed, for each thing can have but one opposite. He meets by some curious reasoning the anticipated objection that circular motion in one direction may have its opposite in circular motion in the other direction. For example, he says, the purpose of circular motion is to convey a thing from one part of a circle to another part, and whatever thus moves necessarily comes uniformly round to each part in succession; the circular motions by which a body may be carried from the point A to the point B are infinite in number, since an infinite number of circles may be drawn so as to pass through the fixed points A and B; and in a circle there are no contraries of place, as above or below, before or behind, right or left, on which opposition of direction can be founded. Circular motion being, therefore, without a contrary, so the heavenly body to whose nature this kind of motion is conformable can have no contrary; and, having no contrary, the celestial body must be indestructible, since destruction occurs by a body changing into its opposite. And for the same reason it is not possible for the celestial body to have been produced or generated, since in production a body arises from its opposite.

Aristotle advances similar arguments to prove that there can be only one sphere of the heavens, and that no body beyond can exist outside of that sphere. He has also long metaphysical discussions about the Finite and the Infinite, etc. Sometimes, but rarely, he cites in support of his theories a fact which he considers analogous. Thus, to illustrate the assumption that light and heat arise from the stars, by reason of the friction against the air which their rapid motion produces, he says that it is the nature of motion to set on fire pieces of wood, stone, and iron, and that arrows become heated by their rapid motion through the air.

That the earth can itself revolve, either in the centre of the world or about the centre, appears to Aristotle impossible, because such a motion would necessarily be violent—that is, against the nature of the earth. "We know," he says, "that it is the nature of portions of the earth to move in straight lines" (he refers here to falling bodies), "and that the motion which naturally belongs to the whole belongs to each of the parts. But the order of the world is eternal, and therefore no violent or contra-natural motion can have a place in this order." Though Aristotle thus denies motion to the earth, he contends that its form is spherical, and in the proof he advances an argument still used, namely, the figure of the shadow cast on the moon in her eclipse. He also asserts that bodies do not fall towards the earth in lines which are parallel at different places. That the magnitude of the earth is not great he proves by the change in the appearance of the starry sky when the observer changes his position; for when we go to the north, stars which are visible in Egypt cease to appear, and others, which in Egypt rise and set, remain always above the horizon. The circumference of the earth, he says, had been calculated by some geometers, who had estimated it to be 400,000 stadia.

Among the writings of Aristotle is a treatise on Mechanical Problems, and his treatment of a very elementary question will at once illustrate his system, and exhibit the condition of the science in his time. The problem was to give a reason why a lever or balance with arms of unequal length keeps unequal weights in equilibrium. Aristotle states that the explanation is to be found in the properties of the circle, and of these properties he proceeds to give an enumeration, remarking finally that it could not be surprising that a figure which possessed so many wonderful properties should have also another one in the balancing of unequal weights. This solution has been admired and developed by Aristotelean philosophers, in spite of its obvious absurdity. Aristotle's treatise is, however, not without some indications of correct notions concerning forces, and in an elementary case the composition of velocities is discussed; but the reasoning by which the propositions are supported is hardly more satisfactory than that of which specimens have already been given.

The best work of Aristotle relating to natural science is, perhaps, his treatise on animals. As he is the first known author of any attempt at a scientific description and classification of animals, he may be considered as the originator of zoology. The treatise just mentioned includes a great number of statements which show indeed that Aristotle, or others for him, had no lack of ability in observing facts. So many truths regarding the anatomy and dispositions of animals are set down, that some modern authors suppose that the ancient sage must certainly have himself examined and dissected animals of every kind. There is a story, too, about Alexander the Great causing strange animals to be collected in Asia, and sent to Greece for the inspection of Aristotle. Aristotle divides animals into viviparous and oviparous, and under the first he ranks all quadrupeds; the second comprehending birds, fishes, and insects. It is now well known that such a division is defective, some quadrupeds (*e.g.*, lizards) being oviparous, and some fishes viviparous.

Among the great number of true observations, however, which Aristotle's treatise contains, there are no inconsiderable number of particulars so entirely false, that they are inconsistent altogether with any possible derivation from actual observation. Thus, it gravely stated that the lion has but one vertebra in its neck, that the river-crocodile moves its upper jaw, and that certain conformations of the hand indicate certain dispositions, etc. We are also informed that the breath enters the heart, that this organ has three cavities, that the back part of the head is empty, that men have eight ribs, and that men have more teeth than women. There are many other particulars, equally removed from truth, which are set down without the least indication that they are not facts which the author has himself observed.

A very eminent English physiologist (Professor Huxley) has quite recently propounded a theory which happily explains the occurrence of such astounding errors in a work displaying an amount of otherwise accurate observation that is truly marvellous, considering the age in which it was written. Professor Huxley supposes that the treatise on animals of Aristotle, as we now possess it, is nothing more or less than a collection of notes of Aristotle's lectures, taken by one or more of his students. The Professor cites his own experience and that of other teachers as showing that in students' notes it is not unusual to meet with the most extravagantly erroneous statements even in the midst of notes displaying otherwise an extraordinary degree of accuracy. Moreover, after discussing in detail all the statements concerning the heart in Aristotle's treatise, Huxley concludes that what have been pointed out by all the Stagyrite's commentators as glaring errors are, by a very slight change of the commonly accepted sense of his words, capable of being interpreted as exact statements of facts. Thus, by considering one of the cavities of the heart as simply a dilation of one of the great vessels—a view for which the appearances afford a certain justification—Aristotle's account would become, as far as the science of his epoch extended, substantially correct.

Two other men with names greatly celebrated among the ancients may be referred to here, as representatives of what may be termed the Natural History group of sciences. One of them was a contemporary of Plato, the other was a pupil of Aristotle. The first is the famous physician HIPPOCRATES (B.C. 470—375), to whom is attributed the foundation of medicine as a *science*. The healing of wounds and the cure of diseases is an *art*, and as such must have been practised in some form at a period coeval with the existence of mankind. The successful practice of this art depends largely upon a knowledge of the causes, symptoms, and course of diseases, and upon a knowledge of the anatomy and physiology of the human body. This kind of knowledge constitutes but a part of that division of science which occupies itself with the study of living things, and therefore it is only as in subordinate relation to zoology generally that the special medical sciences can claim our attention in these pages. Hippocrates deserves notice as having been the first who attempted to base the practice of his art upon rational and scientific principles. Before his time diseases were ascribed to the direct agency of offended deities, and the chief remedial measures were sacrifice and prayer to the gods. Hipprocrates was born in the little island of Cos, where his ancestors had practised medicine for generations, as priests of the temple of Æsculapius. But Hippocrates threw aside all traditional mystery, and sought from philosophy and from accurate observation reasonable grounds for his practice. He declared openly the measures he adopted in the treatment of diseases, frankly acknowledging his failures, and he willingly taught his art to all who chose to become his pupils. His descriptions of the symptoms and courses of diseases are wonderfully accurate; but the progress of science has for the most part superseded his theories. His doctrines ruled the practice of medicine for ages; and his ideas of the causes of disease are still so popularly current everywhere, that in the language, and sometimes in the practice, of our surgeons and apothecaries we can trace the

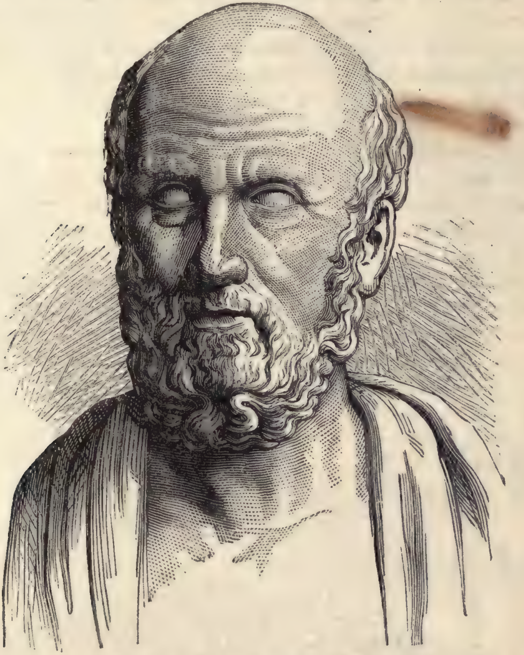

FIG. 7.—HIPPOCRATES OF COS.

influence of the famous theory of "the Humours." For, according to Hippocrates, the human body contained four humours; namely,—blood, phlegm, yellow bile, and black bile. Disease was occasioned by the undue accumulation of some one of these humours in particular organs, and it was the office of the physician to get rid of the peccant humours by various evacuant remedies, such as bleeding, purging, sinapisms, blisters, diaphoretics, etc., etc.

Hippocrates the physician must not be confounded with another Hippocrates, who as a *mathematician* had also a very high reputation among the ancients. To this mathematician we shall refer again in the next chapter, although he really belongs to the earlier period of Greek science.

Two treatises by a pupil of Aristotle's named THEOPHRASTUS (*c.* B.C. 370), have come down to us almost entire, and they constitute the earliest known writings on botanical science. In these works, about five hundred species of plants are referred to, and descriptions are given, but these are of so imperfect a character that many of the species described cannot now be identified with certainty. To Theophrastus the world is indebted for the care of the works of his great master, who, when dying, left them in his charge. Theophrastus is said to have lived to his hundred and seventh year, and to have died lamenting the shortness of life.

THE PARTHENON.

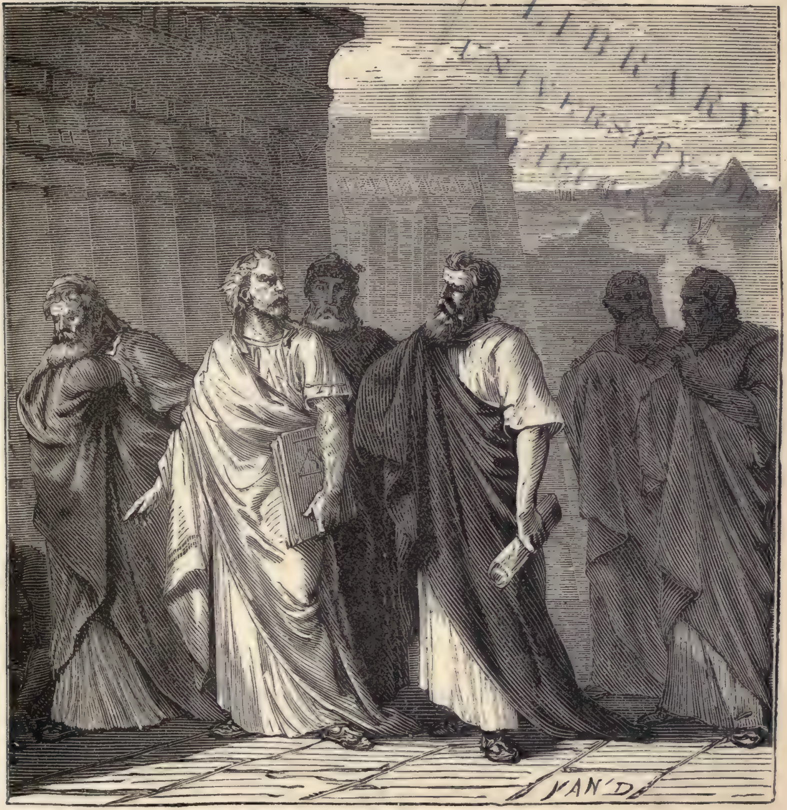

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FIG. 8.—ALEXANDRIAN MATHEMATICIANS.

# CHAPTER II.

## ALEXANDRIAN SCIENCE.

THE scientific views which have been mentioned in the preceding pages were in many cases portions of all-comprehensive systems of speculative philosophy, rather than results of independent study of the phenomena of nature. But a new era in the development of the Greek intellect was marked by the adoption of a method of attacking the problems of nature, that henceforth proved the true and fruitful method of natural science. It was a method in which observation, experimental investigation, and a careful sifting of facts, took the place of deductions from assumed principles, and of theories based upon foregone conclusions or false analogies. Its adoption immediately led to results which have proved of permanent value. The astronomy, the mathematics, the physics, the medicine, the theology, the scholarship of the moderns are all under obligation to the labours of the celebrated school of philosophers with which the reader is about to make acquaintance. Coincident with the commencement of this new era in the history of science, the geographical locality of its chief home changes, and our scene shifts from Athens to Alexandria. This city, which perpetuates the name of the great conqueror, was founded by him about three hundred and thirty years before the Christian era, and was intended to become a vast centre for trade between the East and the West. Alexandria did, in fact, enjoy a long career of commercial prosperity, and soon became a metropolis famous for its magnificence, but most of all for its intellectual glories.

Perhaps the influence of Aristotle may be traced in the personal interest which Alexander took in scientific studies; and this taste appears to have been transmitted to his great captain and successor, PTOLEMY LAGUS. At the division of Alexander's empire, Egypt fell to the share of Ptolemy, and this prince resolved to make Alexandria not only the greatest commercial *entrepôt* in the world, but also the great centre of science and learning. This last ambition descended to the successors of Ptolemy Lagus, and in order to realize it, they collected the famous Library, which contained, it was said, all the writings of the world; and they established and maintained that remarkable institution which was called the Museum. The reader must not be misled by the sense we now commonly attach to the word museum into supposing that the Museum of Alexandria was a repository of curiosities. It was a portion of the king's palace appropriated to men of learning, who were there maintained by the royal liberality, and provided with all the appliances for facilitating their studies. The vast libraries, containing upwards of 700,000 volumes, were close at hand. There was a botanical garden for the phytologists, a menagerie for the zoologists, a dissecting-room for the anatomists. Here the astronomers were supplied with every instrument known to their science—armillary spheres, astrolabes, mural quadrants, dioptras. Here poets, grammarians, historians, astronomers, mathematicians, engineers, chemists, physicians, theologians, magicians, and astrologers dwelt under one roof and fed at one table. Sometimes the monarch himself would preside at their repasts. Verily, these were golden days for men of learning! To Alexandria, as to a centre, were attracted the studious of every nation, of whom there were, it is said, at one time, no fewer than 14,000 gathered together.

Under the protection of the first Ptolemy (surnamed Lagus, and afterwards Sotor) and his successors, there flourished at Alexandria several distinguished men, any one of whom would have sufficed to make the place notable in the annals of science. No name belonging to the Alexandrian school is more widely and honourably known than that of the mathematician EUCLID (B.C. 323—283). In his well-known "Elements of Geometry" he collected the truths of elementary geometry which were known in his time, and his work became everywhere among the Greeks the text-book of the science. Its use as such has been continued down to the present day, when it still retains its pre-eminence as the model of exact reasoning and clear demonstration. Its propositions are so arranged that each one is related to those that precede it and to those that follow, forming a necessary link in a long chain of demonstrations. Some moderns have supposed that by certain alterations they could improve upon the arrangement of Euclid, but no attempt of this kind has yet been acknowledged as successful. The superiority in philosophical system of Euclid's train of demonstration is admitted even by those who have proposed to initiate youth into the science of geometry by some method less severe than the inflexible logic of the Alexandrian mathematician. The work of Euclid is so immediately accessible to every reader that it would be quite superfluous to even mention the subjects of which it treats. But if the reader should perchance be unacquainted with the Euclidean geometry, we would recommend him to turn to the forty-seventh proposition of the first book of Euclid's "Elements," and observe the specimen of geometrical demonstration employed in proof of that theorem, in which he will recognize the discovery of Pythagoras already mentioned on page 13. The elegance of the demonstration will be understood only by such as have made sufficient progress in the science to be able to reason out the conclusion for themselves from the data which are supplied in the definitions and axioms with which Euclid sets out. But any one may directly by practical methods convince himself of the truth so irrefragably established by the elaborate chain of deductive reasonings. He need only construct the triangle and squares on a sheet of paper, and he may then easily discover by actual superposition that the equality of the areas is invariably true. Each village bricklayer is almost daily taking practical advantage of the truth which Pythagoras discovered and Euclid demonstrated. When required to set out the rectangular sides of a building, he will tie to a peg one cord of three yards length, and another of four yards in length, and when he has stretched these so that their other extremities are exactly five yards distant from each other, he will be confident that he has a right angle between the cords. The connection between the theorem of Pythagoras and the practice of the mechanic will not fail to manifest itself to the intelligent reader.

Euclid's geometry enables us to compare the areas of any two plane figures bounded by straight lines. He compares also curvilinear figures with each other in certain cases. The problem of comparing the areas of curvilinear figures with those of rectilinear figures is a much more difficult one, and one case of it has attracted more general attention than perhaps any other mathematical problem that could be named.

It is this. Given a circle, to find what is the length of the side of a *square* which has precisely the same area as the circle? This is the celebrated problem of the Squaring of the Circle, which has in all ages had a strange fascination even for those little versed in mathematics. Investigators who never solved a strictly geometrical problem in their lives have by scores devoted themselves to the squaring of the circle with an ardour almost akin to insanity, and at the present day this race is perhaps more numerous than ever. The problem would be solved by the determination of the ratio between the circumference of a circle and its diameter, if any number expressed exactly how many times the circumference of a circle is longer than its diameter. For all practical purposes, this number has been estimated with sufficient accuracy ages ago; but this ratio, like many others, cannot be expressed with absolute exactness, because the diameter and circumference belong, as we have every reason to believe, to the class of magnitudes called incommensurable, that is, no numbers whatever will *exactly* express their ratio. The diagonal and side of a square are in the same predicament, as are many other lines, etc., in geometrical constructions. One section of Euclid's "Elements" (Book X.) is devoted to a discussion of the doctrine of incommensurable quantities in general. The attention which the problem of the quadrature of the circle has attracted, and the discussions to which it has given rise, have caused certain mathematical principles to be subjected to the most searching examination by the true cultivators of the science, and thus the futile problem has done as good service for mathematics as the Philosopher's Stone did for chemistry. But the strange and ungeometrical modes of solutions which persons mistaking the real conditions of the problem have proposed would form a curious chapter. Such expedients as rolling a wheel along a plane and measuring the length passed over in one revolution, cutting out circles and squares from thin plates and weighing them, have been often resorted to by those of a somewhat mechanical turn; while another tribe of circle-squarers invest the problem with mystical significance, and contrive to read its solution in the stars, or deduce it from "the number of the beast" in the Revelation!

One of the strictly geometrical methods by which the quadrature of the circle has been vainly attempted, may be illustrated by the theorem which made the name of HIPPROCRATES of *Chios* famous among the ancients, because his discovery was the first instance in which the area of curvilinear figures was exactly determined. This Hippocrates, who flourished about the fifth century B.C., is the *mathematician* referred to at the close of the last chapter. He was originally, it is said, a merchant, but having by chance been present at a philosophical lecture at Athens, he was so charmed with some geometrical demonstrations he then heard, that he forthwith renounced his mercantile pursuits in order to devote himself to the study of mathematics. The theorem of the *Lunulæ* (or *Lunes*) *of Hippocrates* is an elegant demonstration of the strict equality of the area of two crescent-shaped figures to that of a triangle, and it is so simple that even the unmathematical reader who has noticed the theorem of Pythagoras (p. 13) and that of Thales (p. 8), mentioned in the preceding chapter, will have no difficulty in following the demonstration. He must first preceive that if circles were inscribed in each of the squares in Fig. 4, the circle in the square opposite to the right angle would have an area equal to those of the other two circles put together also that the like would be true of the halves of the semicircles. In Fig. 10, A F B G C is the semicircle on A C; A D B is that on A B; B E C is that on B C; and the area of the first semicircle is equal to that of the second and third together. If from these equals we take away the common parts (denoted by the shaded spaces), the remainders will be equal; thus the areas of the *lunulæ* A F B D, B G C E, will together equal that of the triangle A B C.

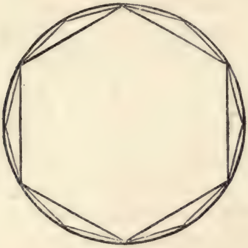

FIG. 9.

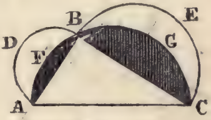

FIG 10.

Euclid extended the resources of geometrical reasoning by his adoption of the method of *exhaustions* as it was termed, which is only an application of the general principle of *limits*. The geometrical meaning of this term may be easily understood. If a regular hexagon (Fig. 9) be inscribed in a circle, it is obvious that the area of the hexagon is less than that of the circle, and also that the circumference of the circle has a greater length than the sum of the sides of the polygon. Now, suppose that by drawing straight lines between the extremities of each side of the hexagon, and a point in the circumference half-way between them, we inscribe a twelve-sided figure in the circle, it will be equally obvious that the area and perimeter of this figure will be greater than those of the hexagon, but yet less than those of the circle. By doubling the number of sides in the polygon, we may approximate yet more closely to the circle, and by sufficiently increasing that number we may arrive at a rectilinear figure as nearly coinciding with the circle as we please. The circle is therefore said to be the *limit* of the inscribed polygon; that is, the boundary to which the latter continually approaches by increase in the number of its sides, without the possibility of its absolutely reaching it. It is by the adoption of this principle that Euclid is able to prove that the areas of circles are to each other as the squares of their respective diameters.

Aristarchus of Samos (*c.* third century B.C.) is famous for his efforts in behalf of the Pythagorean doctrine of the earth's motion. He taught that the sun was stationary, and that the earth revolved about that luminary. He even explained away the objection which would be brought forward against this doctrine, namely, that if the earth moved as he alleged, the stars ought to present varying aspects: he said that the reason this change in the apparent position of the stars did not take place was because they were at so great a distance that the earth's orbit was a mere point in comparison. A contemporary of the astronomer raised another kind of objection to the doctrine of the earth's motion, by declaring that its author was leaving no space for the repose of the gods; but perhaps this objection was not urged in seriousness. Had it been so, Aristarchus might have incurred no small risk in earning the title of the Greek Galileo.

Aristarchus hit upon an ingenious method of forming an estimate of the relative distances of the sun and moon from the earth. He knew that the moon is a spherical opaque body illuminated by the sun. He reasoned that when the moon is seen exactly divided into a dark and a light semicircle, the straight line joining the moon and the sun must be at right angles to the line between the moon and the observer. Thus, in Fig. 11, let the observer's station on the earth be represented by E; when he sees the illumined side of the moon at M as an exact half-circle, it is obvious that the line S M, between the sun and moon, will be at right angles to the line E M. Now, it frequently happens that the half-moon and the sun are simultaneously visible, and it is only necessary to measure the angle between them in order to find the angle formed by the lines M E, E S. When this has been ascertained, a triangle constructed on a line M E, with an angle at E equal to the observed angle and with a right angle at M, would show the relative distances from the earth to the sun and moon by the lengths of the lines E S and E M. Aristarchus found that the angle M E S was not less than 87°, and that therefore the sun was at eighteen or twenty times the distance of the moon; and although this is vastly below the reality, the estimate of Aristarchus greatly extended the limits of the universe as they were accepted in his time.

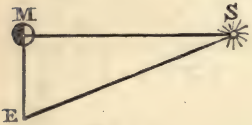

FIG. 11.

Another Alexandrian astronomer, Eratosthenes (B.C. 270—196), attempted an exact measure of the magnitude of the earth, and the method he adopted was in principle the same as astronomers have used ever since. He had observed that at his native city of Syene, in Southern Egypt, vertical bodies at the time of the summer solstice cast at noon no shadows, or, in other words, the sun was at this time exactly overhead in Syene. Now, Eratosthenes measured at Alexandria the length of the shadow cast by the gnomon at midday on the summer solstice, that is, at the very moment when he supposed the sun to be vertical at Syene. The annexed diagram, Fig. 12, will serve to explain how the magnitude of the earth was inferred from the observation. Let the circle A S D represent the earth, C its centre, S the vertical gnomon at Syene, A that at Alexandria, the dotted lines representing the direction of the sun's rays; and these are practically parallel, because the distance of the sun is immensely great in comparison with the distance between them. Now, from the length of the shadow A *c* of the gnomon, the angle A *b c* is determined, that is, the angular distance of the sun from the zenith or point of the heavens vertically over Alexandria. It will be evident to the merest tyro in geometry that this angle is equal to that between the lines S C and A C, subtended by the arc A S. The angle was found by Eratosthenes to be included by one-fiftieth part of the whole circumference. The whole circumference of the earth must therefore be fifty times the distance between Alexandria and Syene, which Eratosthenes estimated at 5,000 stadia, and he thus found for the earth's circumference 250,000 stadia, or about 31,250 miles, an estimate not very greatly wide of the truth, although, as we now know, there were several sources of error in the data. Syene, for instance, is not on the meridian of Alexandria, as Eratosthenes supposed, but 30′ to the east of it. Eratosthenes also made a very good determination of the obliquity of the ecliptic by observing the altitude of the midday sun at the summer and at the winter solstice, half the difference between these angles being the obliquity, that is, the inclination of the earth's plane of axial rotation to the plane of its orbit.

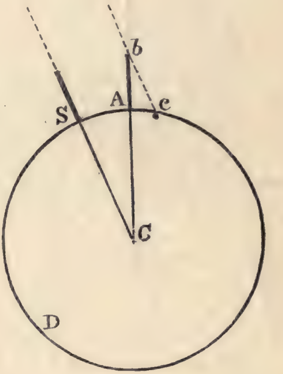

FIG. 12.

Another great name in the list of Alexandrian men of science is that of ARCHIMEDES (B.C. 287—212). His birthplace was Syracuse, but he came as a young man to the capital of the Ptolemies, and studied mathematics under the pupils of Euclid of Alexandria, for by that time the great master had himself passed away. Archimedes returned to his native city, where he soon had opportunities of applying his science in practice. Indeed, it is chiefly by the great services which in various ways he rendered to Hiero, the King of Syracuse, that his name has become so celebrated. There are writings of Archimedes still extant, which show that he must also be considered one of the greatest mathematicians of ancient times; and of certain branches of physical science Archimedes had a real experimental knowledge, so that some fundamental principles which he first announced remain as important truths to this day. But that which is most remarkable in the achievements of Archimedes is the notion of *applying science.* The ancient geometricians considered the application of their science to the purpose of practical life a degradation to its dignity, and a surrender of its proud claims to ideal pre-eminence. It was therefore a new departure for human progress when the lofty abstractions of geometry came to aid the mechanical arts of every-day life. In the present day applications of scientific theory have come to be so much regarded as matters of course, that we forget that it was only by one of those great leaps of thought, so characteristic of genius, that two provinces of human activity which at one period seemed so remote and distinct as abstract science and practical life, were brought first into contact and into relations of mutual helpfulness.

We read of Archimedes taking part in designing a huge ship, in which he introduced the then novel plan of erecting three lofty masts. It is related that at the launching of this ship Archimedes displayed the power of the lever by using it for urging the ship off the stocks, and that in reply to King Hiero's expression of wonder at the great force thus excited, Archimedes uttered his famous boast, "Give me but a place to stand on, and I could thus move the earth." Archimedes is considered the first who established statics and dynamics on true principles. His statics rests on the notion of the *centre of gravity*, of which he is the author. By its help, and that of certain incontestable truths, he demonstrates the principle of the reciprocity of the weights and distances from the fulcrum of the lever and balance with unequal arms. He also determines the position of the centre of gravity in various figures, and his investigation of that of the parabola is worthy of his genius, showing as it does that his researches in this direction were certainly not stopped by difficulties. Archimedes devised catapults and other warlike engines, and the story of his setting fire at the siege of Syracuse to the Roman ships of war, by means of mirrors concentrating the rays of the noonday sun, has in it nothing improbable. An eminent French naturalist, about the middle of the last century, demonstrated the possibility of this feat by means of a number of plane mirrors suitably arranged, with which he was able to set wood on fire at the distance of one hundred and fifty paces. We may at least infer, from the story of there being mirrors, that Archimedes had made experiments involving some knowledge of the laws of optics.

The acquaintance which Archimedes possessed with the mechanics of liquids may be illustrated by the contrivance which is still called the screw of Archimedes, and of which one form is represented in Fig. 13. It commonly consists of a pipe wound like the thread of a screw round a cylinder. The cylinder being placed in a sloping position with its lower extremity immersed in a reservoir of water, the effect of turning it round is to cause the water to ascend the pipe, from the upper orifice of which it is discharged, and thus the water is transferred from the lower to the higher level. It may be that this apparatus was not invented by Archimedes, but only adapted by him from Egypt, where it is still in use.

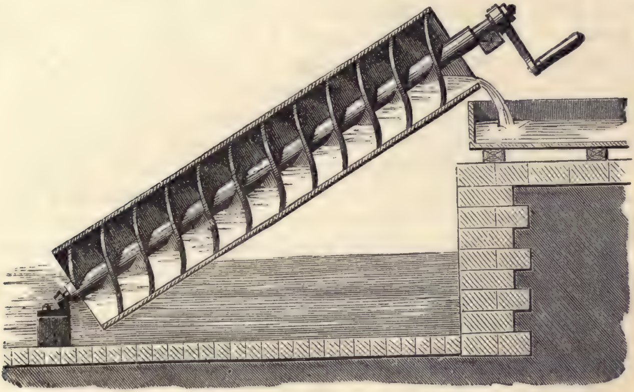

FIG. 13.

The story of Hiero's crown has often been told. The King had given to an artificer a quantity of gold which was to be fashioned into a crown. When the work was completed, the King found that its weight corresponded with that of the metal which had been delivered to the goldsmith; but he suspected that some of the precious metal had been kept back, and its weight made up by baser materials alloyed with the gold in the crown. He sent the crown to Archimedes to pronounce on the true state of the case. How to do this was for some time a puzzle to the philosopher; but while thinking over the matter, a slight incident suggested the solution of the problem. He was one day entering his bath, which happened to be quite filled, and noticing that the water overflowed its edge in proportion as he immersed his body in the liquid, it struck him that the quantity of water which thus ran out constituted an exact measure of the bulk of the immersed body which displaced it. He immediately perceived if the crown were of pure gold, it would, when immersed in a vessel quite full of water, cause the same quantity of the liquid to run over the brim as would a lump of gold of the same weight as the crown; whereas the latter, if alloyed with silver or bronze, would displace more water than the lump of gold. When this idea flashed upon the bather's mind, he was so overjoyed at his discovery that he leaped from the bath, and ran home, unclad as he was, crying "*Eureka! Eureka!* I have found it out! I have found it out!" Thus led to make experiments on the weight of bodies in air and in water, Archimedes soon arrived at the general fact which is still called in our text-books the *principle of Archimedes*, and is thus expressed: "Every body immersed in a liquid loses of its weight a portion equal to the weight of the liquid it displaces." The true explanation, however, of this apparent loss of weight in a solid body immersed in a liquid was reserved for modern science.

In geometry Archimedes determined by calculation the perimeters of inscribed and circumscribed polygons of ninety-six sides, and that the ratio of the circumference of the circle to the diameter lay between $3\frac{10}{70}$ and $3\frac{10}{71}$. Treatises by him on the spiral called by his name, on spheroids and conoids, and on the sphere and the cylinder, have come down to us, and these contain some beautiful and interesting theorems.

He devised methods of calculating the surfaces and solid contents of these forms and of given portions of them. One celebrated demonstration proves the singularly simple relation which exists between a sphere and a cylinder exactly enclosing the sphere, namely, that the surface of the sphere has two-thirds the extent of the surface of the cylinder (including in the latter its two bases), and that the solid contents of the sphere are also two-thirds of those of the cylinder. Archimedes showed also that the curved surfaces of the cylinder and sphere comprised between any two planes perpendicular to the axis of the cylinder are equal. The discovery of these relations was so gratifying to Archimedes, that he requested that after his death his tomb should have a cylinder and a sphere figured upon it. The *method of exhaustions* received at the hands of Archimedes a new and beautiful application in his method of determining the area of a parabola. He inscribes a triangle in the parabola, then in each of the two remaining segments another triangle, after that another triangle in each of the four, eight, sixteen, etc., segments successively remaining. The sum of these triangular areas continually approaches that of the parabola. He shows that if the area of first inscribed triangle be called 1, that of the next two triangles will be $\frac{1}{4}$, that of the next four $\frac{1}{16}$, and so on. Therefore the sum of the series is $1+\frac{1}{4}+\frac{1}{16}+\frac{1}{64}+$etc. It may surprise a reader unacquainted with mathematics, that the addition of any number whatever of successive terms will not increase the sum of this series beyond a certain limit. Continual addition only brings the sum *nearer* to the limit, which is $1\frac{1}{3}$. This investigation, therefore, involved the notion of a new kind of limit, namely, one to which a numerical quantity might continually approach. We see, therefore, that the area bounded by the parabolic curve must be precisely equal to $1\frac{1}{3}$ times the area of the inscribed triangle. The mere enumeration of all the various mathematical labours of Archimedes would require more space than we can here afford. His genius has justly been regarded as one of the greatest the annals of science can exhibit.

Archimedes perished in the fall of his city to the Romans, B.C. 212. Marcellus, the Roman general, raised over his grave a monument bearing a cylinder and a sphere. Nearly a century and a half afterwards Cicero, who greatly admired the genius of Archimedes, had to make a minute search in order to discover the burial-place of the philosopher, which he found neglected and overgrown with vegetation. Thus, he remarks to the Syracusans, the memorial of their greatest citizen would have remained unknown had it not been shown to them by a man from Arpinum.

Scarcely had the career of Archimedes prematurely closed at Syracuse before the school of Alexandria witnessed the commencment of that of another famous geometer, APOLLONIUS OF PERGA (*c.* B.C. 200). His works were regarded by the ancients as the full sources of all geometrical science, and among these works his treatise on the conic sections has most contributed to his celebrity. He appears to have been the first who perceived that the circle, ellipse, parabola, and hyperbola could be formed by different sections of the same cone. The sections of the cone are now so familiar to us that the merit of this discovery is liable to be underrated. Apollonius investigated a multitude of the properties of these curves. Every section of a cone made by a plane passing through the vertex is bounded by two straight lines passing through that point. If the cone be cut by another plane parallel to one of those just mentioned, the section will be the curve called the hyperbola. If the cone be cut by a plane parallel to any one which passes through its vertex without intersecting the cone, the section is an ellipse or a circle, according as the plane is inclined, or is perpendicular to the axis of the cone. *See* Fig. 14. The Conic Sections of Apollonius consisted of eight books, of which only seven remain, and three of these are known only through an Arabic version.

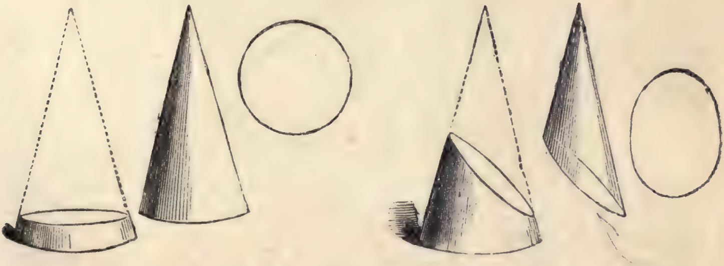

FIG. 14.

Apollonius treated also of involutes and evolutes, and of geometrical *maxima* and *minima*.

NICOMEDES, a geometer who lived about two centuries before the commencement of the Christian era, is remembered as the inventor of a peculiar curve called the *conchoid*, from its supposed resemblance to the outline of a shell. We are provided with the means of mechanically tracing this curve by means of an arrangement of straight rulers contrived by Nicomedes himself. As it may be interesting to some readers to observe for themselves the curious way in which the same movement of a single straight ruler may be made to trace out an indefinite number of differently curved lines, we shall briefly describe the instrument of Nicomedes by help of Fig. 15. A B and C D are flat rulers, of which A B has a groove or slit parallel to its length, and C D has a fixed peg at E, which passes through a slit in the movable ruler F G, which is itself provided with a fixed peg at H, that always slides in the groove A B. A pencil or tracing-point being fixed on any part of the ruler F G, the movement will cause this point to describe a *conchoid*, the form of which varies according to the point of F G to which the pencil is attached. The conchoid affords a means of solving some interesting geometrical problems.

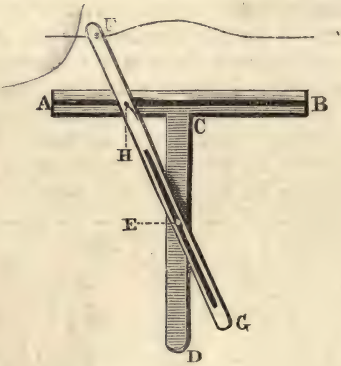

FIG. 15.

Pre-eminent among the many distinguished men belonging to the school of Alexandria, we find the great astronomer HIPPARCHUS (B.C. 160—125). He has been called, not without justice, the father of scientific astronomy, and the Newton of the Greeks. Only one of the works of Hipparchus is now extant, and it is perhaps the least important of the treatises he is known to have written. It is a Commentary on an astronomical poem by an author named Aratus, and is chiefly interesting as showing that Hipparchus was acquainted with spherical

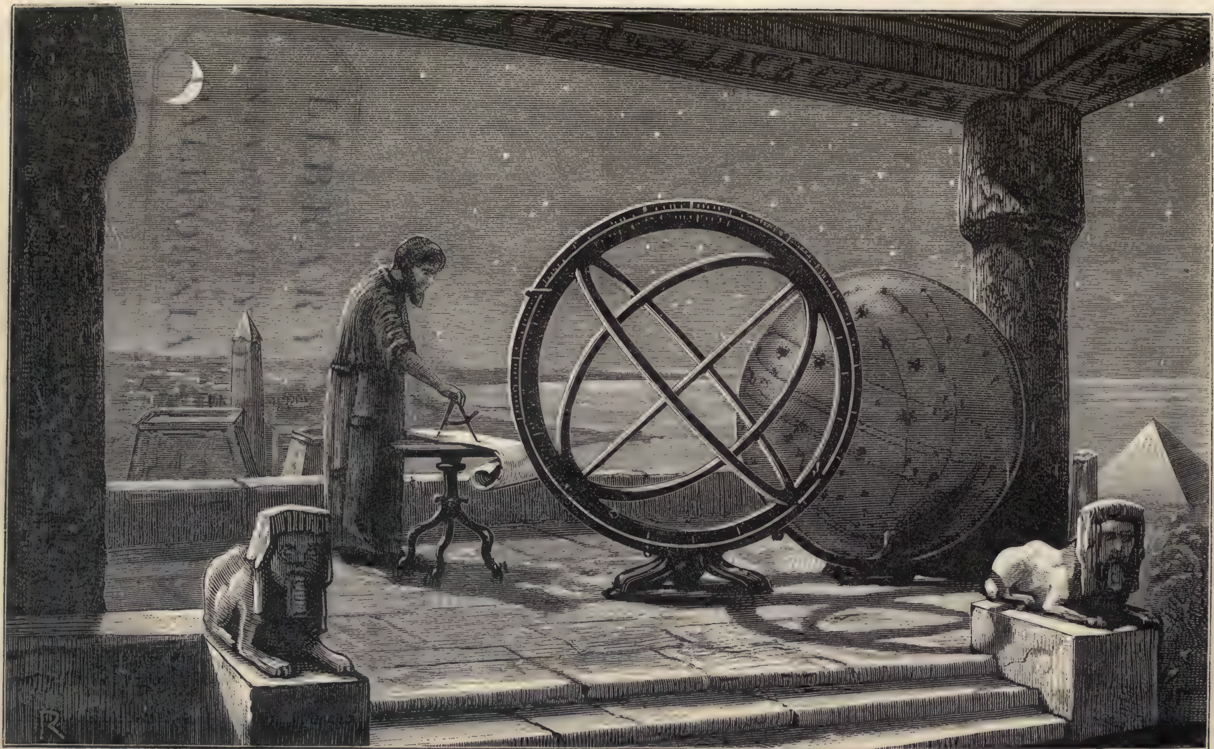

PLATE III.—HIPPARCHUS.

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trigonometry. The great armillary sphere which had been set up at Alexandria according to the design of Eratosthenes proved in the hands of Hipparchus the instrument of great discoveries. The representation of it in the illustration on the preceding page will serve to give the reader only a general notion of the instrument. It consisted of two large bronze circles, the outer one fixed in the plane of the meridian, and the inner movable with the former in the same plane; one of these circles was divided into 360°, and the other carried a pair of pointers or projecting bars. Within these circles were others corresponding with the planes of the ecliptic and the equator.

CTESIBIUS and HERO must be named as the successors of Archimedes in the mechanical sciences at Alexandria. It was they who resolved all machines into the simpler elements which we still call the *mechanical powers*, namely, the lever, the wheel and axle, the wedge, the screw, and the pulley. Ctesibius is credited with the invention of the forcing-pump; and both invented curious hydraulic and pneumatic apparatus, such as clepsydras with moving figures, windmills, fountains, syphons; and contrived methods of exhausting and compressing air, not apparently unlike those still employed in the air-pump and the air-gun.

There are notices in Vitruvius and Cicero of other mechanicians, astronomers, geographers, and mathematicians, belonging to the older school of Alexandria, but we pass over these as less important. But here we shall interrupt the history of Alexandrian science, to say a few words about the science of the Romans.

Great as were the Romans in war and civil polity, they had to acknowledge the superior genius of the Greeks in art, poetry, oratory, and philosophy, and they made the great Hellenic productions their models in all those departments. Geometry, astronomy, and physical science generally, the Romans disregarded altogether; nay, they even contemned the pursuits of science as partaking of a mechanical character, regarding them as beneath the dignity of a man of liberal education or of high birth. Not a single great original work on philosophy or science can be attributed to a Roman. Philosophy appears to have attracted the attention of the Romans only as furnishing precepts for the guidance or consolation of life, and we find them divided between the two rival schools of the Epicureans and the Stoics. The knowledge of nature interested them only from the same practical point of view, if we may judge from Virgil's lines:

> Felix, qui potuit rerum cognoscere causas
> Atque metus omnes et inexorabile fatum,
> Subjecit pedibus strepitumque Acherontis avari.

In the great poem of LUCRETIUS (B.C. 99—55), "De Rerum Natura," we find a remarkable exposition of the philosophy of the Epicureans, who had in great part adopted the physical views of Democritus, which have been already adverted to. These views were developed by Lucretius with great clearness and penetration. His scientific doctrines are in general more just than those of former writers; thus, instead of repeating the Aristotelean error that heavy bodies move faster than light ones, Lucretius expressly states that all bodies are equally borne onwards in an unresisting void, though their weight should be unequal.

FIG. 16.—THE RUINS OF THE COLOSSEUM AT ROME.

Ovid is another poet who enters much into subjects connected with physical science, and other writers might be named who exhibit in a striking light some part of the system of nature, in adorning the doctrines of their philosophical schools with all the attractions of poetical imagery.

Only one Roman, C. SULPITIUS GALLUS, is mentioned as a cultivator of astronomy. Cicero translated the poem of Aratus, which merely contains the most elementary notions of the science, and these are in no small degree mixed up with astrological absurdities. When the Roman calendar had become involved in extreme confusion by the neglect of astronomical data, Julius Cæsar, himself possessed of considerable astronomical knowledge, obtained the assistance of a Greek astronomer named Sosigenes to devise some method of adjusting the civil year into correspondence with the astronomical year. This was accomplished by every fourth year intercalating an additional day, and thus was introduced the Julian Calendar, which has, with some slight modifications, continued in use to the present day. In it the year is assumed to be 365¼ days, and the civil year is made 365 days, except in every fourth year, when it is 366 days. The true length of the year is, however, somewhat less than 365¼, and it was this excess of a few minutes which had accumulated to eleven days in the year A.D. 1752, that rendered necessary the change from the "Old Style" to the "New Style" in Great Britain.

We have no original scientific discoveries to record of a single Roman. Their labours as regards the knowledge of nature appear to have been limited to collecting the records and observations of others. PLINY THE ELDER (A.D. 23—79) has left the most voluminous writings of this kind, in which he treats of the stars, the heavens, wind, rain, hail, minerals, trees, plants, flowers, all animals, birds, and fishes, together with geographical descriptions of places and histories of arts and sciences. Pliny fell a victim to his scientific curiosity regarding an eruption of Vesuvius, and his name therefore deserves an entry in the list of the "martyrs of science." The connection of the tides with the moon is clearly pointed out by Pliny, who, however, mentions that it had been vaguely known before his time. Pliny states that a sphere of rock crystal was used as a burning-glass for collecting the sun's rays to a focus, and he mentions the power of a globular glass vessel filled with water to produce the same effect, expressing his surprise at finding that the water itself remained quite cold. Seneca also mentions the magnifying power of a glass of water, and he refers this effect to some power of the water. He notices also the prismatic colours seen in an angular piece of glass.

A later writer named AULUS GELLIUS (*c.* A.D. 130) has some interesting particulars about optical contrivances, and from some of his statements it would appear that he was acquainted with the fact of concave mirrors producing images which might pass among the ignorant as miraculous apparitions.

We may now return to Alexandria, which continued, after Egypt had become a Roman province, to be the seat of almost all the science of the time. It is true that the glory of the place as a school of learning had declined, so that we do not find any names that need be here mentioned from the time of Ctesibius and Hero until we reach the astronomer Ptolemy.

CLAUDIUS PTOLEMÆUS (*c.* A.D. 150), who is usually known by the name of PTOLEMY, flourished at Alexandria in the second century of the Christian era. His name is not derived from any connection with the race of Egyptian monarchs, but from the circumstance of his having been a native of Ptolemais in Egypt. He revived the study of astronomy at Alexandria, where little advance in the science had been made since the time of Hipparchus. He formed the design of collecting all the materials which his predecessors had left behind, and embodying the existing astronomical knowledge into a regular and complete system. The publication of Ptolemy's work, entitled Μεγαλη Συνταξις, "The Great System," forms an epoch in the history of science. This work constituted the basis of all the astronomy of the Middle Ages and, for a considerable time, of that of modern Europe. It is a matter of regretful speculation to consider how much more rapid might have been the progress of astronomy had Ptolemy accepted the Pythagorean opinion, which places the sun and not the earth in the centre. In extenuation of this error, it may be remembered that the ancients were not in possession of so many proofs of the earth's motions as are familiar to us; yet the arguments which had been advanced by Aristarchus and others ought to have decided the point, if they had been considered in a truly philosophic spirit. The argument advanced by Ptolemy against the earth's motion round the sun, on the ground of the invariable aspect of the fixed stars, had been refuted, as we have seen, by Aristarchus four centuries before. Ptolemy also reasoned from the erroneous and arbitrary theories of motion assumed by the Aristotelean philosophers, contending that if the earth were really in motion, it would leave behind it, riding on the air, all the loose bodies we see on its surface, because they are so much lighter than the earth. This shows how little the Greeks were acquainted with the facts relating to the motions of bodies, and how their best geometers could arrive at false conclusions on such subjects, by reason of assuming false premises. They accepted it as a truth that a heavy body must move faster than a lighter one, whereas now everybody who has heard of the guinea and feather experiment knows that this is not the case. Ptolemy repeats the same argument in opposing the doctrine of the earth's diurnal rotation, which appears to him as ridiculous as the former; for the earth rapidly revolving from west to east would leave behind it the clouds, the birds in the air, and, in general, all objects not solidly attached to it. A stone thrown eastwards would not, he thought, advance at all, but the earth would gain upon it by reason of its greater velocity.

Ptolemy therefore pronounces that the earth is fixed and motionless in the centre, and that the planets revolve round it in the following order, according to their distances from it: first the Moon, then Mercury, Venus, the Sun, Mars, Jupiter, Saturn, and beyond these the sphere of the fixed stars.

To account for the observed motions of the moon and planets, Ptolemy adopted and extended the theory of *epicycles*. Hipparchus had explained the sun's motions by supposing that the sun revolved in a circle, but the position of the earth was not at the centre of this arch, but nearer to one part of the circle than to another. This is the theory of *excentrics*. The sun's motions admit also of the following explanation, which was that advanced by Ptolemy. Suppose E (Fig. 17) to be the earth's position, and that the sun revolves about it, not simply in the circle A B C D, which is the path described by the centre C of a smaller circle in which the sun revolves, while the small circle is carried uniformly round the larger one A B C D. The smaller circle is called an *epicycle*. Now, the moon's motion is much more complex than that of the sun, and one of the inequalities (due to the form of its orbit) had been discovered by Hipparchus. Ptolemy detected another, called the *evection*, of a more complicated kind, and depending on the angular distance between the sun and moon. To explain these inequalities in the moon's motion, Ptolemy was obliged to resort to a very complex combination of eccentrics and epicycles, and when he came to deal with the movements of the planets, an increased complexity in the systems of excentrics, epicycles, and deferents was necessary.

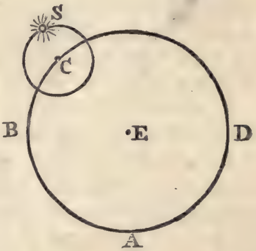


FIG. 17.

This extremely complicated system arose from Ptolemy's binding himself by the dogma of the celestial motions being always circular and uniform. It was not unnatural that in the infancy of astronomical science all the movements of the heavenly bodies should have been conceived to take place in circles at an unvarying velocity, and such a theory would doubtless offer facilities for calculations which would yield results sufficiently corresponding with observation to meet the requirements of that time. But the error which had so long retarded the progress of science, and which lay in mistaking gratuitous and arbitrary assumption for the laws of nature, still showed itself in Ptolemy's accepting Aristotle's fancies about the perfection of circular motions, incorruptibility of the heavens, etc. Yet the Ptolemaic system of the universe attained the ascendency, and prevailed until modern times, in spite of the increasing perplexities in which every new discovery of a hitherto unobserved inequality involved its followers, by requiring additional machinery, until they had

> the sphere
> With centric and excentric scribbled o'er,
> Cycle and epicycle, orb in orb.

Ptolemy has given an account of the instruments used for astronomical observations at Alexandria. The use of the gnomon, a vertical pillar by the shadow of which the sun's altitude may be determined, was known to the Greeks from a very early period, but the Alexandrian astronomers seemed to have superseded it by armillary spheres of various kinds. These consisted of metallic circles or rings fixed in a position corresponding with the position of the meridian, the ecliptic, the equator, etc. Thus, for observation of the equinox they used two large circles attached to each other, the one fixed in the plane of the meridian and the other in the plane of the equator: at the moment of the equinox the latter circle was presented edgeways to the sun, so that the shadow of the outer portion fell upon the inner edge. For the solstice one circle fixed in the plane of the meridian had another concentric circle in the same plane revolving within it, and carrying two pins projecting one at each extremity of a diameter, and perpendicular to the plane of the circle. To observe the sun's altitude the inner circle was turned until the shadow of one pin completely covered the other, when the graduations on the rims of the circles gave the altitude of the sun's centre. Ptolemy improved on the solsticial circle by substituting a quadrant. He also invented a simple method of observing the altitudes of stars by means of a pair of jointed rulers, one of which being maintained in the vertical position while the other was pointed at the star, the angle between these directions was inferred by the distance intercepted on a third graduated ruler between the other two. Considering the imperfect appliances at their disposal for observations and measurements, it is a matter for wonder that the ancient astronomers should have been able to accomplish so much. The want of an instrument for accurately measuring time was a great deficiency, for the chronometer is the very soul of practical astronomy. Some of the ancients endeavoured to use the clepsydra, or water-clock; but this instrument was so liable to give erroneous indications that Ptolemy rejects it altogether. The clepsydra was simply an arrangement for showing the quantity of water which escaped from a small orifice in a large vessel, but the rate of flow in such an arrangement is liable to irregularities which cannot be provided for. In default of clocks the ancient astronomers had resort to a rather ingenious expedient when it was necessary to ascertain the hour at which an astronomical occurrence took place. Suppose, for example, that an eclipse of the moon was being observed, the times of its commencement and termination were found by the observer taking the height at those instants of some well-known fixed star. From this observation, the latitude of the place, and the known position of the sun, it was not difficult to deduce the time. The same method was used by astronomers down to the period when the clock was invented.

Besides the great work on astronomy which Ptolemy produced, he compiled a remarkable and voluminous treatise on geography. The materials for this were collected from a multitude of authors who had given descriptions of countries and travels, and these materials were laboriously compared and sifted by Ptolemy. It is noteworthy that in this work Ptolemy gives us the first notions of the construction of maps, and in it he makes use of the method of fixing the positions of places by their latitude and longitude, which had been introduced by Hipparchus. Ptolemy's geography, like his astronomy, continued to be the great text-book of its subject down to the rise of modern science. Nor does the list of the labours of Ptolemy close here, for he left a treatise on optics, in which he discussed the subject of atmospheric refraction, the laws of reflection from spherical mirrors, and other subjects. We have also a treatise on music by Ptolemy, and ancient authors mention also works on mechanics and other subjects.

After the age of Ptolemy there were many cultivators of the mathematical sciences at Alexandria. One of the most noted of these is DIOPHANTUS (fourth century), who is often named as the inventor of algebra. He devoted much attention to the kind of problems called *indeterminate*, and these were in consequence known afterwards by the name of the Diophantine Problems. In the fourth century there also flourished at Alexandria the distinguished geometer PAPPUS. In his work called "Collectiones Mathematicæ" he discusses questions relating to geometrical *maxima* and *minima*. The nature of these questions may be understood by a single illustration: Let a number of regular figures be given, as a square, a hexagon, an octagon, a circle, etc., such that all these figures shall have their boundaries of one and the same length: the question may be asked, which of these "isoperimetrical" figures has the greatest area? The investigation of this point by mathematical reasoning is not a very simple matter, but Pappus succeeded in solving the problem for a variety of cases. But what is most curious is the application of mathematical doctrine which the Alexandrian mathematician makes to the form of the cells of the honeycomb. He shows that of the regular figures only equilateral triangles, squares, and hexagons would occupy a space without leaving interstices. But the discussion of isoperimetrical figures having shown that the hexagon contains for the same boundary the greatest area of any of these three figures, the economy of material resulting from the form of the bees' cells is demonstrated, and this induces Pappus to exclaim, "The bees work with a kind of geometrical forethought." Contemporary with Pappus was THEON OF ALEXANDRIA, whose Commentaries on Euclid and on Ptolemy's *Syntaxis* have come down to us.

The beautiful and hapless HYPATIA (A.D. ?—415), the daughter of Theon, presents us with the rare phenomenon of a distinguished female cultivator of mathematics and philosophy. She wrote Commentaries on the works of Apollonius and of Diophantus, and calculated some astronomical tables. Of her writings none are extant but the Commentary on the Third Book of Ptolemy's Syntaxis, which Theon expressly acknowledges to be hers. She lectured at Alexandria on mathematics and the Neo-Platonic philosophy. But science and philosophy had now at Alexandria fallen upon evil days, for the population of this once famous seat of science was now kept in continual turmoil by the bitter disputes and deadly animosities of three parties—the Christians, the Jews, and the Pagans. Under the unscrupulous fanatic Cyril, the former obtained the ascendency, and of course Hypatia and Greek philosophy could no longer be tolerated. At the instigation of Cyril, a mob of raging yelling bigots assailed Hypatia as she was one day returning from her lecture-hall, and with circumstances of the greatest barbarity dragged her into a church, where she was killed by the ready club of a zealot known as "Peter the Reader." The murder of Hypatia may be taken as marking the almost complete extinction of the Greek science and philosophy at Alexandria. Such was the close of the famous school which had for so many centuries held uninterrupted and nearly exclusive possession of whatever was in the world intellectually best, and had been constantly furthering the highest interests and truest progress of humanity.

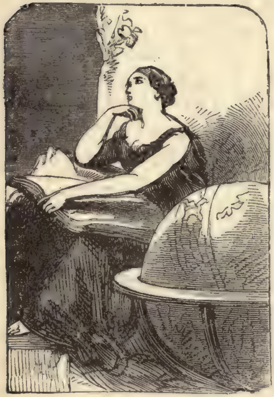
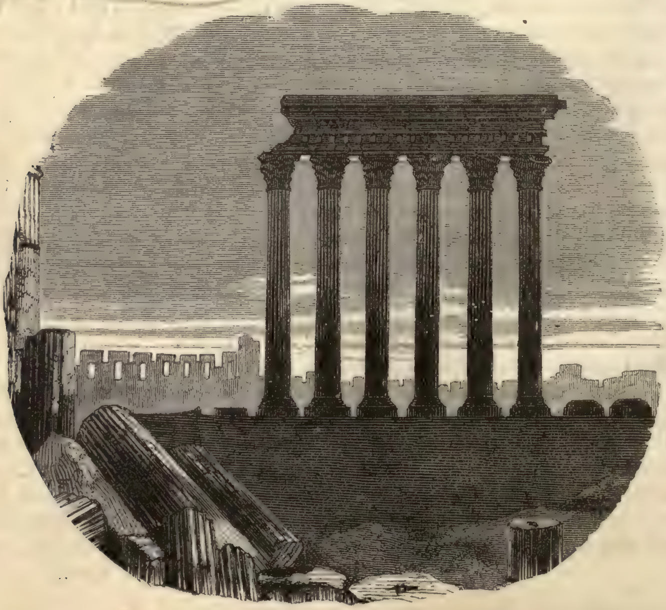


FIG. 18.—HYPATIA.



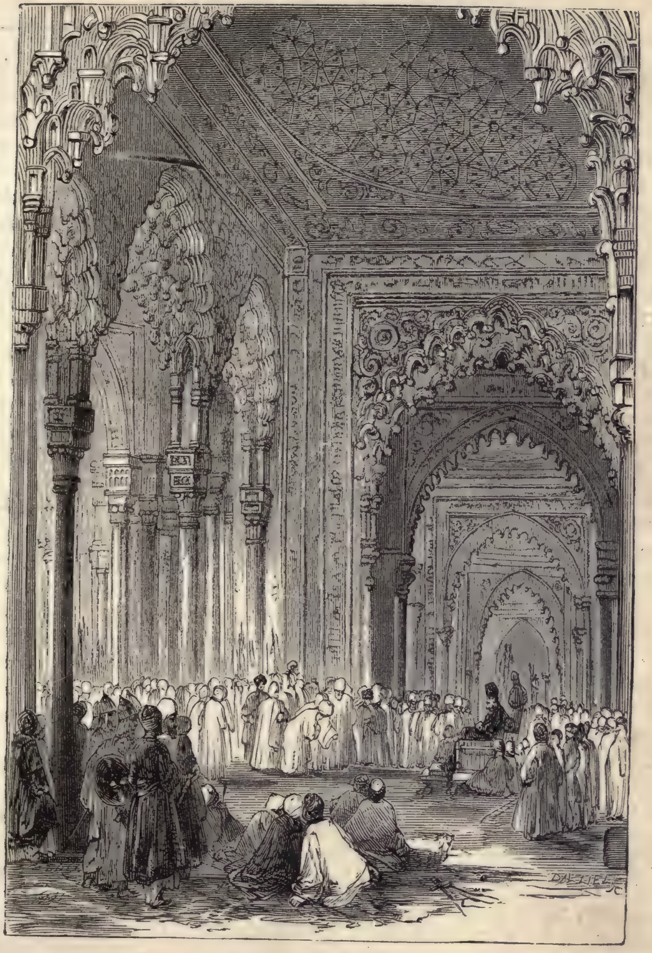


FIG. 19.—A HALL IN THE ALHAMBRA.

# CHAPTER III.

## ARABIAN AND MEDIÆVAL SCIENCE.

THE once famous school of Alexandria continued in a state of decadence without sign of revival, while the course of events was bringing about the almost complete extinction of philosophy and learning in Europe. The division of the Roman empire under Theodorus, A.D. 395, and the troubles occasioned by the invasions of the Goths and other barbarous tribes, may account for that final neglect of Greek literature at Rome which caused the records of science and philosophy, written as they were in the Greek language, to remain for ages a dead letter to the nations of the West. Centuries elapsed before the writings of Aristotle and Ptolemy were again made accessible to the European student by Latin translations—not from the Greek originals, but from versions of those prepared and preserved by an Eastern race, with whose annals our history now becomes connected, and who cultivated science and fostered learning at a period when every European nation was enveloped in the deepest intellectual darkness.

Perhaps no page in history is more strange than that which describes the rise and progress of the Mahometan empire; but the accounts must be sought in the general history of the world, where the reader will find a record of the wonderful series of events which gave the Arabs of the desert an empire more extensive than that of the Cæsars. There the reader may learn how these people, bursting from the sterile plains of their native peninsula, spread themselves with the force and rapidity of a deluge over many beautiful and fertile regions, whose possessors were too enervated by luxury, too depressed by tyranny, or too cowed by superstition to oppose an effective resistance. In the East the Mahometan conquests embraced Syria, Persia, and Asia Minor; along the north of Africa they stretched from Egypt to Morocco; and in Spain extended from Gibraltar to the Pyrenees.

The Koran, when first promulgated by Mahomet, was universally accepted by his followers as containing everything necessary or useful for man to know. His people were at first acquainted with no other book, and indeed considered all other learning superfluous. When the city of Alexandria was taken by the Saracens (A.D. 642), their general sent to inquire of the Khalif Omar what should be done with the library. The reply was that the library was to be destroyed, "for the books which contain what is already in the Koran are unnecessary, those that contradict it are pernicious, and those that treat of other matters are useless." Accordingly the remnant of the great library of the Ptolemies, which, after sustaining for nine centuries damages from fire, war, plunder, and theological bigotry, was still a magnificent collection, was committed to the flames, or, as some say, furnished fuel for six months to the public baths of Alexandria.

Yet the Arabians, although they were originally amongst the most barbarous of Asiatic races, had a true aptitude for science, philosophy, and literature, and their intellectual genius was only awaiting the proper conditions for its development. For no sooner had the Saracen empire been extended and the Mahometan religion established by a series of conquests of unprecedented rapidity, than the hitherto uncompromising fanaticism of the followers of the Prophet subsided; and so marvellous a change came over the opinions of their leaders, that when the rest of the world was steeped in ignorance and barbarism, literature, philosophy, and science found a home with the Arabs. ALI, the fourth in succession from Mahomet and son-in-law of the Prophet, was already a great patron of literature, and even himself an author. Some of his sayings are still extant, such as "A man's learning is more valuable than his gold;" "Eminence in science is the highest of honours;" "To the dominion of science there is no end;" "He dies not who gives his life for science."

In the tenth century of our era circumstances led to a threefold division of the sovereign power amongst the Arabians. From that period one series of Khalifs governed the Eastern Arabs at Baghdad, another held rule in Egypt, and a third presided over the Saracenic empire in Spain. The cities at which the seats of government of these several monarchies were established became the most populous in the world. Such was Baghdad on the Tigris, and such was Cordova on the Guadalquiver. The latter city, according to its Moorish historians, contained at the height of its prosperity a million of inhabitants, and more than 200,000 houses. "After sunset," says Dr. Draper in his "History of the Intellectual Development of Europe," "a man might walk through Cordova in a straight line for ten miles by the light of the public lamps: seven hundred years after this there was not so much as one public lamp in London. The streets of Cordova were solidly paved: in Paris, centuries afterwards, whoever stepped over his threshold on a rainy day stepped up to the ankles in mud." Probably by drawings or descriptions the reader knows something of the beautiful residence of the Moorish princes of Granada, or has at least seen its glories shadowed forth in the elegant "Alhambra Court" of the Crystal Palace. Let him compare that one Moorish palace with the dwellings of the contemporary monarchs of France, Germany, and England, which are described as mere hovels without windows or chimneys, and with mere holes in the roof for the smoke to escape. The contrast here suggested can hardly fail to show the great mental and material advance which the descendants of the desert tribes had made in comparison with our European ancestors.

Everywhere in the Arab dominions, schools, colleges, universities were established when these strangers had settled in their new conquests. In Spain alone seventy libraries were open for public use, and the library of the Spanish Khalifs comprised 600,000 volumes. The library at Cairo contained a vast collection, and also boasted of the possession of the great brazen globe which Ptolemy Claudius had used for his astronomical observations, besides another globe made of silver, valued at a fabulous sum. Cairo was the seat of a great medical school, in which students were required to pass regular examinations before they were permitted to practise. Compare this with the condition during the same period of Christian Europe, where even the kings could not sign their own names, priests could hardly read, and sick people had no other hope than miracle-cures. A long list also might be made of important inventions and improvements in the arts, for which we are indebted to the Arabs. Improved preparation of gunpowder; cultivation of silk; weaving of silk; fabrication of finely-tempered weapons, as in the famous Toledo blades; preparation of the best kind of leather, still called morocco and cordovan; training of the horse, so that the world obtained a variety of that noble animal possessing the highest development of its finest qualities; use of the mariner's compass: these are but a few of the advances in the arts of civilized life which we owe to the Arabs. We must not omit to mention another invention of theirs which has in no slight degree contributed to the diffusion of learning, and that is paper made from linen, to take the place of the far more costly parchment.

Among the various literary, philosophical, and scientific studies which were zealously pursued at Baghdad under the Khalif ABN JAAFAR AL MANSUR (*reign:* 754—775), mathematics, astronomy, and medicine occupied a prominent place. In all these subjects the Arabs profited by the works of the great Greek writers, whose treatises they translated and published in the Arabian language. The regular study of medicine, which was a great feature in Arabian science, appears to have been introduced by Al Mansur. From the circumstance of this ruler engaging a Christian physician at a princely remuneration to instruct his people, it may be inferred that among the Arabs theological intolerance was soon subordinated to the love of knowledge. Incidents of this kind abound, and they contrast strangely with the ignorance and bigotry which then and long afterwards prevailed in other quarters. Haroun al Raschid (a name familiar to every one whose imagination has been charmed with the wondrous stories of the "Arabian Nights' Entertainments"), enacted, as we are assured by grave historians, that every mosque in his dominions should have a school attached to it, and—what is still more noteworthy—he entrusted the general superintendence of all the schools to a Christian. In Spain we read of learned Jews occupying positions at the head of the great schools or universities which the Moors had established in that land. A man's intellectual fitness, and not his religious opinions, was exclusively regarded by these enlightened Arabs as his proper qualification for teaching science or learning.

The golden age of Arabian learning in the East was attained under the KHALIF AL MAMUN, who ruled at Baghdad from A.D. 813 to 833, and to whose court resorted the best poets, philosophers, and mathematicians of the time. He sent learned men to all parts of the world to collect ancient manuscripts, and during his reign it was not uncommon for trains of camels to enter Baghdad laden with nothing but the precious volumes which held the literary treasures of the past. This khalif greatly encouraged the study of mathematics and astronomy. But there were not wanting some Mahometan theologians

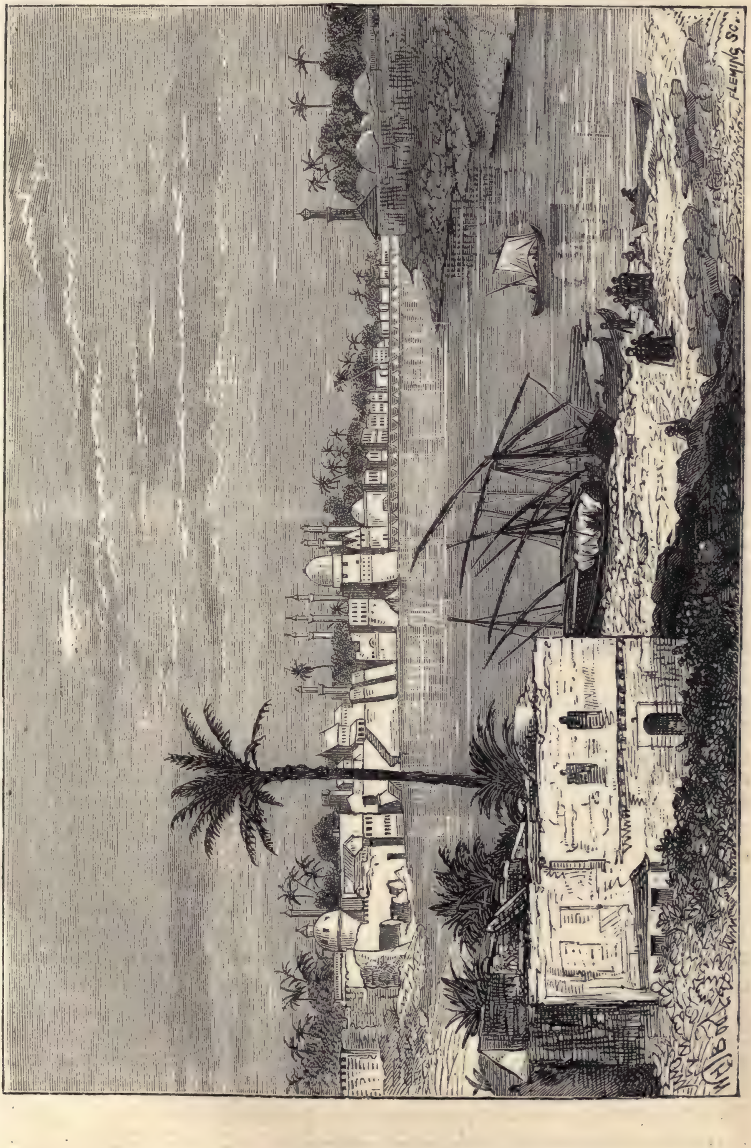


FIG. 20.—BAGHDAD.

ready to take alarm at this dissemination of the knowledge of science. It is recorded that a doctor of divinity of the period took upon himself to denounce such studies, asserting that the khalif would assuredly draw down upon himself the vengeance of Heaven for daring to permit the diffusion among the Faithful of philosophies and sciences which might unsettle their beliefs. This khalif it was who caused the great work of Ptolemy Claudius to be translated into Arabic (A.D. 827) under the title of the "Almagest," a word compounded of the Arabic article *al* and the first syllables of the Greek title of Ptolemy's treatise, Μεγιστη Συνταξις.

We may turn with more immediate interest to the acquirements of the Arabian conquerors of Spain, because from them we trace our scientific descent in the direct line. The Spanish Moors had no sooner become settled in their new possessions than they also turned their attention to intellectual pursuits, and they became the main channel through which the influence of Greek learning again flowed into Europe. It was only in the Moorish universities in Spain that Gerbert, who afterwards in the tenth century became Pope under the title of Sylvester II., could obtain a knowledge of geometry and astronomy, and could be made acquainted with the Arabic numerals, the use of which he introduced to the rest of Europe. It was in Moorish Spain, too, that the earliest astronomical observatory in Europe was erected. It is the tower of the Girada or Giraldo of Seville of which we here speak. It still exists, converted into a campanile or bell-tower, a change of its use which occurred when the Moorish power in Spain had been overthrown.

Astronomy was indeed a science which had a peculiar fascination for the Arabians, by whom it was diligently cultivated. Perhaps their predilection for astronomy may be accounted for by its connection with a pretended science that was more generally and more eagerly pursued, and exercised no little power through its adepts, both before and since the Arabian period. We allude, of course, to astrology, which professes, by observations of the positions of sun, moon, and planets at a particular moment, to foretell the destiny of an individual or of a state. The belief in the influence of the heavenly bodies on man's fate appears to have existed from the earliest times, especially in the East, whence its professors appear to have first passed into Europe. We read, for example, in Judges v. 20, that "the stars in their courses fought against Sisera;" and Horace warns Leuconöe against seeking to penetrate the future by means of the Babylonian tablets (or almanacs of planetary position). The calculations of the astrologer were founded upon the notion that the position of the planets with regard to that sign of the zodiac which was rising at the moment of a person's birth determined, or at least announced, his fate. This configuration of the position of the planets is termed the person's *horoscope*, and it is obvious that to draw a horoscope correctly implies a

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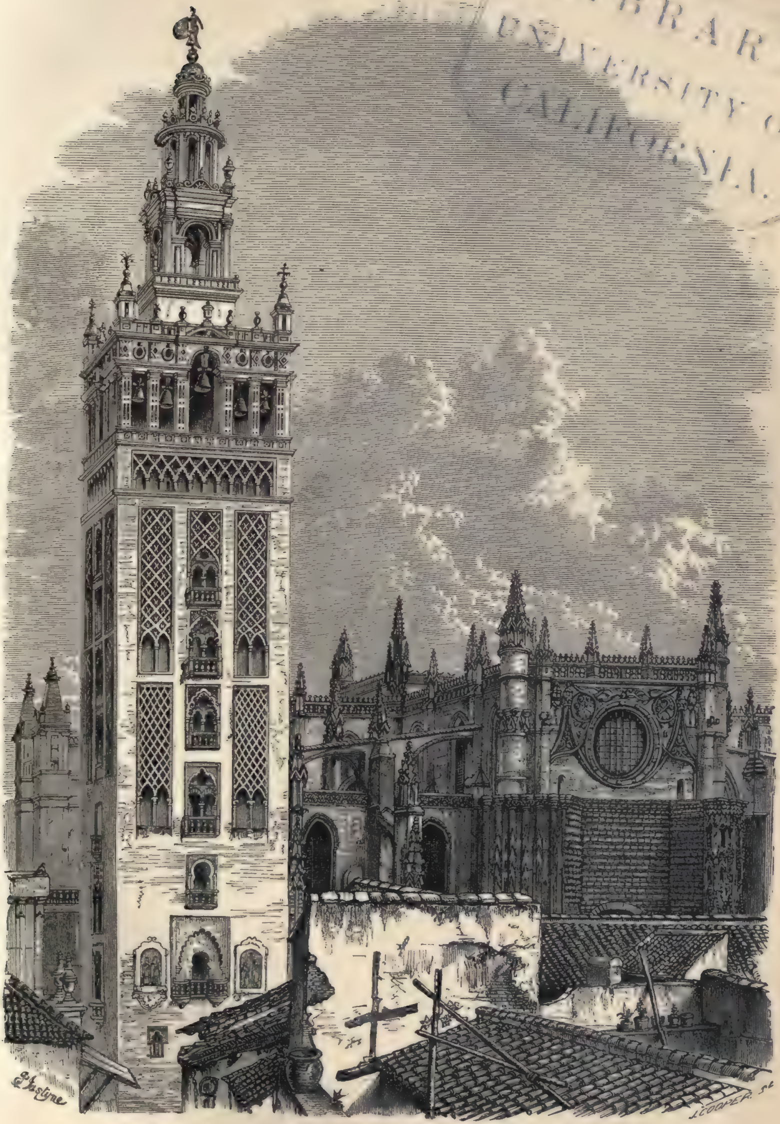
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PLATE IV.—THE GIRALDA AT SEVILLE,

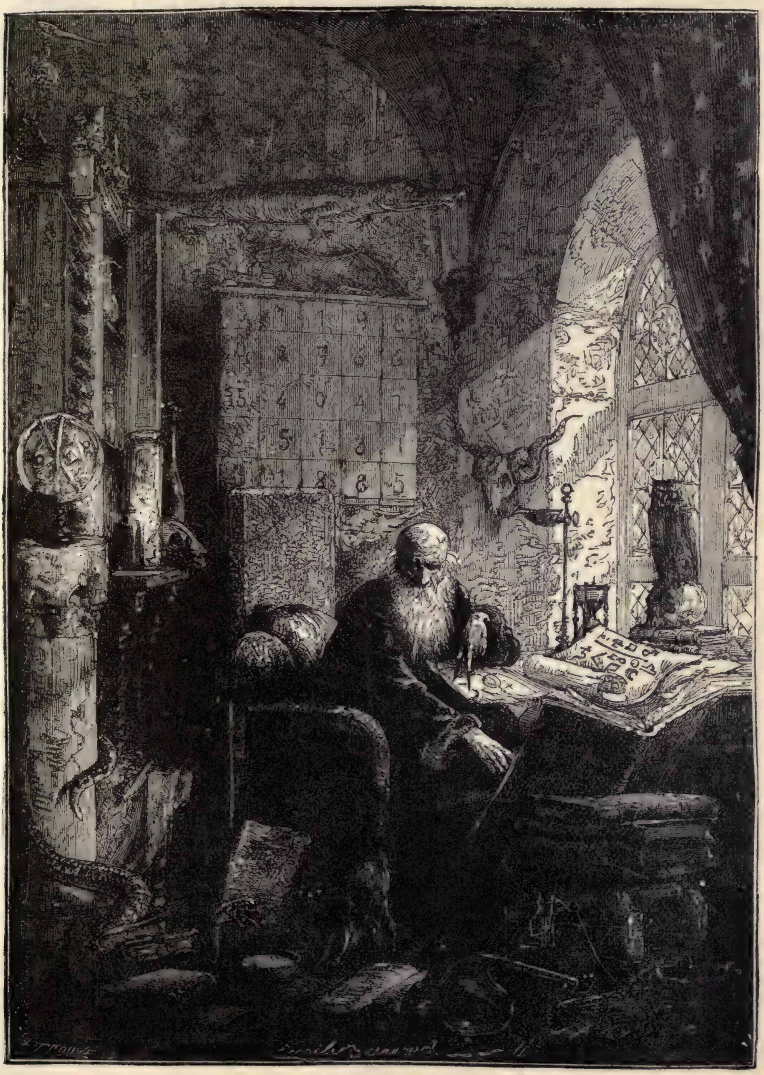

FIG. 21.—THE ASTROLOGER.

considerable knowledge of the motions of the heavenly bodies. There can be little doubt that genuine astronomical science has largely benefited by the general and long-sustained belief in the delusions of astrology; for the desire of penetrating into the future supplied a powerful motive for unremitting and accurate observation at periods when the love of science for its own sake might have failed to secure such a result. To astrology was owing the ardour with which the ancient Chaldeans scanned the heavens; and to the same cause we can trace the encouragement which many European princes, eager to read the course of human affairs through the starry influences which ruled them, so lavishly bestowed upon astronomers before the revival of letters and subsequently.

The list of Arabian astronomers is a long one, and a vast number of the observations they have recorded are still extant to testify to the accuracy and diligence of their compilers. It would be without interest to the general reader to give here a dry record of their several labours; but some of the more remarkable must be briefly mentioned. The astronomers of the Khalif El Mancoura were directed by him to make a new and more exact measurement of the length of an arc of the meridian. This determination was made on the plains of Mesopotamia, where two parties started from a given point, one measuring a degree northwards, the other southwards. The distances obtained were apparently 56 miles and 56⅓ miles respectively; but as there is some doubt about the exact value of the units of length in which the results are recorded, we are unable to judge confidently of their correctness.

The most distinguished of all the Arabian astronomers was perhaps ALBATEGNIUS, otherwise called EL-BATANI, whose observations were far superior in accuracy to any recorded by the Greeks. Yet the Arabs do not seem to have employed any other than instruments of the same kind as those of the Greeks, viz., the gnomon, armillary spheres, and mural quadrants. El-Batani over-corrected Ptolemy's estimate of the rate at which the precession of the equinoxes takes place. The Greek astronomer had made it 100 years for one degree. El-Batani's data gave him 66 years for one degree; but the true period is 72 years. An Egyptian astronomer named IBN JOUNIS, who about the year A.D. 1000 established an observatory at Cairo, drew up a set of tables containing many improvements on those of Ptolemy, and left a treatise on astronomy, which is remarkable for showing greatly improved methods of applying trigonometrical formulæ. The science of astronomy would have made but small progress without trigonometry, which treats of the relations between the angles and sides of triangles. By an investigation of these we are able to "*resolve*" triangles; that is, when any two angles and one side, or two sides and an angle, or the three sides of a triangle, are known, we are enabled by trigonometry to calculate all the remaining parts of the triangle. The Greeks investigated some of the purely geometrical relations of angles, but they did not advance to trigonometry, because they had a dread of contaminating their geometry with any consideration of numerical quantities. They therefore preserved to their geometry its ideal perfection, and they would have shrunk from the line of investigation which gives trigonometry a style so different from that of the Euclidean geometry. The foundation of trigonometry is the simple idea of giving *names* to the *ratios* between the length of the sides of a right-angled triangle. In Fig. 22 let ABC be a right-angled triangle, having the right angle at B; and let $a\,b\,c$ represent the respective lengths of the sides opposite to the angles marked by corresponding letters. The ratio $a : b$, that is, $a \div b$ or $\frac{a}{b}$, is called the *sine* of the angle CAB; $\frac{a}{c}$ is named the *tangent;* $\frac{c}{b}$ the *co-sine;* $\frac{b}{c}$ the *secant;* $\frac{c}{a}$ the *co-tangent;* and $\frac{b}{a}$ the *co-secant* of the same angle. The Arabian mathematician appears first to have introduced the *sine* in the calculation of angles; and about A.D. 1000 ABOUL WEFA, an astronomer of Baghdad, constructed tables of the values of the tangents and co-tangents. By a still later Arabian astronomer of Spain, Geber by name, we first find the cosine employed. The introduction of trigonometrical formulæ has placed in the hands of mathematicians a most powerful and fertile invention.

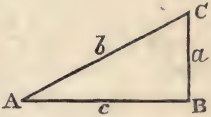

FIG. 22.

Though trigonometry is founded on geometrical considerations, it is practically a branch of algebra, a kind of universal arithmetic which was diligently cultivated by the Arabs. We have already seen that the Alexandrian Diophantus has some claims to be considered the first to use a system of algebraical signs. But it is usually considered that algebra had its origin in the East, and that from thence the Arabs obtained their knowledge of it. It is on record that Mahomet Ben Musa of Khorasan, who is distinguished as the author of the earliest treatise on algebra, travelled into India for the express purpose of improving his acquaintance with this subject. The same writer is also noted as being the first to introduce among his countrymen the well-known characters 1, 2, 3, etc., in which we usually write numbers. These Arabian characters, as we still call them, probably also came from the East. Use has made these characters so familiar that we seldom think what arithmetic would be without them; but we recommend the reader to find for himself how much they facilitate calculation, by trying to go through a sum using only the Roman numerals I., II., III., IV., etc. The quadrature of the circle engaged the attention of the Arabian geometers, and their calculation made the ratio of the circumference to the diameter to be 3,927 to 1,250, which is a very close approximation.

Among the Arabians most distinguished in science we must not forget to name ALHAZEN, astronomer and mathematician, who flourished in Spain during the eleventh century. He is best known by a treatise on optics, containing some original and refined applications of geometry. He treats of the refraction of light and the cause of twilight. As an explanation of the well-known fact of the sun and moon appearing (for it is an appearance only) much larger when near the horizon than when high up in the sky, Alhazen suggests that our judgment is the result of a comparison made between the terrestrial objects such as trees, buildings, etc., which are visible on the horizon near the luminary; but when the latter is high up in the sky we think it looks smaller because we unconsciously judge it to be more distant.

Astrology was not the only pretended science sedulously cultivated by the Arabians. They were great students of alchemy, a science which professed to investigate methods of converting or transmuting common metals into silver or gold. Innumerable trials and experiments made with a view to this illusory object were the means by which many entirely new and valuable truths of chemistry were first acquired. It is by the Arabians that we first find recorded some of the most important discoveries in the domain of chemistry, and our history of this branch of knowledge may not improperly commence with the name of the Arabian alchemist GEBER (*c.* 800), who must not be confounded with the mathematician of the same name already mentioned. Of course a vast number of facts regarding the reduction of metals, glass-making, preparation of drugs, etc., had been known and acted upon long before chemistry as a science had any existence. But it is only when men have learnt to read the laws of a science in such processes, or in such facts, that these can be considered as belonging to the science in question.

Geber was not only a famous alchemist, but he has a fair claim to be considered the founder of chemistry. Little appears to be known with certainty as to the place of his birth, his life, or the precise epoch at which he flourished. His name, and a few of his numerous writings, are all that have reached our times. He was undoubtedly the author of important discoveries in chemistry, and he was appealed to as an authority for centuries afterwards by those who cultivated this branch of science. Geber is the first chemist who described a method of preparing *nitric acid*, a powerful reagent which is now in constant employment in every chemical laboratory. In his works the first indications are met with of some knowledge of the part played by gases in chemical phenomena. Geber also describes certain operations which since his time have been everywhere employed by alchemists and chemists. Such operations are dissolving metals in acids, filtering, evaporating, crystallizing, and distilling; and the chemist of to-day employs these more frequently than any other processes. What is now called crystallization was indeed then termed coagulation, which word was used in a wider signification than with us, and included all cases in which by any process a liquid was converted into a solid.

One interesting case mentioned by Geber of the conversion of a liquid into a solid deserves to be noted here, because nine centuries afterwards the same experiment became almost the starting-point of the fundamental principles of modern chemistry. When quicksilver is maintained with access of air for a considerable length of time at a somewhat high temperature, it is finally converted into a red powder. Geber performed this operation in an open glass vessel with a long neck, and he says that its success depended upon the vessel being left open in order to allow the *humidity to escape.* The true explanation of this experiment had to wait for a discoverer until about a hundred years ago, and the reader will find it again referred to in a subsequent chapter.

A method of preparing caustic alkali from the ashes of plants is given by Geber, and this method is identical with that still in use, which consists in treating the ashes with quicklime. The true explanation of this process also was reserved for a chemist of the seventeenth century, whose labours are referred to on a subsequent page. Nitric acid, which is one of the most important of Geber's discoveries, he obtained by heating in a retort a mixture of saltpetre, alum, and Cyprus vitriol. The condensed vapour formed that powerful solvent long known under the name of *aqua-fortis.* Geber found that by the addition to aqua-fortis of sal-ammoniac a still more potent liquid was produced, for it could dissolve gold, a metal which resisted the action of all other acids. This discovery, as may be imagined, attracted the attention of the alchemists in no small degree. The new solvent was called *aqua-regia,* and the name is still applied to the mixture of nitric and hydrochloric acids.

The writings of other Arabian alchemists and chemists are still extant, and although they are not without interest to those who specially cultivate this branch of science, we may here pass them over without further notice. RHAZES (A.D. 860—940), a famous physician of Baghdad, must, however, be named as the first author to describe the preparation of sulphuric acid, which he obtained by distilling green vitriol (sulphate of iron), a process still used in preparing the strongest kind of sulphuric acid. This writer also gives the earliest directions for the production of spirits (*aqua vitæ*) by distillation, and the production therefrom of alcohol in a still more concentrated state, by another distillation over quicklime. The words alcohol, alkali, alembic, and some other chemical terms, are of Arabian origin, and bear testimony to the past history of the science. The Arabs devoted much attention to the preparation of drugs and medicaments, and many of the preparations which long figured in our pharmacopœias derived their origin from Arabian sources, as some of their names still indicate.

While the Arabs were cultivating science and letters with the zeal and intelligence to which the few details we have given above bear testimony, Southern Europe, from which the barbarian incursions had effaced nearly all the remains of Greek and Roman civilization, was sinking into increasing intellectual darkness. The professors of Christianity had generally despised the classic literature, and altogether rejected science and philosophy. Even the great Pope Gregory could ignore the rules of grammar in his writings, and, it is said, boasted of doing so. Yet to the Latin language the whole learning—and it was but little—of the Middle Ages was confined, while the archives of science remained sealed books, for they existed only in Greek and, as we have seen, in Arabic translations, after the Arabs had become a learned nation. Hence when in the eleventh century GERBERT, a monk of the Low Countries, desired to acquaint himself with mathematics, all Christendom could not furnish an instructor, and the future Sylvester II. had, as we have before intimated, to resort to the Moorish universities of Spain. In the next century the example set by Gerbert was followed by ADHELARD, an English monk, who for this purpose made himself master of the Arabic language, and turned his acquisition into account by translating the Arabic version of Euclid's "Elements" into Latin, his work being the first Latin version of the immortal treatise of the Alexandrian geometer. The translation also

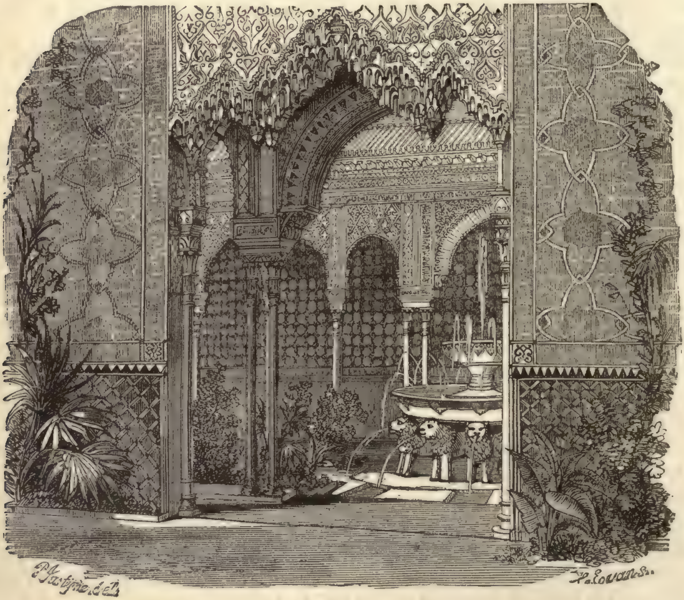
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FIG. 23.—A COURT IN THE ALHAMBRA.

made from the Arabic by CAMPANUS a century later became, however, a more widely-known work, and it was from this that the earlier printed editions were prepared. Many others imitated Adhelard in resorting to the Moorish universities. It is interesting to meet with a number of English names among the cultivators of the mathematical sciences in those days of the almost universal ignorance which prevailed in our own country as elsewhere. We read of DANIEL MORLAY; ROBERT OF READING; WILLIAM SHELL; CLEMENT LANGTOWN; ROBERT, BISHOP OF LINCOLN, surnamed GROSSETÊTE; and other Englishmen.

If these persons themselves did little to add to the store of scientific wealth bequeathed by the Greeks, they at least served to transmit it to later times. And when men's minds began to awaken into intellectual life after the long night of the Dark Ages, it was the study of the ancient authors which gave the first impulse. These works aroused in some minds a curiosity which might long have remained dormant, and inspired for the knowledge of nature an enthusiasm which would otherwise have been wholly expended upon theological subtleties. Wisely studied, these writings might have directed research and enlightened investigation, by showing what had already been accomplished, and by illustrating methods which might have led to fresh acquisitions. As a matter of fact, however, we find that during the Middle Ages science made little or no progress. The minds of the few who cultivated learning were in general occupied with other matters, and the number of those who devoted themselves to the study of nature was extremely small; but in some extenuation of the many charges which have been brought against mediæval ecclesiasticism, it should be observed that many of those who first specially directed their attention to scientific studies in Christian Europe were monks or bishops. Indeed, it was only among persons of this class that any literary culture could be found, and before the eleventh century the monasteries were the only schools of such literature and science as Christendom possessed. The twelfth century witnessed the establishment of Christian colleges and universities; but these institutions cannot be considered to have done anything to promote the progress of science. The science studies pursued in them aimed at nothing higher than an acquaintance with the writings of Aristotle, and the most ambitious aim of their professors was to comment upon the text of the old Greek. This remark applies to collegiate institutions and universities down to a comparatively recent period. Theology, metaphysics, or frivolous disputes, which really turned upon verbal distinctions, exhausted all the energies and employed all the ingenuity of teachers and students. What new discoveries could be expected from men trained in such schools?

Some illustrious examples, however, are to be noticed of students of natural science, so far superior to the narrow bigotry and ignorant prejudices of their age, that they seemed as if "born out of due time"—the harbingers of a brighter day in advance of their epoch. Among the best known of these is the Englishman, ROGER BACON, born at Ilchester in Somersetshire in 1214. He studied at the University of Paris, where he took the degree of doctor in the faculty of theology. His love of knowledge induced him to master Latin, Greek, Hebrew, and Arabic, and to peruse a multitude of books written in those languages. Gifted with a genius worthy of a better epoch, he perceived that no real progress in science would be reached by the path then followed, and he proposed to repudiate the authority of Aristotle, and appeal to nature by experiments. He contended also for mathematics as the first and foremost of the sciences, as that only which could guide the interpretation of experiments. This being in the period when the scholastic philosophy was at its acme, and Aristotle ruled the schools with an absolute despotism, Roger Bacon's revolt raised against him a host of adversaries. The most bitter were the brethren of the Franciscan Order, which he had joined, hoping probably in the tranquillity of the cloister to pursue his chosen studies more freely than in the bustle of the world. This association proved, however, the bane of his life, for he was accused by his superiors of magic and dealings with Satan, and in a general chapter he was condemned, and forbidden to write. On renewed charges of this kind he was more than once committed to prison, where he finally remained for ten years. When released, at an advanced age and with shattered health, he continued his writing, and died peaceably at Oxford in 1292.

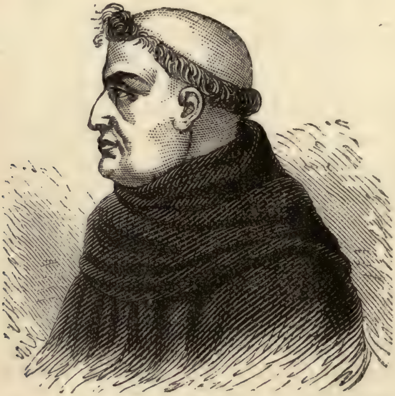

FIG. 24.—ROGER BACON.

Roger Bacon's studies appear to have embraced a wide field. He was well acquainted with mathematics, and was versed in the theoretical mechanics known in his time. His attainments in astronomy were far superior to those of his contemporaries. His inventions prove his skill in practical mechanics and in chemistry, and many of the schemes he proposed have since been realized. The composition of gunpowder was known to him, although he does not give the details of its preparation practically. The principle of the diving-bell is clearly described in his works, and many automatic machines were projected or constructed by him. It is said that he constructed a brazen head which would speak and answer questions addressed to it. This is an acoustical curiosity easy enough to understand now, but in Friar Bacon's time it was naturally regarded as proof of the magical power of its fabricator.

Bacon wrote many treatises, and the chief of these are comprised in his book, first published, under the title of "*Opus Majus*," in 1266, and first printed in London in 1733. This work is remarkable for the great width of its views and well-grounded observations on various optical subjects, such as astronomical refraction, the apparent size of objects, and the peculiar appearance of the sun and moon when upon the horizon. These subjects were treated, as the reader is already aware, by Alhazen, and his works are quoted by Bacon, as is also a treatise by Ptolemy on optics which is not now extant. Bacon discusses various optical questions, such as the foci of spherical mirrors, the round image formed by rays of the sun passing through a small opening of any shape whatever, the nature of vision, etc.· but although in some cases he seems to be approaching the true explanation, he usually falls short of it. Much discussion has arisen on the optical discoveries of Roger Bacon, some having claimed for him the merit of being the inventor of the telescope, while others deny that he had any practical acquaintance with even the simple lens. He does, indeed mention that a segment of a sphere of glass would magnify letters on which it might be placed with the plane side downwards, so as to have its convexity towards the eye. This, however, is only what Alhazen had previously stated in his work on optics, observing also that the larger the segment of the sphere the greater the magnification. Bacon, on the other hand, expressly states that the smaller the spherical segment, the better will the letters be seen and the larger will they appear. This being contrary to fact, would seem to prove that Bacon could not have made the trial; and when, immediately after the passage here referred to, he states that a flat piece of glass will produce the same effect, it becomes more difficult to believe that he had made any experiments on these effects. The passage which has been interpreted as referring to the telescope does not in reality specially describe such an invention, but it is interesting for its foresight of the effects which have since been realized by such optical instruments as the telescope, the microscope, the multiplying-glass, etc., all depending upon the principle of refraction. "We can so shape transparent bodies, and so place them with respect to our eyes and the objects, that the rays shall be bent and turned in any direction we desire, and under whatever angle we please, and so that we shall see an object either at a distance or close at hand; and thus we may from an incredible distance read the smallest letters and count the grains of sand, etc. Thus, a boy may appear a giant, and a man seem a mountain, and a small army appear very great. So too may we make the sun and the moon apparently descend to this lower world, and show themselves upon the heads of our enemies."

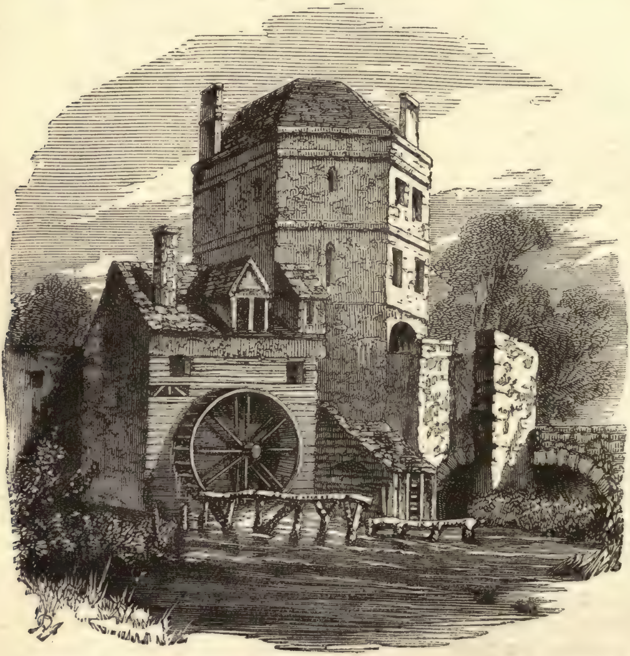
FIG. 25.—ROGER BACON'S HOUSE AT OXFORD.

Besides the "*Opus Majus*," there exist other writings of Roger Bacon, such as the "*Opus Minus*," the "*Opus Tertium*," and a treatise on the *Calendar*, containing astronomical tables. This last work gives proof that Bacon was aware of the error which had accumulated in the Julian Calendar in his time; and he even proposed the same means of rectifying it which were applied under Gregory. For all this, Roger Bacon was a believer in astrology,—at least, so far as to attribute to the configuration of the stars an influence on the temperaments of men, and on the action of medicines. The weather he held also to be under planetary influence. The popular weather lore still retains traces of the pretended science of the astrologers; and perhaps there are also not a few of the less instructed classes, who at the present day believe that certain almanac-compilers can so read the stars as to foretell the future of human affairs. The influence of the planets on the weather has, in common with the other pretensions of astrology, been everywhere derided by men of science; but apart from all the absurdities of astrology, it is curious that quite lately coincidences have been traced between certain periodical positions of the principal planets, disturbances of the solar atmosphere, and certain meteorological conditions of our own atmosphere. Roger Bacon appears also to have admitted the possibility of transmuting base metals into gold; and he wisely counsels those who desire to accomplish this to first study the method by which nature has produced metals, and then to imitate it by their art. Yet among his works we find one directed against the pretensions of magical arts ("*De Nullitate Magiæ*"). Bacon wrote also on various philosophical and metaphysical subjects, and in the list of his acquirements must be included a considerable skill in medicine.

The Emperor Frederic II. caused the "Almagest" of Ptolemy to be translated into Latin from the Arabic, for Greek was still unknown in the West. But it was in ALPHONSE X., King of Castile, that astronomy found a patron whose name will ever retain a place in the history of that science. The zeal which he displayed in its cultivation leads us to suppose that he himself must have been well versed in the science. He spared no trouble or expense in its promotion, for he brought the most accomplished Christian, Jewish, and Arabian astronomers from all parts of Europe, and provided for them in Toledo on the most magnificent scale. Here they conferred together on the defects of the older theories which were found to disagree with the results of observation. After the labour of four years the tables were compiled and published, which became famous under the name of the Alphonsine Tables (A.D. 1250). Although these tables cost the King a sum of money which would have served for a prince's ransom, they proved faulty, owing to some erroneous assumptions of the compilers. These consisted in giving to the fixed stars an unequal movement, to represent which periods of 7,000 and 49,000 years were assigned to certain motions. In the choice of these numbers we may trace the influence of the Jewish astronomers, for whom the numbers 7 and 49 had a mystical significance. The extremely complicated hypotheses which were required to account for the movement of the celestial bodies caused Alphonse to make the somewhat irreverent but significant remark, that had God at the creation of the world taken him into His counsels, things would have been arranged in a simpler and better way. In a revised edition of the Alphonsine Tables many corrections were made by its compilers; and their remaining defects should be imputed rather to the age than to any want of skill or industry on the part of the compilers. Yet the comparatively small superiority evinced by these tables over those drawn up by Ptolemy, shows that during the eleven centuries between their respective dates no very great advance had been made in astronomy.

We find the thirteenth century of our era comparatively poor in men of scientific genius, but characterized by at least one invention which has proved of the very highest importance to the progress of science. The invention is that of spectacles or glass lenses, which can be traced with distinctness no further back than the end of the above-mentioned century. Possibly the writings of Roger Bacon may have led to the actual production of segments of glass spheres in order to effect the enlargement of objects, so clearly indicated in the extract we have given from Roger Bacon's works. Much learned discussion has taken place upon allusions in ancient authors to the magnifying power of glass globes filled with water, etc., but the use of glass lenses to aid the vision seems to date only from about the time above stated. Italy appears entitled to boast of the honour of this invention. In the discourse of an Italian friar, preached in 1305, the hearers are reminded that spectacles had been invented only twenty years before, and that they are an admirable contrivance.

About the same period there came into use an invention destined to exercise a remarkable influence on the progress of knowledge, by so enlarging the resources of navigators that ships began to venture on voyages to greater and greater distances, until at length the passage to India by doubling the Cape of Good Hope was found, and Columbus discovered the New World. We need hardly say that we here allude to the Mariner's Compass, which is almost too well known to require any description. It consists essentially of a slender bar of steel, which, after having been drawn several times along a loadstone, is poised on a pivot on which it can turn freely in the horizontal plane. It will rest only in a definite direction, which, for the present, may be described as pointing nearly north and south. The power possessed by the loadstone of attracting iron was known in ancient times, as proved by allu-

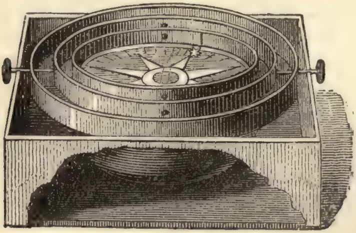

FIG. 26.—MARINER'S COMPASS.

sions in classical authors. The directive power of the magnet is, however, never alluded to by them. The Chinese chroniclers describe, nevertheless, the use of magnets at an extremely remote period, when they were employed on land to indicate the cardinal points, and serve as guides across some of the vast plains of Tartary. Even in the third century of our era, according to Humboldt, Chinese ships navigated the Indian Ocean by the guidance of the magnetic needle. There are indications that the directive power of the magnet was known to the Arabs; but the first reference to its use in Christian Europe occurs in a poem by Guyot of Provence, in 1190. In a book published in the earlier part of the thirteenth century occurs this passage: "The iron needle, after contact with the loadstone, constantly turns to the North Star, which, as the axis of the firmament, remains immovable whilst the others revolve, and hence it is essentially necessary to those navigating the ocean." A curious passage occurs in another author, writing in the latter half of the thirteenth century, with regard to the use of the magnet at sea. He says: "No master mariner dares to use it, lest he should be suspected of being a magician; nor would the sailors venture to go to sea under the command of a man using an instrument which so much appeared to be under the influence of the powers below." It is generally agreed, however, that the first person who constructed the compass in such a form as to render it practically useful to the seaman was a certain FLAVIO GIOIA, of Amalfi, near Naples, who lived at the end of the thirteenth or the beginning of the fourteenth century. Some use had doubtless been previously made of a ruder compass, in which the magnet was merely floated by aid of some light body on the surface of water; but Gioia poised it on a pivot, and, it is said, indicated its north-seeking extremity with the fleur-de-lys (which still everywhere figures on the mariner's compass), in honour of the prince then reigning in Naples, who was connected with the royal family of France.

The fourteenth century was marked by an increasing attention to science and learning, and among other names we meet with those of many Englishmen who cultivated mathematics and astronomy, the latter generally qualified by more or less acquaintance with astrology. We need not here particularize these pioneers of restored science further than to mention in passing RICHARD WALLINGFORD, Abbot of St. Albans, who constructed the first clock with wheels of which we have any distinct account. It showed the hours of the day, the apparent motion of the sun, the changes of the moon, the times of the tides, and other things. This was in 1326, in the reign of Edward I., and the clock, which was placed in the Abbey of St. Albans, was going in the time of Henry VIII., when Leland says of it "that all Europe could not produce such another."

Barren as we find the Dark Ages in scientific discoveries, there are a few useful or interesting inventions which must be referred to this benighted period. The windmill, of which ancient authors make no mention, must have been contrived within this period. The organ dates from the eighth or the ninth century; and the art of making paper from rags began to be generally practised, by aid of mechanical appliances, about the fourteenth century. But, as already pointed out, the progress made in any art is no evidence of an advance in the knowledge of the scientific principles by which the practice of that art may be explained. That admirable system of architecture which so grandly unites beauty of form to constructive skill arose in this period,

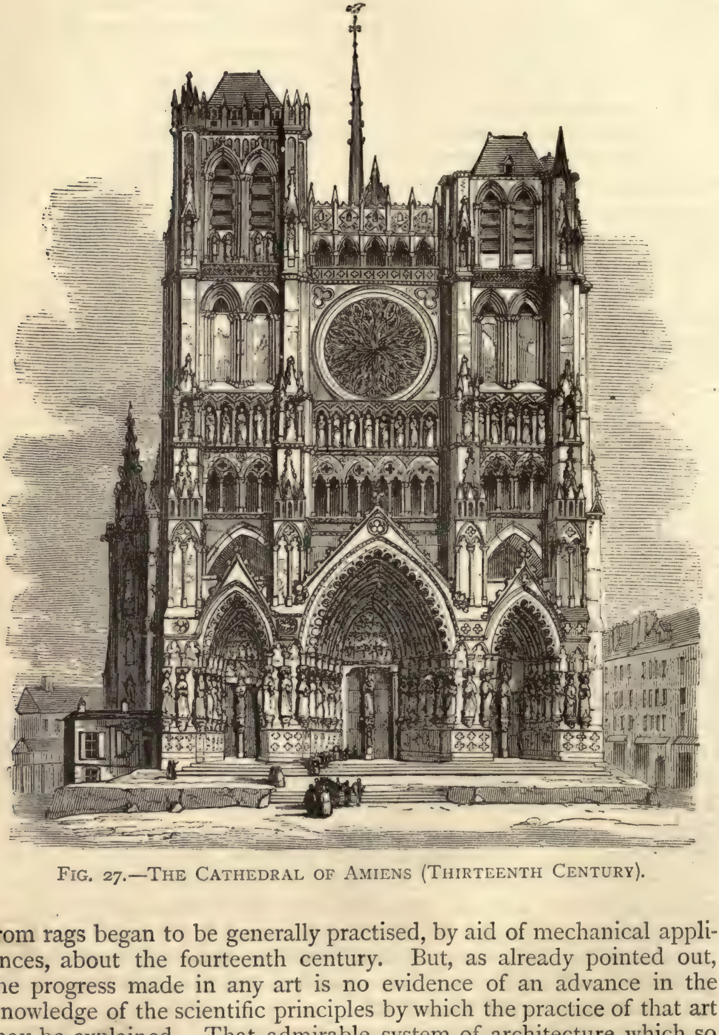

FIG. 27.—THE CATHEDRAL OF AMIENS (THIRTEENTH CENTURY).

and many of its finest monuments date earlier than the fifteenth century. But we must not suppose that the constructive skill displayed in the grand Gothic edifices was a consequence of a knowledge of theoretic mechanics, of which science, indeed, the most elementary propositions were not enunciated until several centuries after some of the finest Gothic cathedrals had been built.

The fifteenth century may be almost characterized as showing the dawn of modern science. It is marked by some events which greatly accelerated the advance of science and learning. The art of printing was invented in Germany about the middle of the century; and at the end of it Columbus made his memorable voyage, upon which that of Vasco da Gama round the Cape of Good Hope closely followed. These voyages vastly increased men's knowledge of the earth; and a few years afterwards this knowledge received another extension by the voyage of Magellan, who visited the South Pacific Ocean, circumnavigated the earth, and brought back an account of the stars of the southern heavens. If no great discoveries in physical science date from this century, it may at least be said of some of the men who belonged to it, that they placed themselves on the right road, and prepared the way for the great revival of the succeeding century. Many cultivators of the ancient Greek learning were, by the capture of Constantinople by the Turks in 1482, driven to seek an asylum in Italy; and these people carried with them copies of the masterpieces of Hellenic literature we now possess. In this way the knowledge of the ancient Greek language revived among the learned in Europe; and translations of the works of Euclid, Archimedes, and other Greek mathematicians soon appeared, the newly-invented art of printing giving unprecedented facilities for their dissemination.

Germany for this century is the seat of the most remarkable scientific labours, for we may almost date the true revival of astronomy from the time of GEORGE PURBACH (1423—1461), who was professor of that science at Venice about the year 1450. Purbach proclaimed the importance of observation in astronomy as the only means by which hypotheses could be confirmed or refuted. He measured with greater accuracy than formerly the positions of the fixed stars, and he computed some tables relating to the planetary motions. He introduced some very important improvements into trigonometry, which greatly simplified calculations. Purbach was desirous of giving the world a more accurate translation of Ptolemy's great astronomical treatise, and his own linguistic attainments not including Greek, he was setting about the acquisition of that language when his death occurred. The design entertained by Purbach was executed by his pupil and successor, JOHN MÜLLER (1436—1476), of Königsberg, whose name is better known under the Latinized form of REGIOMONTANUS. To his translations he added a Commentary, containing problems relating to astronomical calculations. He made use of tangents in trigonometry, as the Arabs

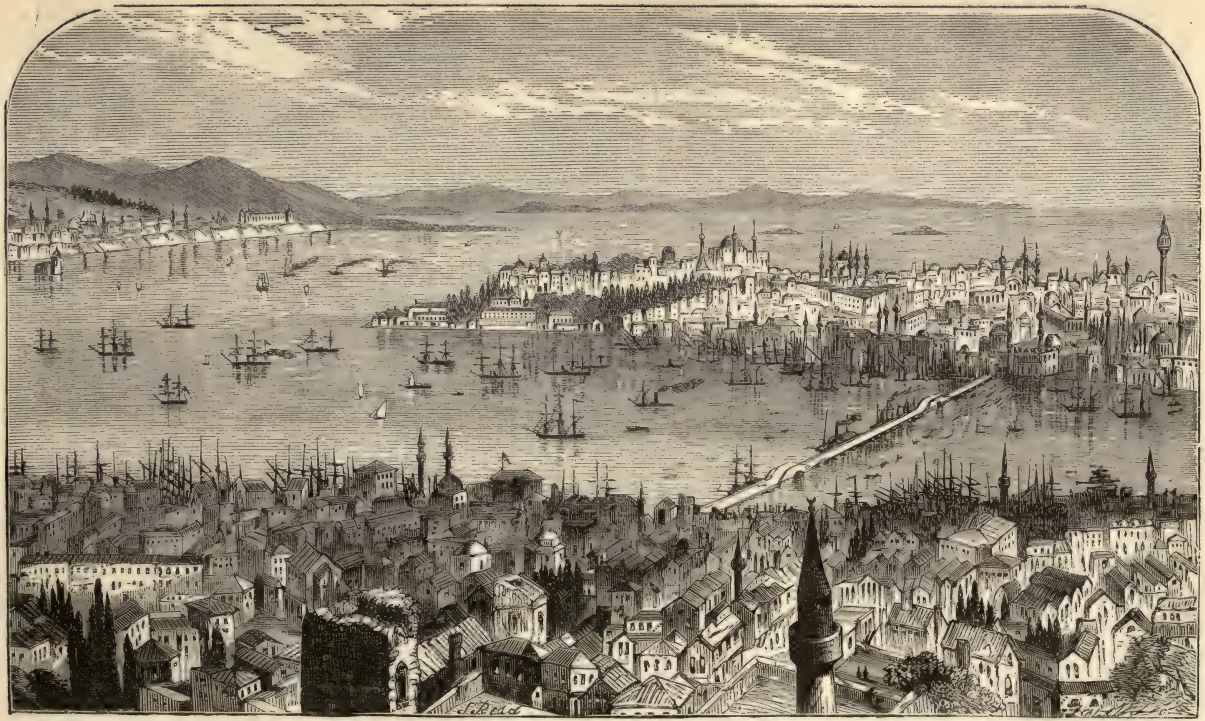

FIG. 28.—THE PRESENT ASPECT OF CONSTANTINOPLE.

had done before, and he was in Europe the first to calculate a table of tangents, in which he gave the numerical value for every degree of the quadrant. He also extended the table of sines calculated by Purbach to every *minute* of the quadrant, and referred them to a radius of 1,000,000 parts, an arrangement which has been found so convenient that it has remained in use to the present day.

One of the earliest and most zealous cultivators of astronomy by observation was certainly BERNARD WALTHER, a rich citizen of Nuremberg, and his observations are the more interesting from having been made with clocks regulated by wheels. Walther, who was unacquainted, in all probability, with the writings of Alhazen, was the first modern astronomer to remark the effects of atmospheric refraction. He seems, however, not to have understood its true principle, for he regards it as affecting the apparent place of a star only when near the horizon.

Throughout the Middle Ages the alchemists attract our attention as the representatives of the more modern race of chemists. The period from the thirteenth to the sixteenth century was the golden age of alchemy, which was then eagerly pursued by ecclesiastics as well as others, and could reckon among its adepts bishops and kings—and even a pope! The pursuit of the two grand objects of alchemy—the transmutation of the common metals into gold, and the discovery of the universal elixir—became with many a passion to which time, money, and health were prodigally sacrificed. Disappointment and failure could not damp the ardour of the alchemist, nor could poverty force him from the pursuit of his illusory objects. His faith in his ultimate success sometimes carried him on to persevere in his labours, even at the cost of his life. Indeed, this wonderful perseverance and sublime patience are the most striking characteristics of the alchemists. Perhaps any object less fascinating or glorious than the discovery of the philosopher's stone and the elixir of life would not have induced men to spend their lives in laborious experiments. It was doubtless through these alchemical experiments that truths were acquired which might for ages have remained unknown. The alchemists made innumerable experiments with metals, and with sulphur, nitre, etc., by which they succeeded in obtaining, not the object of their search, but a knowledge of many important and valuable properties of various substances. It may perhaps appear to the reader a matter for surprise that alchemy should have been by so many eagerly pursued for so many ages, and result in discoveries so few in comparison with those which a single generation of modern chemists may bring to light. The illusory nature of the objects pursued by the alchemists must, however, be borne in mind, and also the fact that the true methods of chemical science had yet to be discovered. There was always a great amount of mysticism mixed up with alchemy, and this was especially the case in those ages when the notion of the supernatural predominated in men's minds.

We have omitted before the mention of one of the most learned men of the thirteenth century, ALBERTUS MAGNUS, Bishop of Ratisbon, whose many works include a famous treatise on alchemy. His acquirements in arithmetic, geometry, astronomy, and music, on all of which subjects he left treatises, entitle him to be regarded as one of the chief precursors of the revival of science. Like Gerbert, Roger Bacon, and others, he also was reputed a magician. Among the most celebrated of the alchemists of the Middle Ages were RAYMOND LULLY (thirteenth century) and NICHOLAS FLAMMEL (1406—1490). We may pass over the details of their labours, and of the new facts with which they enriched chemical science. Another fifteenth century alchemist, BERNARD OF TRÉVISE, has left among his other writings some personal details regarding his pursuits, so curious that we are tempted to transcribe a few passages as an illustration of the life of an alchemist.

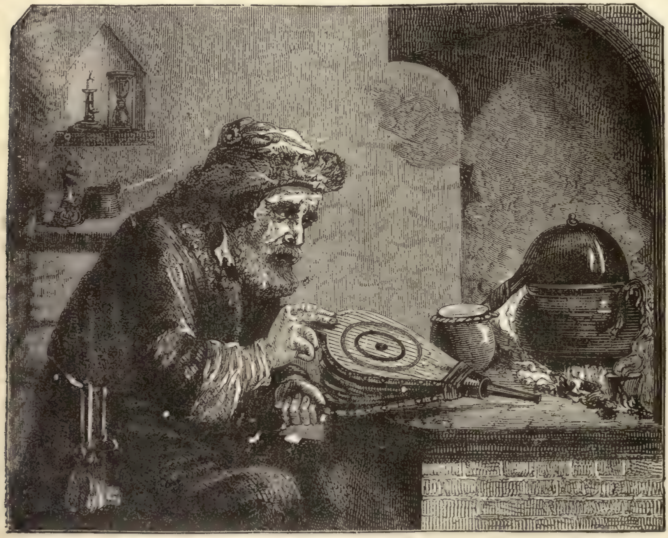

FIG. 29.—THE ALCHEMIST.

"The first book I had was Rhazes. I passed four years of my life and expended eight hundred crowns in trying his experiments. Then I had Geber, who cost me quite two thousand crowns. I passed twelve or fifteen years in this way, spending much and finding nothing; but trying receipts without end; dissolving and crystallizing all kinds of salts, such as common salt, ammoniacal salt, and metallic salts; hundreds of times dissolving, coagulating, and calcining alums, copperas, and all manner of animal and vegetable matters, such as blood, eggs, etc.; and effecting separations of elements by alembics and pelicans, by decoction, reverberation, ascension, descension, fusion, ignition, elementation, rectification, evaporation, conjunction, elevation, sublimation, and endless other sophistical rules. I was occupied in these operations upwards of twelve years, so that I was then thirty-eight years of age, still seeking to extract mercury from plants and animals; whilst I had spent about six thousand crowns." Twenty years more were passed in calcining egg-shells, treating copperas with vinegar, dissolving silver in aqua-fortis, and so on, without any result. He tells us that his health suffered from his application to the laboratory, so that he became at length unable to eat or to drink, and "so thin and haggard that everybody thought I was poisoned; and I was now more than fifty-eight years of age!" After that he began to travel, in order to see if he could not yet find the philosopher's stone in some remote corner of the world. "We saw no end of whitenings and reddenings, receipts and sophistications, in ever so many countries; in Rome, Navarre, Scotland, Turkey, Greece, Alexandria, Barbary, Persia, Messina, Rhodes, France, Spain, the Holy Land, and the neighbouring countries; and throughout Italy, Germany, England, and almost everywhere in the world. But everywhere we found people who required sophistical things, animal and vegetable matters, minerals, etc.; and never did we find them working with the right materials. In these

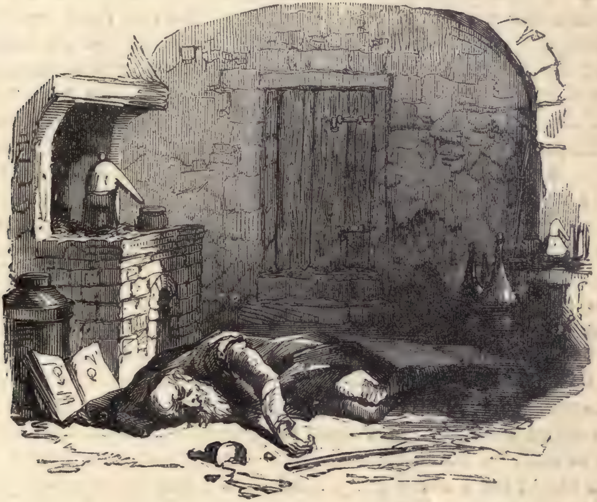

FIG. 30.—ARS LONGA, VITA BREVIS.

affairs I spent upwards of ten thousand three hundred crowns in mere experiments, and, in fact, I was reduced to such poverty that I had but little money left; and yet I was more than sixty-two years of age. I was obliged to leave my native country, and trusting in God's mercy, which is never wanting to those who work with a good will, I withdrew for privacy to Rhodes, and there still sought to find something that might bring me comfort." Bernard, however, soon met with "*un grand clerc et religieux*," who influenced him to spend more time and money in this endless search. This attempt occupied him for three years, and cost him five hundred crowns. "In this way," he says, "all was lost." Yet once again did he devote himself to the study of nature and the perusal of the old authors, and this time, he declares, the attempt was crowned with complete success. He at length discovered the secret of the philosopher's stone! It was concealed in a maxim often quoted by the masters of the mysterious art: "*Nature s'esjouit de sa nature, et nature contient nature.*" This, interpreted into plain English, would mean *You can make gold only out of gold.* The latent satire of Bernard's account of his alchemical researches is perceptible in the quaint manner he fulfils his promise to the reader of revealing the whole secret of the production of gold. He tells a long story of a king who went to bathe in a fountain which was reserved for royalty alone. As the king divests himself of his clothing, the robe is given to Saturn, who keeps it for forty days; the vest to Jupiter, who keeps it for twenty days, etc., etc. "It was an aged priest," says our author, "who told me all these particulars about the king's fountain. I said to him, 'What is the use of this?' And he said to me, 'God made one and ten, a hundred and a thousand, and two hundred thousand, and then multiplied the whole by ten.' And I said to him, 'I do not understand.' And he said to me, 'I will tell you no more, for I am tired.' And then I saw he was weary, and I was very sleepy myself!"

A German alchemist, belonging to the fifteenth century, is **Eck of Salzbach**, who first experimentally demonstrated a fact which afterwards became important in the development of chemical theories. It is, that when a metal is *calcined*, the weight of the *calx* is greater than that of the metal. The non-chemical reader may at once understand the import of these terms by a familiar instance. He has, doubtless, some time or other, seen lead melted in an iron pot or ladle placed on the fire, and probably he has noticed that the surface of the fused metal becomes speedily covered by a dull film. When this film is skimmed off, the silver-bright surface of the lead is seen only for an instant, for the freshly-exposed surface is again quickly overspread by the film, and thus if the films be successively removed, the whole of the lead may be converted into a dull un-metallic-looking substance. This is termed in the language of the old chemistry a *calx*, and the fact of its weighing more than the original metal would not be difficult to prove. Eck's experiments related to the calcination of quicksilver, of which he states six pounds heated for eight days gained in weight three pounds. This increase was largely over-estimated by Eck, although he clearly proved the fact of the augmentation, which he explained by supposing that in the calcination a spirit (*spiritus*) is united to the body of the metal, and he mentions as proof of this, that when the calx is submitted to distillation a spirit is disengaged. We shall show that more than three centuries after these observations of the German alchemist were made, the the same experiments became in the hands of Priestley and Lavoisier the starting-point of modern chemistry.

The writings of BASIL VALENTINE, also a German of the fifteenth century, contain the earliest records of several important chemical preparations and operations. He first made known the properties of the metal antimony; the preparation of "*spirits of salt*" (*i.e.*, hydrochloric acid) by the distillation of common salt with green vitriol (*i.e.*, sulphate of iron); the extraction of metals in the "*wet way*," as when a piece of iron is immersed in a solution of copper; the distillation from beer of spirits of wine, and the concentration of the spirit; and, most remarkable of all, the discovery of ether by distillation of oil of vitriol with spirits of wine.

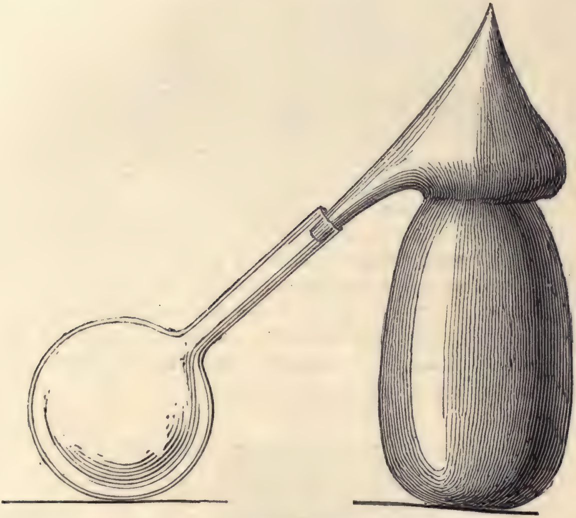

FIG. 31.—ALEMBIC AND RECEIVER.

Many of the most commonly used processes of the modern chemist were practised by the alchemists, who contrived for the purpose, forms of apparatus, which, in modified shapes, may still be seen in the laboratory. For instance, Fig. 31 represents the alchemical apparatus for distillations, called an *alembic*, together with its receiver. The alembic here represented is made in one piece, but not uncommonly the head was movable, as in Fig. 32, where A is the body of the alembic, B the movable head,—sometimes in old books of chemistry called "the Moore's head,"—C the conducting tube ("the nose of the Moore's head"); E the receiver. When the substances to be distilled had been introduced into the body A, the neck of the "Moore's head" was dropped into its place at F, and if necessary, the juncture was sealed by a *lute* of clay or other substance. At the conclusion of the operation the head was separated, and thus the residuum in the body A could easily be removed, as well as any sublimed solids which might have condensed in the head B, while the receiver E would contain the volatile liquids.

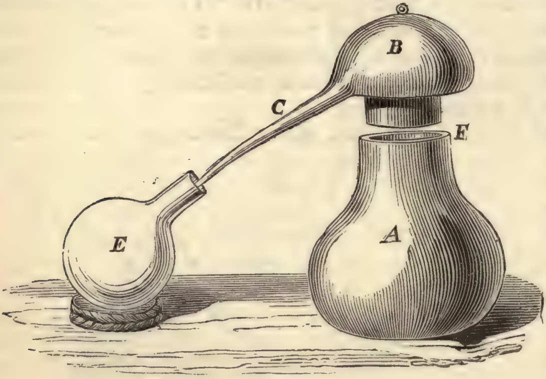

FIG. 32.

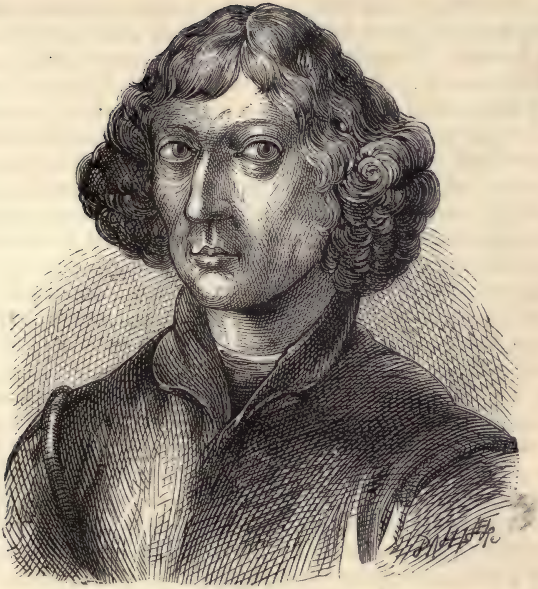

FIG. 33.—COPERNICUS.

## CHAPTER IV.

### SCIENCE OF THE SIXTEENTH CENTURY.

THE sixteenth century is one of the most notable periods in the history of science. It is the period in which astronomy, after remaining for hundreds of years barren of great discoveries, acquired new life by the Copernican reversion to the theories of Pythagoras and Aristarchus; after which an advance of unparalleled rapidity and brilliancy was effected by the labours of three remarkable men, all living during some part of the sixteenth century. This century saw the foundation of modern physics laid by Galileo's mechanical investigations; it saw also the origin of the prolific sciences of electricity and magnetism in the experiments of Gilbert; and its close found Francis Bacon preparing his great treatise on the philosophy of science.

Astronomy, as we have seen, began to be cultivated in Germany in the fifteenth century, and during the sixteenth century that country was the scene of the labours of Copernicus, of Tycho Brahe, and of

Kepler. NICHOLAS KOPERNIK (1473—1543), or COPERNICUS as he is more commonly called by English writers, was a native of Thorn, in Polish Russia. From early youth he manifested a taste for science, especially astronomy. Sent to the university at Cracow, he profited so well by the studies there that he obtained the doctor's degree, and soon afterwards was appointed professor of mathematics at Rome, a position which he retained with great credit for several years. About the beginning of the sixteenth century he left Rome; for having entered the Church, he was appointed by a relative to an ecclesiastical office at Frauenberg in Prussia, where he had leisure to pursue his daily study in peace and comfort. After passing many years observing the motions of the celestial bodies and meditating upon the systems of Ptolemy and the Pythagoreans, he drew up his celebrated treatise entitled "*De Revolutionibus Orbium Celestium*," which completely changed for ever the science of astronomy. The great complexity of the Ptolemaian system, its want of symmetry and order, and the difficulty of conceiving so vast a machine as the sphere of the fixed stars to revolve about the earth with the rapidity which the diurnal motion required, caused Copernicus to carefully compare his own observations with the counter or helio-centric theory which had been advocated by the Pythagoreans. The results convinced him of the falsity of the then accepted theory, and led him to propose that system the truth of which is now so abundantly demonstrated. But for twenty-six years he continued by observation to test this now familiar theory before announcing it to the world, and in fact his work was published only a few days before its author's death in 1543. The Copernican System places the sun motionless in the centre, and makes the earth and the planets revolve about it in the following order:—Nearest the sun is Mercury, and in succession come Venus, the Earth, Mars, Jupiter, and Saturn. The moon only revolves about the earth, with which it is carried round in the annual revolution. Copernicus supposed that the earth revolved in a *circle*, in the plane of which circle the sun was situated, and that the earth also turned on its axis every twenty-four hours, as if it rolled on the convex circumference of its orbit, which would cause the sun to appear to move round the heavens in a year, in a direction contrary to that of its diurnal motion. That the evidence of the senses which attribute movement to the sun and stars is an illusion, Copernicus illustrates by a familiar experience and an apt quotation. "Why then do we hesitate to give to the earth the mobility suitable to its form, rather than admit that the universe, whose bounds we do not and cannot know, should revolve? Why should we not confess that the diurnal revolution is apparent only in the heavens, and real in the earth? Thus Æneas in Virgil exclaims,

> Provehimur portu terræque urbesque recedunt.

While the ship glides tranquilly along all external objects appear to the sailors to move in proportion as their vessel moves, and they alone and what is with them seem to be at rest." The reasoning by which Copernicus supports his views is not however always so sound, for he often uses arguments which are as frivolous as those of the Aristoteleans he undertakes to refute. He attributes to the celestial bodies motion only in perfect circles, and accepts the doctrines about "natural" motion and "violent" motion which Aristotle laid down. Copernicus could not therefore discard the epicycles which figure so largely in the Ptolemaian theory; but compared with that system the Copernican was simplicity itself. Further, all objections which could be urged against his theory by those who accepted the Aristotelean physics, Copernicus refuted by an appeal to the same principles. Thus, he said, if it be true that the velocity of the earth's motion could cause the dispersion into space of the bodies belonging to it, then must the celestial sphere revolving with an infinitely greater velocity be liable in an infinitely greater degree to this dispersion.

Copernicus advanced some speculations on gravity which read almost like an anticipation of Newton's grand idea. The followers of Aristotle observing that bodies on the earth's surface tended to move towards its centre, hastily concluded that this point was the centre towards which every body in the universe was attracted. But this, remarks Copernicus, is very doubtful, for gravity being but the tendency of parts to draw together and coalesce in the form of a globe, "it is probable that such a tendency exists in the sun, moon, and other heavenly bodies; but this does not prevent them from describing their respective orbits. If, then, the earth have other motions, these must be the same as those which other bodies appear to possess." The reasoning of Copernicus on the nature of the motions of the heavenly bodies is however completely Aristotelean. "It is impossible that a single celestial body can move unequally in one orbit; for that must happen either through the inconstancy of the moving power, whether it be extraneous or belonging to its intimate nature, or through a disparity in the body revolving. But both of these suppositions are repugnant to our understandings." But as the planets plainly do not revolve in circles of which the sun is the centre, Copernicus was obliged to have recourse to the ancient hypothesis of epicycles; and this was perhaps the best available one until the discovery of the true form of these orbits by Kepler.

The author of the "Celestial Revolutions" was probably aware that his novel system of the world would encounter much opposition, for he seems to have been anxious to present it in a form as little obnoxious as possible to the adherents of the established theory. There was indeed some difficulty in the choice of the place where the new views were to be given to the world. Rome was avoided, doubtless for good reasons; and the detestation in which Melanchthon (Schwartzerde) held astronomical science induced the friends of Copernicus, to whose care the publication of his work was entrusted, to consider Nuremberg a more suitable place than Wittenberg, where the influence of the Protestant reformers was supreme. Even there it was judged expedient to prefix to the work an apologetic preface, in which it is stated that the new doctrine was put forth not as a fact but only as an hypothesis. In the light of subsequent events it appears not a little remarkable that Copernicus was encouraged by some ecclesiastical friends in the publication of the astronomical doctrines which proved so obnoxious to the theologians of every creed. Among men of science the new system of the world found at first but small recognition. The unquestioned belief of ages had left the earth firmly fixed in the centre of the universe, and it could hardly be moved from its position without the strenuous opposition of the many who in every age are unwilling to accept new truths. The scientific men who had not risen above the dogmatism of the old philosophy brought forward the old absurd arguments, and the theologians made the most positive assertions on a question they did not understand. It was noticed, however, that those who did accept the new doctrine of the universe were astronomers and scientific men of the first order. Amongst others was Mœstlin, who is distinguished for having been the instructor of Kepler, and the first probably who pointed out the true cause of the faint illumination of the moon which is sometimes seen on the dark side of our satellite when in her first quarter, and is then popularly known as "the old moon in the new moon's arms." Some say, however, that the famous painter Leonardo da Vinci (1452—1519) was the first to suggest the real explanation of this appearance. This is not improbable, for Leonardo is known to have been profoundly versed in the science of his day, and as an artist he would be conversant with the phenomena of reflected light, of which the appearance in question is only an instance on the grand scale. Before the time of which we are speaking the illumination of the shaded part of the moon's surface was vaguely attributed to the planet Venus, but now every one admits with Mœstlin that it is due to the light reflected from that part of the earth's surface which directly receives the sun's rays. When this simple explanation was first advanced, a great outcry was raised on the ground that it was contrary to Genesis i. 15. But of the opposition of theologians to science we shall have presently to adduce a more famous example.

Tycho Brahe (1546—1601) was born at Knudstorp, in Denmark, of a distinguished family. His career exhibits strange vicissitudes of fortune, and some of its incidents would give interest to the pages of a romance. While a student at Copenhagen, in his fourteenth year, an event happened which determined the bent of his mind. An eclipse of the sun had been predicted for the 21st August, 1560, and Tycho waited with great eagerness to see whether the time and manner of its occurrence would realize the announcements of the astronomers,—or perhaps we might have better said, of the astrologers, for the publication in which the prediction appeared took occasion to connect this astronomical occurrence with the destinies of princes and of nations. When, on the day and at the very instant predicted, he saw the sun obscured to precisely the degree that had been foretold, the boy was greatly impressed, and he resolved to learn the principles of a science which could disclose future events with such extraordinary correctness. The attraction of this study was doubtless much intensified by the supposed influence of such events on the fortunes of mankind. When he afterwards went to Leipsic to study law, instead of devoting himself to his legal tomes, he nightly followed the stars through the heavens. He secretly applied himself to the study of mathematics, and thus unaided acquired a competent knowledge of astronomical calculations. He was about to leave Leipsic and make the tour of Germany when he received intelligence of his uncle's death, and was recalled to his native country to enter upon the possession of the fortune this relative had bequeathed him. His rich relatives were disgusted with his devotion to astronomical studies, which the Danish nobles pronounced to be a contemptible and useless pursuit. Tycho leaves Copenhagen, travels for four years in Germany, where he fights a duel in which he loses the most prominent feature of his face, but repairs the loss by a metallic imitation. He visits Augsburg, where he causes a quadrant of the enormous radius of fourteen cubits to be made, so that its graduated arc may show distinctly minutes of a degree. Tycho now devotes not a little attention to alchemy, and he greatly occupies himself in attempts to produce the precious metals. But, after his return to Copenhagen, an unexpected event withdraws him from his alembics and crucibles. It is the sudden appearance early in November, 1572, of a new star of extraordinary brightness in the constellation Cassiopeia. Tycho saw it on the 11th of November shining with a brilliancy greater than that of Sirius—nearly equal, indeed, to that of Venus at her brightest. He hastened to his observatory, and with his sextant immediately measured the distances of the new star from the other stars of the constellation. He continued his observations during the whole time the star remained visible, which was only about seventeen months. For the splendour it exhibited when first observed had, by January, 1573, gradually decreased, and its light was then about equal to that of Jupiter; and this decrease continued, the new celestial object becoming comparable to stars of the first magnitude, second magnitude, and so on successively, until in March, 1574, it finally disappeared altogether. The colour of the star changed also: at first white and brilliant as Venus, it became successively yellowish, reddish, bluish. It had no parallax, that is, its position among the fixed stars did not vary with the position of the observer, and it was therefore situated in an immensely distant region of space.

Tycho's observations were all regularly entered in a manuscript, which he was afterwards persuaded to publish, but with some reluctance, for he feared that his dignity as a Danish noble would be compromised by communicating to the vulgar world the nature and results of his studies. No sooner had Tycho published his book than he was seized with a fever, and on his recovery he disgusted his noble relatives more than ever by a *mésalliance* with a peasant-girl of his native place. The King himself had to interfere to bring about a reconciliation between our astronomer and his friends. Tycho was now becoming famous as a mathematician and astronomer, and he was prevailed upon by the King of Denmark to deliver a course of lectures on astronomy. In these he not only fully explained the astronomical science of his time, but unfolded some of the mysteries of astrology. It had by this time become the fashion among the German princes to patronize astronomy, and the reigning King of Denmark bethought himself that Tycho's great genius ought to grace his native country rather than lend fame to foreign courts. He, therefore, sent for Tycho, who was again on his travels, received him with great kindness, and offered to erect for him an observatory provided with astronomical instruments, a chemical laboratory, and a dwelling, all of which should be secured to him for life. Accordingly a small but fertile island in the Baltic was chosen as the site of a building for the sage. The description given of this singular structure strongly reminds one of the castles of romance. There were ramparts, gateways, towers, turrets, deep wells, subterranean passages, and vaulted crypts containing the mysterious apparatus of alchemy. The buildings were of a highly decorated character externally, and the apartments were adorned with statues and portraits of the most famous astronomers from Hipparchus down to Copernicus. There were museums, libraries, workshops for the construction of instruments. The King expended on these buildings £20,000, and Tycho himself laid out upon them a like sum. Tycho was also furnished with scores of the most elaborate instruments that ever astronomer possessed. In short, the structure, from its purposes and contents, well deserved the name Tycho gave it—Uraniburg, *the ~~Tower~~* City *of the Heavens.*

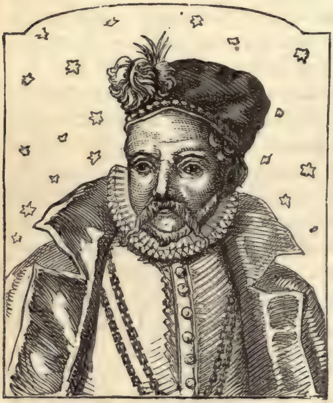

FIG. 34.—TYCHO BRAHE.

Tycho passed at Uraniburg twenty-one years of his life, observing with the greatest care all the phenomena of the heavens, and yet keeping open house for the men of science and rank who came to visit the Danish temple of astronomy, and make the acquaintance of its high priest. As our astronomer had laid out nearly all his own fortune on Uraniburg, the King, his patron, had granted him an annual pension, had given him an estate, and had appointed him to a sinecure office. After enjoying with a thankful heart this long career of unparalleled prosperity, Tycho was overtaken by the Nemesis.

His munificent patron, Frederick II., died, and was succeeded by his son, then only eleven years of age. Although this prince had shown himself favourably disposed towards Tycho, the latter became in a few years painfully sensible that a new king had arisen in Denmark "which knew not Joseph." The Danish nobility had merely shown a courtly acquiescence by professing an interest in astronomy while Frederick II. lived, and he being removed, the envious spirit with which they regarded the liberal endowment of scientific research at Uraniburg, and the fame of its great possessor, had scope to display itself by successfully contriving that Tycho should be deprived of his pension, his estate, and his sinecure office. Though not directly deprived of his island, Tycho was by this blow deprived of the means of maintaining his establishment there; and as he had also to endure other persecutions at the hands of his enemies, he resolved to quit his country. He hired a ship to carry to some more hospitable shore all his astronomical instruments, his alembics and crucibles, and his books. The bark

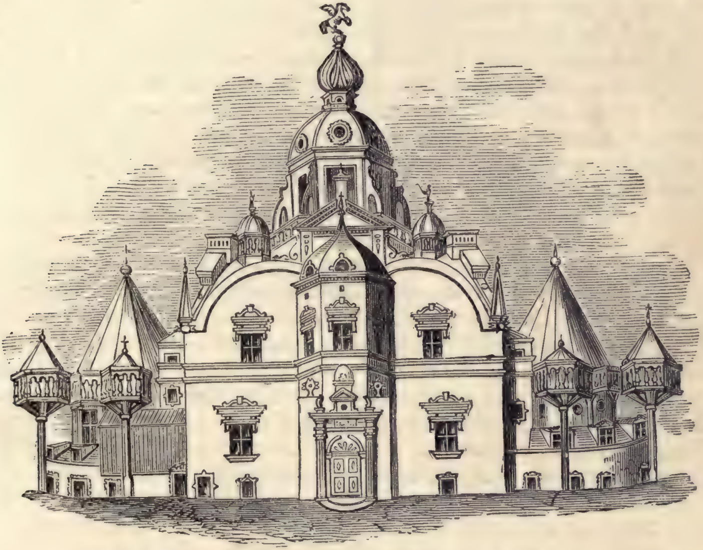

FIG. 35.—TYCHO BRAHE'S HOUSE AND OBSERVATORY.

thus freighted conveyed also Tycho himself, his wife, his five sons, his four daughters, his men-servants, and his women-servants, and landed all safely at Rostock. Surely never before was such an exodus! Rats

It was in 1597, when Tycho was fifty-one years of age, that he thus found himself at Rostock, which place he soon left with all his family, to accept the hospitality of his friend Count Rantzan at the *château* that nobleman possessed near Hamburg. It was here that he completed his work entitled "*Astronomiæ instauratæ Mechanicæ*," and, in order to secure a new patron, he dedicated it to the Emperor Rudolph, who was known to be particularly fond of alchemy, astrology, mechanics, and the sciences. The project was successful. Tycho was invited to Prague, where he was handsomely received by the Emperor (1599), who settled upon him a handsome annual pension, gave him houses for his residence, observatories, and a laboratory. Tycho soon resumed his astronomical observations at Prague; but it was only in the beginning of the year 1601 that he had received his larger instruments from Huen, and that his family had joined him. In October of the same year, however, his life was cut short by a brief illness in the fifty-fifth year of his age.

The collection of instruments which Tycho caused to be constructed, and which he used in his observations, included every contrivance that had been devised for the study of the heavenly bodies; and the improvements which he effected in the construction of those instruments have contributed largely to his reputation. His instruments surpassed in size and workmanship all that had been before constructed. The larger dimensions afforded space for more minute subdivisions of the graduated limbs, and by employing in addition the method of reading small divisions, which is now so well known as the Diagonal Scale, Tycho was enabled to read off his instruments to the three hundred and sixtieth part of a degree, whereas Hipparchus could not come nearer than the sixth part of a degree. Tycho made a catalogue of the fixed stars surpassing in accuracy the catalogues of Ptolemy and of the Arabian astronomers; but though he considered that the errors could not exceed one minute of arc, this accuracy is not found to be maintained throughout the catalogue. Perhaps, however, without the advantages afforded by the telescope and by the use of exact time-keepers, it was hardly possible to attain greater precision. Tycho had tried to improve upon the clepsydra of the Greeks by substituting mercury for the water, and as this did not answer his expectations, he endeavoured to make clocks with wheels which would answer his purpose. The pendulum had not yet been applied to clocks, which at this period were controlled by the oscillations of a weighted bar, which swung horizontally in alternate directions. Tycho had four clocks, which indicated hours, minutes, and seconds. His largest clock had only three wheels, but one of them was 3 feet in diameter, and had 1,200 teeth.

Up to Tycho's time astronomers had entertained very erroneous notions concerning comets, holding those bodies to be merely meteors nearer to us than the moon, and engendered within the earth's atmosphere. Such having been the opinion of Aristotle, it need hardly be said that all the book philosophers since his time held firmly to it. The comet of 1577 gave Tycho the opportunity of learning some facts concerning these bodies; and the results gave a rude shock to the complacency of the Aristoteleans. Tycho found that the comet had no sensible parallax, and this proved that it was situated beyond the region of the moon; he also made an approximate calculation of the comet's orbit, and the proper motion of the wandering planet proved that the heavenly bodies could not possibly be carried round in the solid crystalline spheres, which formed the accredited celestial mechanism of the schools.

The Aristoteleans would not accept these conclusions, and violent opposition was raised, notably by Claramonti, a professor at Pisa, and by a Scotchman named Craig. But as reason and truth were on Tycho's side and could not be overthrown by quotations, his opponents had to resort to personal abuse.

Thus Tycho shivered into atoms and dissipated into air—or rather into vacuous space—the huge orbs of impenetrable crystal which had, in men's conceptions, for so many ages majestically revolved about our earth. It had up to this period been thought necessary to fix each planet on one of these immense transparent spheres, which carried it round in its course; and while these spheres revolved, orb within orb, each in its own circuit, another greater sphere surrounded them bearing the fixed stars; and, external to all, an immense hollow globe called the *primum mobile* rolled itself and the included spheres round the central earth every twenty-four hours.

Tycho did not accept the Copernican system of the world; but, influenced either by religious scruples resting on the presumed interpretation of Scripture current in his day, or by some other motive, he proposed a system of his own, which holds an intermediate position between the Copernican and the Ptolemaic. In this the earth is supposed to be stationary in the centre, the sun and moon revolving daily about it, while the planets revolved about the sun which, in its motion round the earth was supposed to carry all the planets with it. This theory agreed with, or accounted for, the phenomena as well as the Copernican theory did, and could therefore be equally well used as the basis for calculations. It was, however, vastly inferior in simplicity to the theory of Copernicus, for the supposition of so immense a system revolving about a body of dimensions so insignificant as the earth, presents great difficulties and improbabilities. One of the greatest of these was removed by a modification of the Tychonic doctrine, which consisted in admitting a diurnal rotation for the earth while retaining its central position. Neither of these systems, however, found many supporters, and the contest really lay between the Ptolemaic system and the Aristotelean science on the one hand, and the Copernican system and the mechanics of Galileo on the other. The grounds on which Tycho refused to accept the annual motion of the earth have been, for the most part, referred to in our notice of some earlier astronomers. The strongest was the absence of an annual parallax of the fixed stars. That is, the stars remain unchanged in their positions, whereas if the earth revolve about the sun—so Tycho urged—she will at intervals of six months be in places distant from each other by the whole diameter of that orbit, and consequently some change in the relative position of the fixed stars would be apparent, unless the angle which would be formed between two lines drawn from the extremities of the orbit's diameter to the nearest fixed star should include an inappreciable angle between them. But as the diameter of the earth's orbit is of so great a length, Tycho thought this would be impossible. We need hardly tell the reader that we have now incontestable proof of the earth's motion, and that we know, however astonishing the fact may appear, that the earth's orbit is a mere point compared with its distance from the fixed stars.

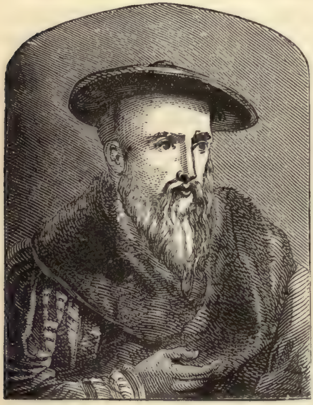

FIG. 36.—KEPLER.

The vast series of accurate observations made by Tycho became a great storehouse of facts from which another illustrious astronomer of a different genius obtained the means of deducing the laws of the planetary motions. When Tycho, in the fifty-fourth year of his age, was carrying on his observations at Prague, he was assisted by this eminent man, of whose own labours we must give a short account.

This was JOHN KEPLER (1571—1630), who at Tycho's death was appointed his successor as principal mathematician to the Emperor. But although the salary attached to the appointment was nominally liberal, it was in reality inadequate to provide Kepler with a livelihood, since the payments were constantly in arrears, owing to the exhaustion of the imperial treasury by the expenses of a war. He resorted to the practice of casting nativities as a means of adding to his resources.

In the year 1609 Kepler published his great work entitled "The New Astronomy, or Commentaries on the Motions of Mars." This remarkable book forms the connecting-link between the astronomical discoveries of Copernicus and those of Newton, and upon it chiefly rests the scientific reputation of its author. The inquiries by which he was led to his discoveries had been entered upon when Kepler first became Tycho's assistant; but it was the observations of the Danish astronomer which furnished the data for the endless calculations made by Kepler in successively testing one theory after another until he had hit upon one according with the facts. He found that it was impossible to represent the observed positions of Mars by any combinations of uniform circular motions, and after testing by calculation every theory which presented itself to his mind, he was led at length to the supposition that the orbit of Mars was an ellipse, having the sun at one of its foci. The same laws, he soon found, applied to the other planets, and hence the first of the three celebrated theorems called Kepler's Laws: *The planets move in elliptical orbits round the sun, which is placed at one of the foci.* The second law which he deduced in the course of this investigation is famous among astronomers as "the Law of Equal Areas." Its purport may easily be understood by any reader with the aid of the diagram Fig. 37. Let the ellipse represent the orbit of a planet—Mars, for example; and let S be the place of the sun in one of the foci of the ellipse. Let $M_1$ be the place of the planet at a given instant, and $M_2$ its place after any given interval of time, as an hour, a day, or a month; and, further, let $M_3$ be the planet's place in its orbit after another equal interval of time; so that at the end of the second hour, day, month, etc., from $M_2$ the planet is passing the position $M_3$. Suppose that a straight line always passing from the centre of the sun to the centre of the planet is carried round by the movement of the planet, like the cord tied to a stone whirled in a circle about the hand. Kepler discovered that in the case of Mars, and of each of the other planets, this line (*the radius vector*) swept out equal areas in equal times. Thus in our diagram the area of the space enclosed by the lines $M_2$ S, S $M_3$, and the arc of the ellipse intercepted between them, would exactly equal the similar area enclosed by $M_2$ S, S $M_2$, and $M_1$ $M_2$.

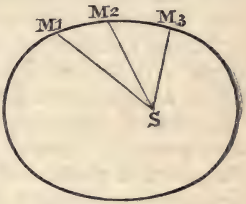

FIG. 37.

Kepler was conducted to brilliant results by a method very different from that by which others have advanced to great discoveries. His reasonings were often vague, and they frequently rested on no other foundations than arbitrary assumptions. But Kepler had much mathematical knowledge, and his perseverence in calculations which would appal most men was something extraordinary. He would assume an hypothesis at random almost, and work out its consequence by laborious calculations. The results thus obtained he then carefully compared with actual observation: if they contradicted it, he abandoned the hypothesis, and tried another—the first that presented itself to his fertile imagination—repeating his calculation on the fresh supposition, however formidable the labour this might involve, and again rejecting the hypothesis which led to results discordant with the facts. Before arriving at the discovery of the true nature of the orbit of Mars, Kepler thus tried no fewer than nineteen other paths. The twentieth guess was right! The appearances were in agreement with the supposition, and Kepler announced that the orbit of Mars was an ellipse, thus overthrowing the ancient prejudice concerning circular motions which had so long checked the progress of astronomical science, and preparing the way for the grand discoveries of Newton.

At length he obtained the arrears of his salary, and in 1613 he was appointed to a professorship of mathematics at Lintz, where he passed sixteen years of his life, and where he published in 1619—a year memorable in the history of science—a work entitled "The Harmonies of the World," containing the announcement of the third of his celebrated laws, that, namely, connecting the distance of a planet from the sun with the period of its revolution. This law is expressed mathematically by saying that the squares of the times of the revolutions are proportional to the cubes of the distances. About the same time Kepler published a work entitled "An Epitome of the Copernican Astronomy." This book contains his opinions and reasonings in astronomy presented in the popular form of question and answer. He puts forward an explanation of the law of periodic times; but of the four suppositions he makes, three are now known to be false. Very curious is an argument he advances against the notion then prevalent that each planet is directed in its movements, or carried round, by an angel. "In that case," says Kepler, "the orbits would be perfectly circular; but the elliptic form which we find in them rather smacks of the lever and material necessity."

BAPTISTA PORTA (1545—1615) was a native of Naples, where he distinguished himself at a very early age by publishing his book on Natural Magic. This work describes a great number of curious observations, experiments, and contrivances, and although many of these are destitute of any scientific value, the whole work nevertheless shows that its author was acquainted with the true principles of science. The book being put forth at the period when the art of printing was affording unprecedented facilities for the dissemination of knowledge, became very popular, and was soon translated into the principal European languages. Its wide circulation may also be taken as an evidence of an awakening interest in all things pertaining to the truths of nature. Porta also gathered round himself at Naples a circle of personal friends devoted to pursuits like his own, and this little circle of curious and learned persons constituted itself into a scientific society meeting to discuss and hear the accounts of new experiments. These proceedings were regarded by the ecclesiastics with no little jealousy, but Porta and his associates were too circumspect to afford the Church any pretence for persecution.

Porta's name must be associated with the invention of the *camera* *obscura*, an instrument which has in recent times become of the greatest utility in practical art and scientific research. He describes how, by making a small aperture in the window-shutter, the images of external objects can be made to appear on the opposite wall of a dark chamber. In this case the image is inverted, because the rays, entering the aperture from the lower parts of an object, proceed in straight lines towards the upper part of the wall or screen, and the rays from the upper parts, also passing through the opening, cross the former so as to come below them. But as the opening, however small, admits not one but a whole group of rays from each external point of the object, the outlines of the image are more or less blurred by overlapping of the images of the adjoining parts of the object. This defect is less as the aperture is smaller, but the brightness of the image is at the same time diminished. The use of a convex lens fitted into a larger opening was a subsequent improvement, for more light is thus admitted, while the rays cross nearly at a point, namely, at the focus within the room. Porta found that transparent pictures, placed in front of the lens outside, formed their images on the wall of the darkened chamber. This is precisely the principle of the magic lantern, and it is somewhat singular that Porta did not think of trying to illuminate his pictures by artificial light. This was not done until many years after by a German named Kircher, who thus invented the magic lantern. Porta had much to say about lenses and mirrors, and in certain passages in his book he would almost appear to anticipate the actual construction of the telescope. "I shall now endeavour," he says, "to show in what manner we may contrive to recognize our friends at the distance of several miles, and how those of weak sight may read the most minute letters from a distance. It is an invention of great utility and grounded on optical principles, nor is it at all difficult of execution; but it must be so divulged as not to be understood by the vulgar, and yet be clear to the sharp-sighted." The description which follows is so obscure that even the sharp-sighted have been unable to make out its meaning, and every writer who has quoted it has been obliged to give the passage in the original Latin. If the passage have any meaning, it is a kind of reflecting telescope which is indicated. But there is another passage in Porta's book in which the use of a concave mirror for magnifying minute objects is clearly described, and the arrangement proposed is in principle the same as adopted by Newton in his reflecting telescope; that is, the image formed by the concave mirror is reflected to the eye by means of a plane mirror. The objects viewed in Porta's arrangements are, in this case, however, not distant objects, but minute characters, the plan for magnifying them being thus described: "Place a concave mirror so that the back of it may lie against your breast; opposite to it, and within the focus, place the writing; put a plane mirror behind it that may be under your eye. Then the images of the letters which are in the concave mirror, and which the concave has magnified, will be reflected in the plane mirror, so that you may read without difficulty." When, a few years before Porta's death, the telescope had become widely known as Galileo's Tube, Porta claimed the invention as his own, and much discussion took place as to the validity of this claim, which is, however, now admitted to rest on an insufficient basis.

Contemporaneous with Porta there lived at Colchester an English physician whose name is memorable as the founder of the sciences of Magnetism and Electricity. Dr. William Gilbert (1540—1603) published in the year 1600, under the title "*De Arte Magnetica*," an admirable treatise in which the facts of magnetism are investigated and its general laws are for the first time enunciated. This work is remarkable also because it is a fine example of inductive reasoning, although the great treatise of Francis Bacon, in which the methods of inductive science were first laid down in a systematic form, did not appear until many years after Gilbert's time. Galileo was acquainted with Gilbert's treatise, and he says of it, "I extremely admire and envy this author. I think him worthy of the greatest praise for the many new and true observations that he has made, to the disgrace of so many vain and fabling authors, who write not from their own knowledge only, but repeat everything they hear from the foolish vulgar, without attempting to satisfy themselves of the same by experience—perhaps that they may not diminish the size of their books."

Gilbert, after completely discussing the phenomenon of the loadstone, proceeds to examine the apparently analogous fact of amber attracting light bodies after it has been rubbed. From the time of Thales to that of Gilbert this remained almost the only electrical phenomenon known to philosophers, the exceptions being some very few, and perhaps doubtful, additions to the list of bodies possessing the like property. The explanation of Thales, that amber has a breath which draws light bodies, or the theory of Pliny, that by friction the amber acquires warmth and life, appear in ancient and mediæval times to have satisfied men's curiosity on this subject. Amber and jet—both costly substances, from their comparative rarity and the demand for them as material for ornaments—were before Gilbert's time the only

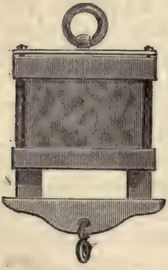

FIG. 38.

FIG. 39.

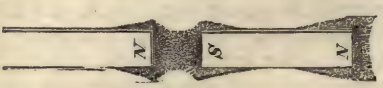

FIG. 40.

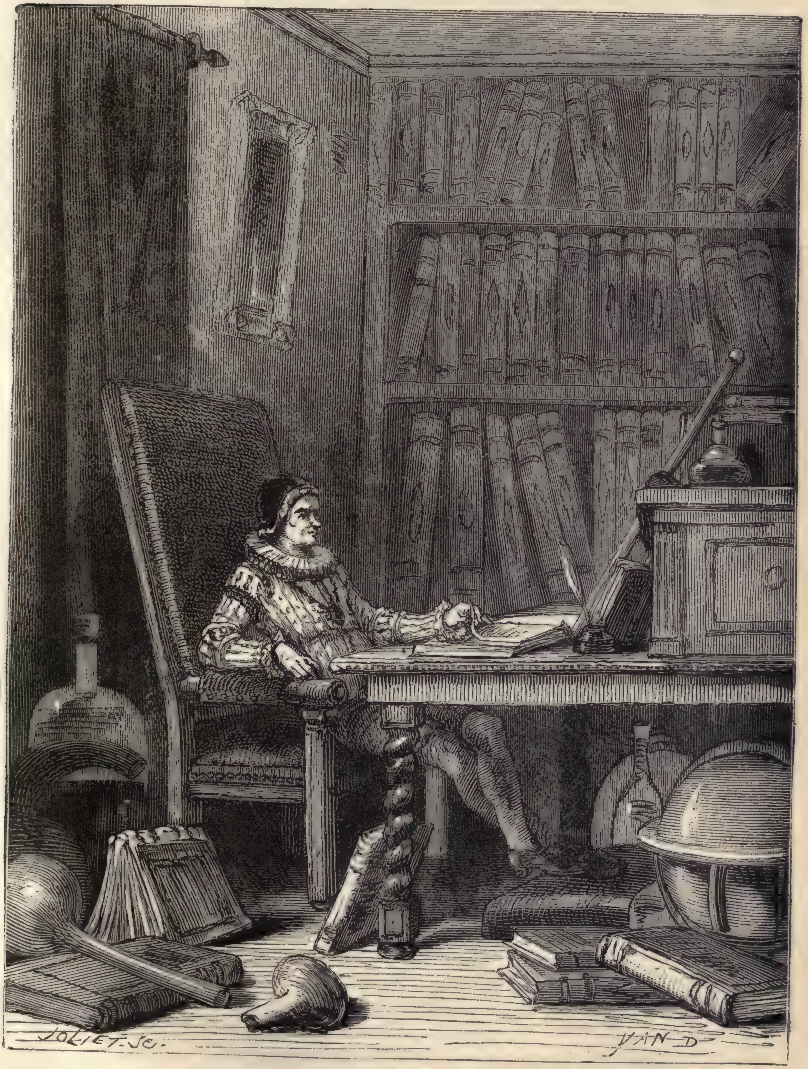
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FIG. 41.—GILBERT COMPOSING HIS TREATISE ON MAGNETISM.

substances known to exhibit the property of attracting light bodies when rubbed. Gilbert found that most gems possessed the same property, and so also did glass, sulphur, wax, resin, talc, alum, and many other bodies. He also observed that the substances attracted might be metals, stones, earths, liquids, etc. Though these observations did not conduct Gilbert to any general law, they sufficed to give the initial impulse to a new science, and we shall in the sequel see how rapidly this science developed. In Gilbert's work the laws of magnetic polarity are clearly demonstrated, the various forms of magnets are discussed, the positions of the poles determined, the *armatures* of loadstones (Fig. 38) considered, the grouping of iron filings about the poles of magnetic bars (Figs. 39 and 40) examined, and many other questions treated by purely inductive and experimental methods.

The science of Mechanics had its foundations laid in the century we are now considering. The principle of the resolution and composition of motions was distinctly referred to in an astronomical work by Jerome Fracastor, who shows that bodies have a tendency to move towards the centre of the earth in a straight line, and when they are projected in any direction transverse to that straight line they still have the same downward motion as if they were simply falling to the earth. The conditions of equilibrium with regard to the lever were known to the ancients, and this is, perhaps, the only one of the mechanical powers of which they established the theory. The inclined plane seems to have presented greater difficulties, and its principle was not demonstrated until the close of the sixteenth century, after the problem had been unsuccessfully attempted by Cardan and by Ubaldi in 1577. The latter considers chiefly the wedge; and in comparing the direction in which it tends to produce motion in the body acted upon with that in which the motion actually takes place, he says that there is a certain "repugnance" between these directions which is greater as the angle of the wedge is more obtuse, and hence he infers that the more acute is the angle of the wedge, the more easily will it produce its effect. He does not, however, find the exact proportion of the force. He correctly observes that the screw may be considered as virtually a wedge wrapped round a cylinder. The first person who really solved the problem of the theory of oblique forces was Stevinus of Bruges (1550—1633), whose investigations entitle him to be considered the founder of the science of Statics. He correctly deduced the ratio of the power to the weight on the inclined plane by a method of reasoning which was at once original, simple, and ingenious. He supposed a perfectly flexible uniform chain to be placed on an inclined plane as represented by the dotted line in Fig. 42. The chain is supposed to form an endless band and to have absolute freedom of

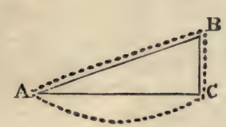

Fig. 42.

motion, yet it would obviously remain at rest. And Stevinus reasons that as the horizontal portion A C could not act by its weight to draw the chain one way rather than the other, the remaining parts, namely, that which lies on the incline, and that which hangs perpendicularly, must balance each other. Now, as the weights of these parts are proportional to their respective lengths, it follows that the power acting along the inclined plane will balance a weight to which it has the same proportion as the height B C has to the length A B of the inclined plane. When the principle of the action of oblique forces had thus been established in this one case, various other instances of the same kind of action were investigated by other authors, who employed the principle established by Stevinus. The first suggestion of the fundamental principle of Hydrostatics, namely, that the pressure at any point in a liquid is in proportion to the depth of the point below the surface, is also due to Stevinus.

The rise of Chemistry in the sixteenth century may be illustrated by the labours of three persons, each of whom opened out a new and unexplored field for the science. Paracelsus applied chemistry to medicine; Agricola founded the metallurgical department of the science; and Palissy showed how chemical truths might be utilized in extending the resources of technical art.

PARACELSUS (1493—1541), whose real name was AURELIUS PHILIPPUS THEOPHRASTUS AB HOHENHEIM, was a native of Switzerland. After having visited nearly every country in Europe as a student in medicine, and having in many places practised the healing art with great reputation, he was in 1526 appointed professor of surgery at Bâle. He there broke through all the established rules and precedents by delivering his lectures to his students in their native German tongue, and he is therefore entitled to the merit of being the first *savant* who sought to popularize science. But at his first lecture the students were still more astounded by his avowed contempt of the venerated medical authorities of that day, namely, Hippocrates, Galen, and Avicenna. Paracelsus had the works of these authors brought into the room, and there he deliberately committed them to the flames, informing his audience that his beard, his hat, and his shoes knew more of medicine than all the physicians of antiquity put together. Our readers will perceive from this incident that Paracelsus was not a man likely to fail in attaining his object through diffidence in his own abilities, or modesty in proclaiming his own merits. Paracelsus marks an era in the history of chemistry, because he showed that this science was worthy of being pursued apart altogether from such objects as the alchemists sought, and that it was capable of conferring solid benefits on mankind by providing the physician with new and potent drugs. Paracelsus is also a conspicuous figure in the history of medicine, as the earliest and boldest rebel against the tyranny of ancient authority. He attacked the rival sects of Galenists and

Hippocratists with equal impetuosity. But he deserves best to be remembered for having introduced into medicine certain compounds of the metals which are, even at the present, reckoned among the most potent resources of the physician. He regarded the human body as itself a chemical compound, and he supposed disease to be produced by chemical changes, and to be successfully treated only by chemical preparations. The doctrines of Paracelsus gave rise to no little controversy amongst the physicians of the sixteenth century, many of whom showed themselves stout defenders of the supreme authority of the ancients.

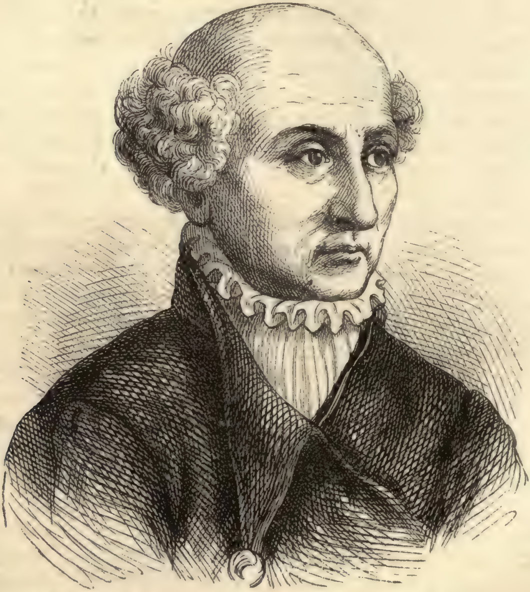

FIG. 43.—PARACELSUS.

That branch of Practical Chemistry which relates to the extraction of metals from their ores had of course been, from the earliest ages, the subject of a blind and limited experience; but, in the sixteenth century, metallurgical processes became, in the hands of AGRICOLA (1494—1555), matters for scientific study. It was the fashion about this period for authors to translate their native names into Latin, or sometimes into Greek, and hence it is under these classic forms that the names of the men of learning of the sixteenth century are known. The

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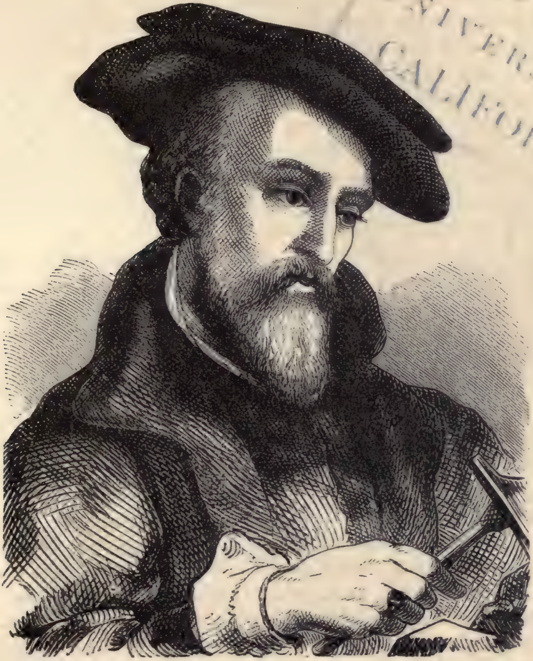

FIG. 44.—AGRICOLA.

real name of Agricola was LANDMANN, and he was a native of Saxony. He wrote a great treatise in which he fully and systematically describes all matters relating to mining and metallurgy. This work contains many curious and accurate observations of facts, which received their true explanation only at a much later period.

Many years before the great Lord Chancellor of England declared that science ought to bear fruit by the improvement of arts, one of the most interesting characters of the sixteenth century was illustrating by his labours the practical ends of knowledge, as well as the true method of experiment and observation by which only that knowledge could be acquired. We allude to BERNARD PALISSY, the famous French potter (*c.* 1500—1588), who, before Bacon was born, was already reading in the great book of nature, and advising others to do the same. "Je n'ai point eu"—he says—"d'autre livre que le ciel et la terre, lequel est connu de tous; et est donné à tous de connoistre et lire ce beau livre." Although the study of Palissy's life is an extremely interesting piece of biography, we must pass over its incidents and its struggles to give one or two extracts from his writings. The man himself is truly remarkable for the variety and extent of his acquirements, as well as for his noble courage and heroic perseverance, and his name has a place in the history of science, as well as in the histories of art, of industry, and of literature. All his writings are in French, and they are unsurpassed for simplicity, vigour, and richness of style. It is curious to observe that these are the very same qualities which, in his ceramic productions, most delight the eye of the artistic connoisseur. Lamartine considers Palissy one of the greatest of French prose writers, rivalling Montaigne in flowing freedom of style, Rousseau in eloquence, and Bossuet in power of imagination.

"I was lately," says Palissy, "one day remarking the colour of my beard, which caused me to think of the little time I had left to finish my course, and that made me consider the lilies and the corn in the fields, and various kinds of plants, which change their colours from green to white when they are ready to yield their fruits. Some trees also hasten to bloom when they find their natural and vegetative power ceasing. Such considerations caused me to remember that it is written that we should take care not to abuse the gifts of God, and hide our talent in the earth; also, that it is written that the fool hiding his folly is better than the wise man who conceals his wisdom. . . . . . For the space of forty years I did dig up the ground to find the things hidden in the bowels of the earth, and, by the grace of God, this means has made known to me secrets hitherto concealed from men, even from the most learned, as may be learnt by my writings. I well know that some will deride, saying that *it is impossible that a man ignorant of the Latin language can understand natural things*, and that they will say that it is great presumption in me to write in opposition to the opinions of so many famous and venerable philosophers who have discoursed on natural effects, and filled all the earth with their wisdom. I know also that others will judge by the outside, saying that I am only a poor artisan. In spite of all these considerations I have not failed to pursue my undertaking, and in order to stop all calumnies, I have arranged a cabinet, in which I have put the curious and extraordinary things that I have taken from the bowels of the earth, and these bear some testimony to what I say; and there will be found not a man but will be obliged to acknowledge that these are genuine, after he has seen the things I have prepared to convince those who would not otherwise give credence to my writings."

The following passage shows that Palissy clearly perceived the fallacy of the doctrines prevailing at his time, when unbounded reliance was placed on accepted authorities and established theories, instead of a direct resort to the examination of the facts of nature.

"The desire which I have that you (*i.e.*, the reader) should profit by the perusal of this book, urges me to warn you to beware of exciting your mind by sciences written in the closet in the midst of theories, imaginary, or drawn from some book written by the imagination of

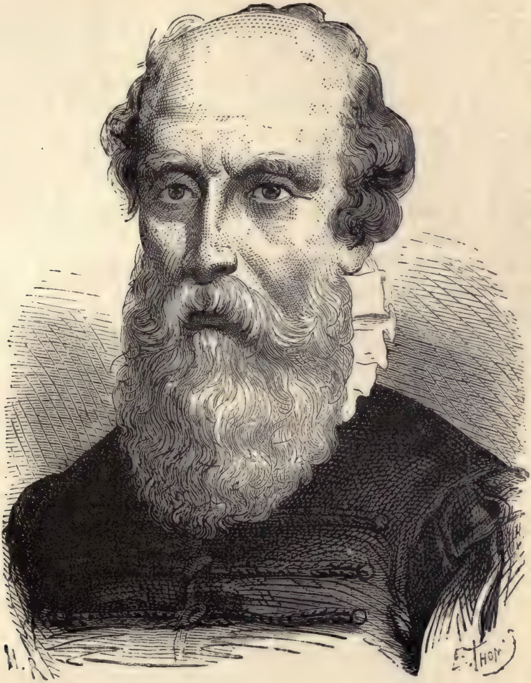

FIG. 45.—BERNARD PALISSY.

those who have practised nothing, and to take care of believing the opinions of those who say that theory has produced practice. . . . . If men could only carry out their ideas, I would be of their party and opinion; but so much is wanting to that! If the things conceived in the mind could be carried out, the poor alchemists would accomplish great things, and would not be amusing themselves in searching as some have done for fifty years. If the theories formed in the minds of the leaders in war could be carried out, they never would lose a battle. I venture to say, for the confusion of those who maintain such an opinion, that if they had all the theories in the world, they could not make a shoe."

He tells how he began his experiments as a potter. "Know that it is five and twenty years ago since I was shown a turned and glazed earthenware cup of such rare beauty that I immediately began to debate in my own mind, recollecting the various remarks that people had made when laughing at me as I was painting figures. Without regarding the fact that I had no knowledge of clays, I began to seek for glazes like a man groping in the dark. Not having heard of the way in which glazes were made, I pounded all the substances which I thought might effect anything, and then I bought a quantity of earthen vessels. After having divided these into pieces, I placed upon them the substances I had ground, and having marked them, I made memoranda in writing of the substances put upon each of them. Then having constructed a furnace to my fancy, I set the aforesaid pieces to bake, in order to see if my stuffs would produce any white colour; for I did not seek any other than a white glaze, because I had heard it said that the white glaze was the basis of all the others."

After long years of toil Palissy at length succeeded in attaining his object, and having thus been put in possession of the means of giving technical perfection to his works, his artistic genius came into play in the production of that peculiar kind of ware for which he is so famous. His works are distinguished by natural objects and ornaments in relief, which are often highly coloured. The colours, however, are limited to a comparatively few, of which a pure yellow, an ochry yellow, a greyish blue, and a deep blue are the most frequent; but some specimens show also certain shades of green and violet of great beauty. The truthfulness of the forms of the natural objects which decorate the Palissy ware has often been remarked, and this quality Palissy obtained by the simple process of taking casts from the objects, such as fish, reptiles, shells, etc. These casts formed the moulds for his dishes, and, of course, from one set of moulds he was able to produce any number of examples. It is interesting to remark that Palissy took his casts of shells from the fossil specimens in the tertiary strata of Paris, and that he contributed a most important part to a debate then being carried on among the learned as to the origin of fossils. Palissy, in a treatise on springs and waters, published in 1580, asserted —and he was the first in France who dared to do so—that the fossil shells and fishes were the actual remains of once living marine animals. He opposed also a doctrine, then much in vogue in Italy, which attempted to account for the fossil shells by a universal deluge.

Alchemists in numbers flourished during the sixteenth century in every country of Europe. Many of these were impostors, who took advantage of the credulity of the age, and avowedly sold "the powder of projection," by means of which, they pretended, base metals could be transmuted into silver and gold. Others doubtless there were, but in fewer numbers than in the previous age, who entertained a sincere belief in the possibility of attaining the great object of alchemy, and these laboured through good report and through bad report to reach the great secret. Many treatises on alchemy belonging to this period are extant; but though these afford much curious lore to the antiquary, we shall pass them over, and resume the history of chemistry in the following century by a notice of Van Helmont.

In the laboratories of the alchemists many strange chemical phenomena must have been first observed, many new processes invented, many compounds formed of which nature had presented no example. But the race of alchemists has left behind no great general principles as permanent acquisitions to science. Their views of the composition of bodies united the ancient theory of the four elements with certain doctrines of their own, the most prominent of which was that metals and minerals were constituted by combinations of "salt," "sulphur," and "mercury." These terms, as used by the alchemists, bear a wider sense than that which we now attach to them; "mercury," for instance, appears to include any volatile liquid whatever. In the search for the means of transmuting metals, the separation from the metals of their constituent "mercury" was a great object, since it was believed that if this basis of the metals could but be isolated and studied apart, the clue to the process of transmutation would be obtained. After all, the proof of the existence of a common base in all metals, and the discovery of methods of transforming one metal into another, are not beyond the possibilities of chemical science.

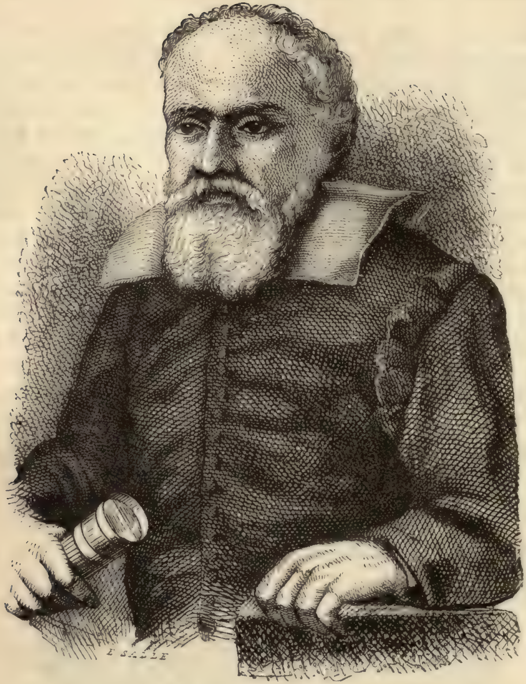

FIG. 46.—GALILEO.

# CHAPTER V.

## GALILEO.

GALILEO GALILEI was born at Pisa in 1564, and was the eldest of a family of three sons and three daughters. His father was a man of considerable talent and varied attainments, and published several treatises on music, in one of which a passage occurs showing that he had a conception of true philosophy which was rare in those days, but was fully realized in the career of his illustrious son. "It appears to me," he says, "that they who, in proof of any assertion, rely simply on the weight of authority, without adducing any argument in support of it, act very absurdly: I, on the contrary, wish to be allowed freely to question and freely to answer you without any sort of adulation, as well becomes those who are truly in search of truth." Galileo in his boyhood was fond of constructing models of machinery and ingenious toys for the amusement of himself and his companions. He acquired, under considerable difficulties owing to his father's straitened circumstances, the rudiments of a classical education and a knowledge of the studies which were then usually followed. Music and drawing formed the occupation of his leisure hours. In the one he became a skilful performer on several instruments, and was so passionately fond of painting that he had at one time decided to make that his vocation. In his desire to fathom the mysteries of perspective, and to unravel the intricacies of the theory of music, he was led to the study of geometry and mathematics. His father had, however, designed him to follow the profession of medicine, and with this object he had been entered as a student of the University of Pisa in 1581. It is said that Galileo's disposition towards intellectual freedom made him at first distasteful to the professors, as he would not accept their quotations from Aristotle in solution of his philosophical problems. When this bold student had, for the reason before indicated, entered upon the study of geometry, he found such a charm in the demonstrations of Euclid that Hippocrates and Galen were neglected. From Euclid he passed to the perusal of the writings of Archimedes, and while he was studying the treatise of the Syracusan on bodies immersed in liquids, he himself wrote an essay on the hydrostatic balance, describing its construction and fully indicating the methods which Archimedes must have adopted for detecting the fraudulent composition of Hiero's crown (page 40). This essay attracted the attention of a distinguished mathematician, who suggested to Galileo the somewhat difficult problem of the position of the centre of gravity in solid bodies as a subject for his next essay. Galileo's treatment of this subject proved so satisfactory that he was recommended to the reigning Duke of Tuscany for the professorship of mathematics at the University of Pisa. He was appointed, and entered upon the duties of the office in 1589, being then in his twenty-sixth year.

Galileo was no sooner settled in his new position than he proceeded with great ardour in his self-imposed task of putting to the test of experiment the truth of Aristotle's physical doctrines. These he found, in most instances, to be without even the shadow of a support from the facts. Though Galileo was not the first who ventured to call in question the trustworthiness of Aristotle in matters of science, no previous philosopher had made so formidable an attack upon the venerated system, which had become so deeply rooted in men's minds that, in spite of ocular demonstration of its falsity, they clung to it with all the tenacity which is displayed in matters of religious belief. It was one of Aristotle's accepted and self-evident truths that the heavier of two bodies falling to the ground moves with a greater velocity than the other. Galileo, on the other hand, maintained that all bodies fall to the earth with the same velocity, or, if any slight difference in the velocity be found, it is fully accounted for by the resistance of air. But the Aristoteleans quoted chapter and verse of their master's works, and ridiculed the idea that the fact could be otherwise than he had stated. As Galileo says, "they fancied that science was to be studied like the Æneid or the Odyssey, and that the true reading of nature was to be discovered by the collation of texts." There is at Pisa a well-known structure, the famous Leaning Tower, of which the peculiarity seems as if providentially designed to offer remarkable facilities for putting the question to the test of experiment. Galileo invited the defenders of Aristotle's doctrine to witness for themselves the result of allowing two unequal weights to fall to the ground from the lofty impending gallery. Yet though they saw with their own eyes the unequal weights strike the ground at the same instant, they asserted as strenuously as before that a weight of ten pounds must naturally fall ten times as fast as a weight of one pound! And they declined to accept Galileo's experiment as settling the case, considering that what they termed the natural velocities were here from some unknown cause interfered with. It seems at first sight marvellous that men should have for centuries accepted upon authority a falsity which could have been so easily exposed; but it should be remembered that Aristotle's axiom was one of the foundations of the accepted system of philosophy, and men are always very reluctant to have their settled convictions disturbed. Galileo's exposure of the folly of some of these opinions produced in the minds of the Aristoteleans not conviction of their errors, but ill-will towards himself, and, to escape from their malicious machinations, he left Pisa to accept the chair of mathematics at Padua. The income attached to this was extremely small, and he was obliged to add to his resources by occupying much of his time by private teaching. Notwithstanding these engagements, he found opportunities for pursuing his investigations, and, while at Padua, he composed several works. While here it is said he invented the air-thermometer, which contrivance has been usually attributed to Santorio. Galileo's was merely a glass tube terminating in a bulb, the tube dipping beneath the surface of a liquid which also partly occupied the tube, while the bulb, which was at the top, remained filled with air. (The arrangement of the apparatus is shown in Fig. 47.) It was the expansion or contraction of the air which indicated the temperature, and though the instrument was sensitive to small changes of temperature, the height of the liquid in the tube was affected also by changes of barometric pressure. The thermometer was improved by one of Galileo's pupils, the son of the Duke of Florence, who brought it into the form we now use it as a spirit-thermometer, in

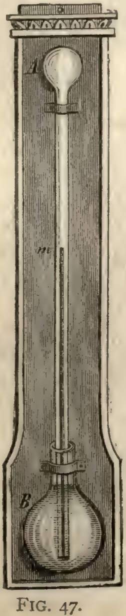

FIG. 47.

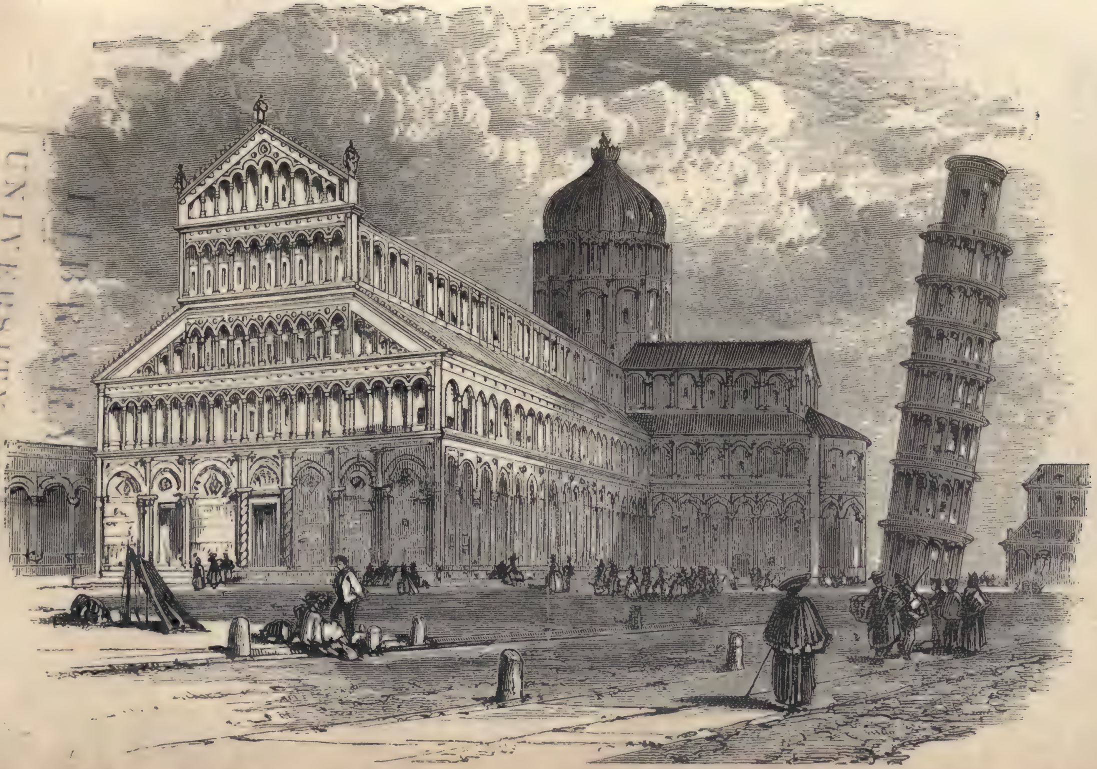

PLATE. V.—THE CATHEDRAL AND LEANING TOWER AT PISA.

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which the temperature is indicated by the expansion of the liquid hermetically sealed in the glass vessel. At a later period (1670) mercury was substituted for spirits of wine.

During this period of his professorship at Padua, Galileo became convinced of the truth of the astronomical doctrines of Copernicus. For some time after he had himself become a convert he was obliged to continue teaching the Ptolemaic system, out of compliance with popular prejudices. In 1594 a remarkable witness against the Aristotelean doctrines appeared in the form of another new star in the constellation Ophiuchus, as if to protest against the dogma of the unchangeability of the heavens. A similar phenomenon had been observed thirty years before, when Galileo was a mere child. On this occasion a crowd of auditors filled the philosopher's lecture-room to learn the results of his observations. He showed from the absence of parallax that the new star could not be, as was generally alleged, merely a meteor at no great distance from the earth, but that it was really situated among the remotest heavenly bodies. This reasoning was unanswerable, as the reader may perceive who will be at the trouble to understand what is meant by parallax. This may be explained by a familiar experience. He has probably observed when travelling on a railway the rapid change produced in the relative positions of the objects of the landscape by the change in his point of view. When a wide extent of country is thus rapidly traversed, the objects almost appear to be revolving round a centre which is being continuously displaced as the train proceeds. He has also doubtless observed that the distant objects do not exhibit changes in their relative positions to anything like the same degree. A distant spire or mountain will, for instance, retain nearly the same apparent position against the sky while the traveller changes his point of view by a considerable distance. Hence it is obvious that if by changing our point of view we produce no perceptible change in the relative positions of a group of objects, it can only be because these objects are at a distance very great as compared with the change of our position. Therefore, as the new star in Ophiuchus had precisely the same position among the other stars from whatever part of the earth it was viewed, it must obviously have been at an immense distance from us.

Galileo's popularity at Padua became so great that his lecture-room was thronged with auditors, and a place capable of holding a thousand persons was at times too small for his audience. After he had held the professorship at Padua for nearly eighteen years, overtures were made which induced him to return to his former position at Pisa on terms which would leave him ample leisure for the prosecution of his researches and the composition of his works. The period immediately preceding Galileo's retirement from Padua was marked by the great invention with which his name will ever be associated—the Telescope, although the honour of priority has been claimed for others. Passages from the writings of Fracastor, who died in 1533, and from those of Baptista Porta and others, have been construed into indications of an acquaintance with the telescope. It is tolerably certain that a Dutch optician named Lipperhey accidentally hit upon an arrangement of two convex lenses, which formed a telescope. The fact of this arrangement being that which is now called the Astronomical Telescope, is indicated by a circumstance expressly mentioned, namely, that the weathercock of a church was seen through it, magnified, but inverted. In 1609 Galileo, being at Venice, heard that an instrument had been invented in Holland which caused distant objects to appear nearer. When he had returned to Padua, he considered by what means this effect must have been produced. He soon arrived at the conclusion that two lenses, either concave or convex, must have been employed, and proceeding to experiments, he found that the desired effect was producible by placing at suitable distances a concave lens near the eye and a convex towards the object. This is exactly the arrangement which is adopted in the opera-glass; but although after Galileo's discoveries had made the telescope famous, telescopes were in the first instance made on his plan, it has been found more advantageous to adopt the principle involved in Lipperhey's combination, and telescopes are now always constructed with convex lenses. As soon as Galileo had completed his first telescope, which magnified only three times, he took it to Venice, where it made a great sensation, and Galileo's quarters were daily besieged by a multitude of persons eager to satisfy their curiosity concerning the new invention. It is said that Galileo spent more than a month in exhibiting his instrument to the principal inhabitants. The Doge caused a hint to be given to Galileo that the Senate would be proud of the possession of so marvellous an instrument, and Galileo, having taken the hint and presented the telescope to his patrons, was rewarded by being appointed to the Padua professorship for life, and his salary was doubled. The public curiosity did not subside for a long time, and instruments of a very inferior kind were soon made and sold everywhere in great numbers as philosophical toys. The instrument was not at first called a telescope, but was known as Galileo's Tube, or the Perspective, or the Double Eyeglass.

Galileo proceeded to construct a second and more powerful instrument, which gave him a magnifying power of eight, and immediately directing it to the heavenly bodies, began with the moon that series of splendid discoveries which will make his name immortal as long as the human race remain to watch the planets which roll through the sky. In the moon he recognized elevations and depressions like the mountains and valleys which diversify the surface of our own globe. The darker spaces he considered to be seas, the lighter continents. The oft-quoted lines of Milton thus correctly describe Galileo's conclusions about the moon:

> . . . . . . . . . . whose orb
> Through optic glass the Tuscan artist views
> At evening, from the top of Fesolé,
> Or in Valdarno, to descry new lands,
> Rivers, or mountains in her spotty globe.

These fresh discoveries were as unpalatable to the Aristoteleans as the confutations of their master's physics, for, according to their accepted doctrines, the moon was a perfect sphere, having, of course, an entirely unbroken surface. Nor would they admit that the argument from the analogy of our own globe had any force. To show to what straits the Aristoteleans were reduced, we may cite the attempt made by one of them to reconcile the old doctrines with the new facts. Constrained to admit the evidence of his senses as to the visible inequalities of the lunar surface, he asserted that the observed hollows must be filled up by an invisible crystalline substance, which would bring up the exterior surface of our luminary to the smoothness of a perfect sphere. Galileo replied that this was an admirable idea, its only fault being that it was neither demonstrated nor demonstrable. "But," says he, "I am perfectly ready to believe it, provided that, with equal courtesy, you will allow me to raise upon your smooth surface crystal mountains (which, of course, nobody can perceive) ten times loftier than those I have seen and measured."

But when, on the 7th of January, 1610, Galileo turned his telescope to the planet Jupiter, a spectacle presented itself which a few subsequent observations showed to be of still higher significance. He noticed three small stars near the planet, and almost in a straight line with the ecliptic. The next night he found that they appeared on the other side of the planet. Plainly, therefore, either the planet had moved with regard to these stars, or they had changed their positions in relation to the planet. The former supposition implied that the planet's motion must be in the direction contrary to that on which all calculations were based. He eagerly watched for the next opportunity of observing Jupiter, which was on the 10th of January, when he saw two of the three stars again on the same side of the planet as on the 8th; and as it was impossible to attribute to Jupiter himself movements in alternate directions, he was forced to a conclusion memorable in the history of astronomy, and abundantly confirmed by his subsequent observations, "that there are in the heavens three stars which revolve about Jupiter, in the same manner as Venus and Mercury revolve about the sun." On the 13th of the same month he discovered the fourth satellite. In honour of his patron, he gave the four newly-discovered bodies the name of the *Medicean Stars* in the account which he published at the end of March, immediately after he had completed his observations. These further discoveries were a greater blow than ever to the upholders of the old system. The telescopic view of the planet itself presented a well-defined disc, while all the fixed stars appeared in the same instrument as mere points of light, only brighter than when observed with the naked eye. Up to this period the only circumstance that had distinguished planets from the fixed stars was their change of place. Now it was proved that they were globes like our earth, and that, like it, they might have moons circling round them. It need hardly be said that in many quarters Galileo's assertions were received with scorn and incredulity. The leading professor of philosophy at Padua absolutely declined to look through Galileo's glass to view for himself the Jovian satellites, though repeatedly urged to do so; and he proved to the Grand Duke of Tuscany by the strictest academical deductive reasoning that they could not possibly exist. But Cosmo, having himself seen the satellites many times, trusted to the evidence of his senses in preference to the professor's syllogisms. Another line of objection was advanced by a Florentine astronomer, which is curious, as it illustrates the tendency towards the notion that the congruities and proportions of numbers, being in some degree pleasant for us to contemplate as forming very definite mental conceptions, must be found also in nature, in accordance with our preconceptions of her perfections. The reader will remember how completely the system of Pythagoras turned upon doctrines of numerical relations, and so on. It certainly supports our view of the ancient philosophies not being the mere vagaries of individual thinkers, but representative either of some aspect of truth or of some permanent tendency of the human mind, when we find these same notions about numbers still in many instances influencing the minds of the present age. For example, a living astronomer of great eminence has published a work in which he traces the most extraordinary relations between the dimensions of a very ancient edifice, the chronology of Jewish history, and the standard British weights and measures. Now, the objections of Sizzi, the Florentine astronomer, to Galileo's Medicean planets, turned upon number. There are, said he, seven openings in the head which communicate with the outer world, namely, two nostrils, two ears, two eyes, and a mouth; there are seven metals, there are seven days in the week, and many other things are reckoned by sevens, from which we may gather that the number of the planets is necessarily also seven. A similar argument was drawn from the seven branches of the golden candlestick in the Jewish temple, and from the seven Churches of the Apocalypse. Some of Galileo's opponents advanced the most audacious denials of the reality of his discoveries. "The telescope," said one, "does wonders upon the earth, but represents celestial objects falsely. I do not more surely know that I have a soul in my body than that reflected rays are the sole cause of Galileo's erroneous observations. I will never concede his four new planets to that Italian from Padua, though I die for it."

Galileo soon recognized the fact that Jupiter's satellites might be made the means of solving a problem which was then engaging the attention of astronomers, namely, a method for finding the longitudes at sea. The reader, even if unacquainted with astronomy, will have no difficulty in understanding the principle upon which Galileo thought of utilizing the newly-discovered planets. Supposing that the exact instant of time at which the Jovian satellites severally plunge into or emerge from the shadow of their primary had been calculated twelve months beforehand, and that the days, minutes, and seconds at which these occurrences would be witnessed at Florence had been registered, a mariner at sea who wished to ascertain his longitude would observe some particular eclipse which had been predicted for that night. He would note the hour, minute, and second at which the eclipse took place; for we may suppose him to have a watch which he had set to the true time by observations of the sun the same day. The difference between the time indicated by his watch and the time at which, according to the clocks at Florence, the eclipse takes place, would give him the longitude. The earth turns through 360° in twenty-four hours, that is, through 15° in each hour; so that the time of noon is later by one hour at places 15° to the west of a given place, and so on in like proportion. Galileo's plan was unexceptionable in principle, but there are practical difficulties in observing the satellites of Jupiter from the deck of a ship at sea; and, further, the mariners of that time were not provided with timepieces or watches upon which reliance could be placed. But for these merely mechanical obstacles, the world would have witnessed, at a period when men had not entirely ceased to draw horoscopes, the realization, in a nobler sense, of the astrologer's vain dream of planetary influences; for the satellites of Jupiter, invisible to the unassisted vision, might be said by their aid to navigation to play no inconsiderable part in directing the destinies of our race.

The next planet which received Galileo's attention was Saturn, but his telescope was not sufficiently powerful to show him distinctly the real configuration of this wonderful planet. He interpreted the appearances presented in his glass as indicating that Saturn was a triple planet. "I have," he says, "observed with wonder that Saturn is not a single star, but three together, which, as it were, touch each other; they have no relative motion, and are constituted in this form—**oOo**, the middle being somewhat larger than the lateral ones. If we examine this with a glass which magnifies the surface less than a thousand times, the three stars do not appear very distinctly, but Saturn then presents an oblong appearance, like the form of an olive, thus—⬭. I have now discovered a court for Jupiter, and for this old man two servants who aid his steps and never quit his side." Galileo's next discoveries were the solar spots, or rather, he for the first time perceived them in their true relations, for these spots are occasionally so large that they become visible without the aid of a telescope, and observations of their appearance in this way are upon record. As Galileo noticed that the spots moved across the sun's disc, he was at first inclined to believe that they were planets revolving very near to the sun, and appearing as black spots when opposed to his luminous disc. He soon saw, however, that the spots belonged to the sun's surface, for they sometimes reappeared in the same order; at other times several spots would unite into one, or a single spot would divide into three or four. They had also different degrees of darkness, and as they approached the edge of the sun's disc he noticed that they always appeared to contract in breadth, without losing their length. Finally, he arrived at the true conclusion that the movement of these spots was due to the rotation of the sun about his axis, which occupied a period of about twenty-eight days. Like all Galileo's other discoveries, the announcement of this one led to bitter controversies. Galileo also discovered the bright patches on the sun's surface which are called the *faculæ*.

In the year 1611 Galileo visited Rome, and was received by the great ecclesiastical dignitaries and others with the consideration due to his talents and reputation. We are informed that he took with him his best telescope, in order to exhibit to his friends at Rome the solar spots, and the crescent form of the planet Venus when in a similar position with regard to the sun as our moon in the first quarter, and his other recent discoveries. While at Rome Galileo joined a scientific society which had not long before been established, and which may be noted as one of the first associations for the encouragement of scientific study. It bore the somewhat fanciful name of the Lyncean Academy, in reference to the acute vision which is supposed to be possessed by the lynx; and the name was thought emblematic of the penetrating insight that should belong to those who seek to investigate nature. Several of Galileo's minor treatises were originally printed at the expense of the Lyncean Academy.

Soon after his return from Rome Galileo published at Florence his "Discourse on Floating Bodies," in which he confirms the doctrines of Archimedes on that subject, and refutes by convincing experiments the prevailing Aristotelean notion that the sinking or floating of bodies in a liquid depends upon their shapes. The Aristoteleans proclaimed that ice floats upon water, not because it is lighter, "for," said they, "ice is produced from water by cold, which is attended by condensation." Galileo affirmed that a piece of ice of any shape will float in water, and that ice, so far from being denser than water, was more rarified, and the fact of its floating was the proof of this.

Galileo's bold manner of announcing new truths, and his energy in defending them against the attacks which never failed to be made upon them, excited much personal hostility against himself and his discoveries; for the teachers and professors of the older systems found their knowledge and their reputation vanishing together, and were the more exasperated by the sarcasm and ridicule which Galileo sometimes resorted to in exposing the absurdities of the current doctrines.

More potent, however, was the theological enmity which now openly displayed itself against the man who had proclaimed so many great truths confirming the objectionable Copernican theory of the earth's motion about the sun. The first decided demonstration on the part of the ecclesiastics against our philosopher was a sermon preached by a Dominican friar, in which Galileo and his followers were abused in the most violent terms. The text was made an unbecoming play upon words, and unmeasured personalities formed the subject of the discourse. It was, "Ye men of *Galilee*, why stand ye gazing up into heaven?" The preacher impressed upon his audience that "geometry is of the devil," and that "mathematicians should be banished as the authors of all heresies."

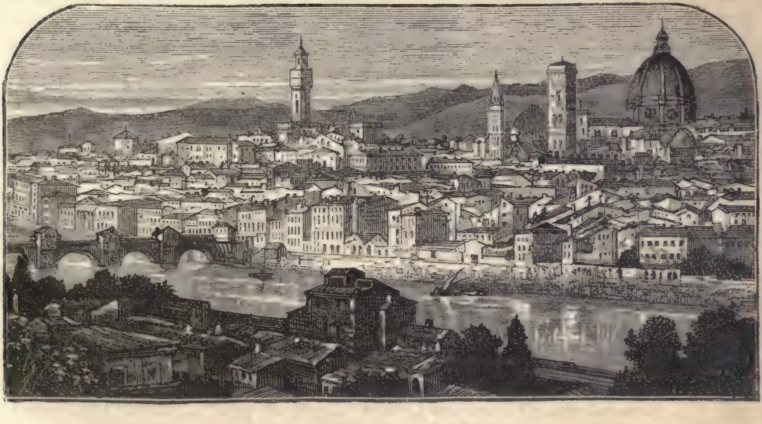

FIG. 48.—VIEW OF FLORENCE.

Galileo, in a letter to the Abbé Castelli, his former pupil, and again in another longer communication nominally addressed to the Grand Duchess of Tuscany, discussed in moderate and sensible language the question then at issue between theology and science. Since Galileo's time, labourers after truth in other fields of science than astronomy have found some of their conclusions impugned on the very same grounds that Galileo controverts in relation to astronomy.

"In the discussion of natural problems we ought not to begin at the authority of texts of Scripture, but at sensible experiments and necessary demonstrations; for from the divine word the sacred Scripture and nature did both alike proceed, and I conceive that, concerning natural effects, that which either sensible experience sets before our eyes, as necessary demonstrations do prove to us, ought not upon any account to be called into question, much less condemned, upon the testimony of Scriptural texts, which may, under their words, couch senses seemingly contrary thereto."

The Inquisition at Rome now set its agents to work to collect evidence against Galileo, and the Archbishop of Pisa did not disdain to resort to the meanest cunning in his endeavour to induce Castelli to place in his hands the original manuscript of Galileo's letter, a scheme which was frustrated by Galileo's express prohibition to his friend against giving up the document to the wily ecclesiastic. In the year 1615 Galileo again went to Rome, where he was lodged in the palace belonging to the ambassador at the Papal Court of his patron the Duke of Tuscany. It was rumoured at the time, that this visit was not a spontaneous one on the part of the philosopher, that he was, in reality, cited to appear to answer before the Inquisition the charge of heresy. Some documents, which have been published only within the last few years, make it clear that such was the fact. The Inquisition appointed certain theologians of their own body to consider the point at issue. As they solemnly condemned the doctrine of the earth's motion, Galileo was ordered to renounce his opinions, and was forbidden to teach or defend these opinions in any way whatever. Galileo, under threats of imprisonment, promised compliance with these orders. All these proceedings were conducted in privacy, and no visible restraint was placed upon the personal liberty of our philosopher. Immediately afterwards the Church committed itself to the grand mistake of publicly pronouncing as false and heretical all writings asserting that the earth moves,—the doctrine of its possessing any motion, diurnal or annual, being declared by authority entirely contrary to Holy Scripture. Thus the theory which Copernicus had unfolded in his great work dedicated to Pope Paul III., after having been permitted for more than seventy years to remain for discussion in the hands of philosophers, to defend or attack it as they pleased, was now invested with more importance by the attitude the Church assumed.

The feelings with which Galileo quitted Rome may be imagined, nor was he able entirely to conceal his opinion of the mental enlightenment of the ecclesiastics, if we may judge by some sentences prefixed to a treatise on a Theory of the Tides which he inscribed to the Archduke Leopold; for the expressions of abject submission to the priestly decrees have really the force of the keenest irony. "This theory," he says, "occurred to me when in Rome, whilst the theologians were debating on the prohibition of Copernicus's book, and of the opinion it maintains concerning the motion of the earth which I at that time believed. But those gentlemen have been pleased to suspend the book, and to declare the opinion false and repugnant to the Holy Scriptures. Now, as I know very well, it becomes me to obey and believe in the decisions of my superiors, which proceed from more profound knowledge than the weakness of my intellect can attain to, this theory I send you I beg your Highness to regard as a fiction and a dream, for it is founded on the motion of the earth, which I now look upon as such. But as poets often learn to prize the creations of their own fancy, so do I in like manner set some value on this absurdity of mine. It is true that when I sketched this little work, I did hope that Copernicus would not, after eighty years, be convicted of error, and I intended to further develop and amplify it; but a voice from heaven suddenly awakened me, and at once annihilated all my confused and entangled fancies."

In 1624, for the third time Galileo visited Rome. Pope Paul V. was dead, and Cardinal Barberini had just been elected to the pontifical chair, under the title of Urban VIII. Barberini had been himself connected with the Lyncean Academy, and had been on terms of personal friendship with Galileo. He was one of the few cardinals who had opposed the decree of 1616. Galileo was urged by his scientific friends to visit Rome to pay his respects to the new Pope, it is probable, with the view of receiving from Urban some more liberal recognition of the claims of science. Galileo's age and infirmities required him to perform the journey in a litter, so that it is not likely he would have undertaken this expedition merely to tender ceremonial congratulations to the newly-elected Pontiff. Be that as it may, he was received in the most affable and cordial manner by Urban, with whom he had several protracted interviews. He received many presents from the Pope, who also addressed to Ferdinand, the son and successor in the Dukedom of Tuscany of Galileo's former patron, a letter, in which he commended the philosopher to that prince's liberality.

Perhaps the reception which had been accorded to Galileo made him flatter himself that he would experience no further molestation from the ecclesiastical powers; perhaps the passionate love of truth and hatred of intellectual oppression, which never ceased to animate his heart, impelled him to brave all risks when in 1632 he published his most famous work, under the title, "A Dialogue, by Galileo Galilei, Mathematician Extraordinary of the University of Pisa, and Principal Philosopher of the Most Serene Grand Duke of Tuscany; in which, in a conversation of four days, are discussed the two principal Systems of the World, the Ptolemaic and the Copernican, indeterminately proposing the philosophical arguments as well on one side as on the other." The work opens with an introduction addressed "To the discreet reader," in which the satire on the ecclesiastical decrees was covered by so flimsy a veil that the author himself would appear devoid of that discretion which he ascribes to his reader. "Some years ago," he says, "a salutary edict was promulgated at Rome, which, in order to obviate the perilous scandals of the present age, enjoined an opportune silence on the Pythagorean opinion of the earth's motion. Some were not wanting who rashly asserted that this decree originated, not in a judicious examination, but in ill-informed passion; and complaints were heard that counsellors totally inexperienced in astronomical observations ought not, by hasty prohibitions, to clip the wings of speculative minds. My zeal could not keep silence when I heard these rash lamentations, and I thought it proper, as being fully informed with regard to that most prudent determination, to appear publicly on the theatre of the world as a witness of the actual truth. I happened at that time to be in Rome. I was admitted to the audiences and enjoyed the approbation of the most eminent prelates of that Court, nor did the publication of that decree occur without my receiving some prior intimation of it. Wherefore, it is my intention in this present work to show to foreign nations that as much is known of this matter in Italy, and particularly in Rome, as ultramontane diligence can ever have formed any notion of, and collecting together all my own speculations on the Copernican system, to give them to understand that the knowledge of all these preceded the Roman censures, and that from this country proceed, not only dogmas for the salvation of the soul, but also ingenious discoveries for the gratification of the understanding. With this object I have taken up in the dialogue the Copernican side of the question, treating it as a pure mathematical hypothesis, and endeavouring in every artificial manner to represent it as having the advantage, not over the opinion of the stability of the earth absolutely, but according to the manner in which that opinion is defended by some, who, indeed, profess to be Peripatetics, but retain only the name, and are contented, without improvement, to worship shadows, not philosophizing with their own reason, but only from the recollection of four principles imperfectly understood."

The persons of the dialogue in Galileo's work are three in number: the principal interlocutor being the expounder of the Copernican doctrines, who answers the ordinary and absurd objections brought forward by a philosopher of the Aristotelean and Ptolemaic school; while the third personage, who proposes the real difficulties, also enlivens the debate with amusing illustrations. No sooner had the work been published than the Inquisition resolved to proceed against the author, and Galileo was summoned to Rome to answer in person before that odious tribunal, though the Duke of Tuscany expostulated through his ambassador at the Pontifical Court, and urged that the advanced years and feeble health of the illustrious philosopher rendered him unfit for such a journey. But the ecclesiastical authorities were inexorable, and the venerable sage was compelled to appear for the fourth time in Rome, where he arrived on the 14th of February, 1633, worn out by age, fatigue, and infirmities, to answer before the tribunals of the Church the charge of scandalous contempt of her authority. And this was at a period when, in Italy at least, the Church appeared to retain her ancient power, and was invested with undiminished pomp and splendour. The vast and magnificent structure that for more than a century had been rising upon the Vatican Hill, and upon which had been

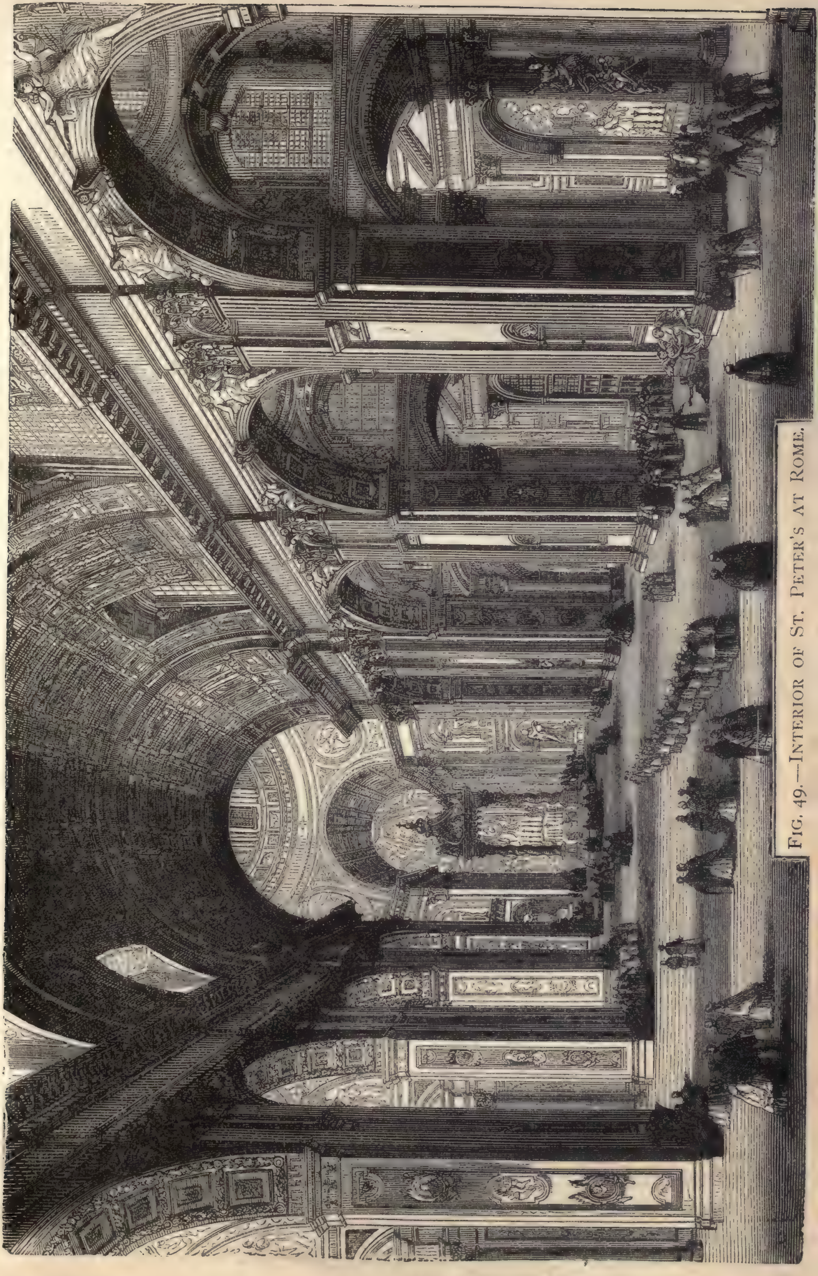

FIG. 49.—INTERIOR OF ST. PETER'S AT ROME.

lavished the genius of a succession of the greatest artists—Bramante, Raphael, Michael Angelo, Vignola, and others—had been completed but a few years before; and, in the interval between Galileo's third and this his last visit to Rome, the majestic pile had been dedicated by Urban VIII. to the great Apostle of the Keys with solemn rites and stately ceremonies.

The circumstances attending the prosecution of Galileo are often referred to in terms which are apt to convey a false impression of the actual treatment he experienced. He is sometimes spoken of as having been plunged in "*the dungeons of the Inquisition;*" we are told of Milton seeing him "in the dim twilight of his cell;" "fierce-eyed jailers" are graphically alluded to, and all manner of sensational horrors have been darkly hinted at. The truth appears to be that Galileo was treated with remarkable lenity, considering the character of the tribunal before whom he was arraigned. There is very good reason for believing that Pope Urban himself was forced only by the official necessities of his position to countenance the prosecution, and that there were even among the ecclesiastical party a few enlightened understandings who would willingly have avoided committing the Church to such a decided antagonism to science. But unfortunately the Church was bound hard and fast to a theological system professedly based on a literal interpretation of the Hebrew records. Whilst his cause was pending, Galileo was lodged at the palace of the Tuscan Embassy, and he was placed under no restraint; here he was visited in a friendly spirit by Cardinal Barberini, the Pope's nephew, who recommended him on prudential grounds to remain as much as possible within the precincts of the ambassador's residence, and to converse only with his own intimate friends. When the proceedings before the Inquisition had arrived at that stage that his personal appearance was required, the usual practice of committing the accused to close and solitary confinement was not followed in his case; he was honourably lodged in the house of the Fiscal of the Inquisition, where his table was supplied from the Tuscan Embassy, and his own servant was allowed to attend without let or hindrance, an adjoining chamber being provided for him to sleep in. After a few days Galileo, however, fretted under even this restraint, and Cardinal Barberini then, on his own responsibility, released him and sent him back to his former quarters in the ambassador's palace.

On the 20th of June Galileo was again summoned to attend the Inquisition, and the next day he was conducted in a penitential garment to the convent of Minerva, where the judges of the Inquisition were assembled to pronounce the sentence. As everybody knows, the venerable philosopher, forced by threats of the possible punishments which that detestable tribunal could inflict, was made formally and solemnly to renounce the truths he had spent his life to learn. As the sentence pronounced upon Galileo, and his abjuration—drawn up, of course, by the Inquisition—will ever remain imperishable witnesses of the folly of opposing narrow and literal interpretations of Scripture to conclusions which science has verified by her own method, we shall give a literal translation of the full text of both documents.

### THE SENTENCE OF THE INQUISITION ON GALILEO.

We, *Gasparo del titolo di S. Croce, in Gierusalemme Borgia;*
*Fra Felice Centino del titolo di S. Anastasis, detto d'Ascoli;*
*Guido del titolo di S. Maria del Popolo Bentivoglio;*
*Fra Desiderio Scaglia del titolo di S. Carlo, detto di Cremona;*
*Fra Antonio Barbarina, detto di S. Onofrio;*
*Laudivio Zacchia del titolo di S. Pietro in Vincola, detto di S. Sisto;*
*Berlingero del titolo di S. Agostino Gessi;*
*Fabricio del titolo di S. Lorenzo, in pane e perna Verospi chiamato Prete;*
*Francesco di S. Lorenzo, in Damaso Barbarino; e*
*Martio di S. Maria Nuova, Ginetti Diaconi;*
by the Grace of God, Cardinals of the Holy Roman Church, Inquisitors-General throughout the whole Christian Republic, Special Deputies of the Holy Apostolical Chair against heretical depravity,

Whereas you, Galileo, son of the late Vincenzo Galilei of Florence, aged seventy years, were denounced in 1615 to this Holy Office, for holding as true the false doctrine taught by many, namely, that the sun is immovable in the centre of the world, and that the earth moves, and also with a diurnal motion; also for having pupils whom you instructed in the same opinions; also for maintaining a correspondence on the same with some German mathematicians; also for publishing certain letters on the solar spots, in which you developed the same doctrine as true; also for answering the objections which were continually produced from the Holy Scriptures, by glozing the said Scriptures according to your own meaning; and whereas thereupon was produced the copy of a writing, in form of a letter, professedly written by you to a person formerly your pupil, in which, following the hypotheses of Copernicus, you include several propositions contrary to the true sense and authority of the Holy Scripture;

Therefore this holy tribunal being desirous of providing against the disorder and mischief thence proceeding and increasing to the detriment of the holy faith, by the desire of His Holiness, and of the Most Eminent Lords Cardinals of this supreme and universal Inquisition, the two propositions of the stability of the sun, and motion of the earth, were qualified by the Theological Qualifiers as follows:

The proposition that the sun is in the centre of the world and immovable from its place is absurd, philosophically false, and formally heretical, because it is expressly contrary to the Holy Scripture.

The proposition that the earth is not the centre of the world, nor immovable, but that it moves, and also with a diurnal motion, is also absurd, philosophically false, and, theologically considered, at least erroneous in faith.

But whereas being pleased at that time to deal mildly with you, it was decreed in the Holy Congregation, held before His Holiness on the 25th day of February, 1616, that His Eminence the Lord Cardinal Bellarmine should enjoin you to give up altogether the said false doctrine; if you should refuse, that you should be ordered by the Commissary of the Holy Office to relinquish it, not to teach it to others, nor to defend it, nor ever mention it, and in default of acquiescence that you should be imprisoned; and in execution of this decree, on the following day at the palace, in presence of His Eminence the said Lord Cardinal Bellarmine, after you had been mildly admonished by the said Lord Cardinal, you were commanded by the acting Commissary of the Holy Office, before a notary and witnesses, to relinquish altogether the said false opinion, and in future neither to defend nor teach it in any manner, neither verbally nor in writing, and upon your promising obedience you were dismissed.

And in order that so pernicious a doctrine might be altogether rooted out, nor insinuate itself further to the heavy detriment of the Catholic faith, a decree emanated from the Holy Congregation of the Index prohibiting the books which treat of this doctrine; and it was declared false, and altogether contrary to the Holy and Divine Scripture.

And whereas a book has since appeared, published at Florence last year, the title of which showed that you were the author, which title is: The Dialogue of Galileo Galilei, on the two principal Systems of the World, the Ptolemaic and Copernican; and whereas the Holy Congregation has heard that, in consequence of the printing of the said book, the false opinion of the earth's motion and stability of the sun is daily gaining ground; the said book has been taken into careful consideration, and in it has been detected a glaring violation of the said order, which had been intimated to you; inasmuch as in this book you have defended the said opinion, already and in your presence condemned; although in the said book you labour with many circumlocutions to produce the belief that it is left by you undecided, and in express terms probable; which is equally a very grave error, since an opinion can in no way be probable which has been already declared and finally determined contrary to the Divine Scripture:

Therefore by Our order you were cited to this Holy Office, where, on your examination upon oath, you acknowledged the said book as written and printed by you. You also confessed that you began to write the said book ten or twelve years ago, after the order aforesaid had been given; also, that you demanded licence to publish it, but without signifying to those who granted you this permission that you had been commanded not to hold, defend, or teach the said doctrine in any manner.

You also confessed that the style of the said book was, in many places, so composed that the reader might think the arguments adduced on the false side to be so worded as more effectually to entangle the understanding than to be easily solved, alleging in excuse, that you have thus run into an error, foreign (as you say) to your intention, from writing in the form of a dialogue, and in consequence of the natural complacency which every one feels with regard to his own subtilties, and in showing himself more skilful than the generality of mankind in contriving, even in favour of false propositions, ingenious and apparently probable arguments.

And, upon a convenient time having been given to you for making your defence, you produced a certificate in the handwriting of His Eminence the Lord Cardinal Bellarmine, procured, as you said, by yourself, that you might defend yourself against the calumnies of your enemies, who reported that you had abjured your opinions, and had been punished by the Holy Office; in which certificate it is declared that you had not abjured, nor had been punished, but merely that the declaration made by His Holiness, and promulgated by the Holy Congregation of the Index, had been announced to you, which declares that the opinion of the motion of the earth, and stability of the sun, is contrary to the Holy Scriptures, and, therefore, cannot be held or defended. Wherefore, since no mention is there made of two articles of the order, to wit, the order "not to teach," and "in any manner," you argued that we ought to believe that, in the lapse of fourteen or sixteen years, they had escaped your memory, and that this was also the reason why you were silent as to the order, when you sought permission to publish your book, and that this is said by you not to excuse your error, but that it may be attributed to vain-glorious ambition, rather than to malice. But this very certificate, produced on your behalf, has greatly aggravated your offence, since it is therein declared that the said opinion is contrary to the Holy Scripture; and yet you have dared to treat of it, to defend it, and to argue that it is probable; nor is there any extenuation in the licence artfully and cunningly extorted by you, since you did not intimate the command imposed upon you.

But whereas it appeared to us that you had not disclosed the whole truth with regard to your intentions, We thought it necessary to proceed to the rigorous examination of you, in which (without any prejudice to what you had confessed, and which is above detailed against you, with regard to your said intention) you answered like a good Catholic. Therefore, having seen and maturely considered the merits of your cause, with your said confessions and excuses, and everything else which ought to be seen and considered, We have come to the underwritten final sentence against you.

Invoking, therefore, the Most Holy name of Our Lord Jesus Christ, and of His Most Glorious Virgin Mother Mary, by this our final sentence, which, sitting in council and judgment for the tribunal of the

Reverend Masters of Sacred Theology, and Doctors of both Laws, Our Assessors, We put forth in this writing touching the matters and controversies before us, between the Magnificent Charles Sincerus, Doctor of both Laws, Fiscal Proctor of this Holy Office, of the one part, and you, Galileo Galilei, an examined and confessed criminal from this present writing now in progress as above, of the other part. We pronounce, judge, and declare, that you, the said Galileo, by reason of these things which have been detailed in the course of this writing, and which, as above, you have confessed, have rendered yourself vehemently suspected by this Holy Office of heresy: that is to say, that you believe and hold the false doctrine, and contrary to the Holy and Divine Scriptures, namely, that the sun is the centre of the world, and that it does not move from east to west, and that the earth does move, and is not the centre of the world; also that an opinion can be held and supported as probable after it has been declared and finally decreed contrary to the Holy Scripture, and consequently that you have incurred all the censures and penalties enjoined and promulgated in the sacred canons, and other general and particular constitutions, against delinquents of this description. From which it is Our pleasure that you be absolved, provided that, first, with a sincere heart and unfeigned faith, in Our presence, you abjure, curse, and detest the said errors and heresies and every other error and heresy contrary to the Catholic and Apostolic Church of Rome, in the form now shown to you.

But, that your grievous and pernicious error and transgression may not go altogether unpunished, and that you may be made more cautious in future, and may be a warning to others to abstain from delinquencies of this sort, We decree that the book of the *Dialogues of Galileo Galilei* be prohibited by a public edict.

We condemn you to the formal prison of this Holy Office for a period determinable at Our pleasure; and by way of salutary penance, We order you, during the next three years, to recite once a week the seven Penitential Psalms.

Reserving to Ourselves the power of moderating, commuting, or taking off the whole or part of the said punishment and penance.

And so We say, pronounce, and by Our sentence declare, decree, and reserve, in this and in every other better form and manner, which lawfully we may and can use.

So we, the undersigned Cardinals, pronounce.

Felix, Cardinal di Ascoli,
Guido, Cardinal Bentivoglio,
Desiderio, Cardinal di Cremona,
Antonio, Cardinal S. Onofrio,
Berlingero, Cardinal Gessi,
Fabricio, Cardinal Verospi,
Martino, Cardinal Ginetti.

The abjuration was drawn up in the following terms:—

### THE ABJURATION OF GALILEO.

"I Galileo Galilei, son of the late Vincenzo Galilei of Florence, aged seventy years, being brought personally to judgment, and kneeling before you, Most Eminent and Most Reverend Lords Cardinals, General Inquisitors of the universal Christian Republic against heretical depravity, having before my eyes the Holy Gospels, which I touch with my own hands, swear, that I have always believed, and now believe, and with the help of God will in future believe, every article which the Holy Catholic and Apostolic Church of Rome holds, teaches, and preaches. But because I had been enjoined by this Holy Office altogether to abandon the false opinion which maintains that the sun is the centre and immovable, and forbidden to hold, defend, or teach the said false doctrine in any manner, and after it had been signified to me that the said doctrine is repugnant with the Holy Scripture, I have written and printed a book, in which I treat of the same doctrine now condemned, and adduce reasons with great force in support of the same, without giving any solution, and therefore have been judged grievously suspected of heresy; that is to say, that I held and believed that the sun is the centre of the world and immovable, and that the earth is not the centre and moveable.

"Wishing, therefore, to remove from the minds of Your Eminences, and of every Catholic Christian, this vehement suspicion rightfully entertained towards me, with a sincere heart and unfeigned faith, I abjure, curse, and detest the said errors and heresies, and generally every other error and sect contrary to the said Holy Church; and I swear that I will never more in future say or assert anything verbally, or in writing, which may give rise to a similar suspicion of me; but if I shall know any heretic, or any one suspected of heresy, that I will denounce him to this Holy Office, or to the Inquisitor and Ordinary of the place in which I may be.

"I swear, moreover, and promise, that I will fulfil, and observe fully, all the penances which have been or shall be laid to me by this Holy Office. But if it shall happen that I violate any of my said promises, oaths, and protestations (which God avert!), I subject myself to all the pains and punishments which have been decreed and promulgated by the sacred canons, and other general and particular constitutions, against delinquents of this description. So may God help me, and His Holy Gospels, which I touch with my own hands.

"I, the above-named Galileo Galilei, have abjured, sworn, promised, and bound myself, as above, and in witness thereof with my own hand have subscribed this present writing of my abjuration, which I have recited word for word. At Rome in the Convent of Minerva, 22d June, 1633.

"I, Galileo Galilei, have abjured as above with my own hand."

The claims of theology to pronounce with authority on any matter within the region of science seem to us to be at once and for ever set aside by the result in this case. The immediate consequence, however, was apparently a blow to science. The documents we have given above were directed to be publicly read in the churches and the universities.

Galileo, in conformity to the terms of his sentence, was sent to the prison of the Inquisition, but after four days he was released, and allowed to reside again at the palace of the Tuscan ambassador, until the place to which he should be consigned had been settled. In July he was sent to the palace of Archbishop Piccolomini, at Sienna, who was one of his best friends, and here he was under no other restraint than remaining within the precincts of the palace. In December in the same year he was allowed to re-enter his own house at Arcetri, near Florence, and here probably he might have returned sooner had not the plague been raging at Florence. He here continued, however, to be placed under restrictions, not even being permitted to visit Florence until after an interval of five years, and even then he was prohibited from leaving his house there and from receiving any of his friends. The misfortunes of the venerable philosopher were not yet at an end, for, in consequence of an incurable affection of the cornea, he lost the sight of both his eyes, and in the year 1637 became totally blind. How severe a deprivation is the loss of sight to any man! and how much more must it have been to Galileo! Father Castelli, his friend and pupil, thus laments the calamity: "The noblest eye is darkened which nature ever made; an eye so privileged and gifted with such rare qualities that it may with truth be said to have seen more than the eyes of all who are gone, and to have opened the eyes of all who are to come." The Inquisition, after this, appear to have relaxed the severity of their restrictions, and he was allowed free intercourse with his friends. Many distinguished strangers also visited him. Milton mentions in one of his prose works that, while in Italy, he saw Galileo, but he gives no details. No reader of "Paradise Lost" will have failed to notice numerous allusions to the great discoveries of this time contained in that sublime poem. Galileo's mind continued to be intent on scientific problems almost to the day of his death, which occurred on the 8th of January, 1642, when he was in the seventy-eighth year of his age.

Galileo's personal character and private virtues secured for him during his lifetime the warm attachment of many friends, while his scientific character, and the number and importance of his discoveries, have won the admiration of all investigators of nature. In person Galileo was about the middle size, square-built, well-proportioned, and of a fair complexion. His countenance usually wore a cheerful and animated expression, and though his temper was irritable, his anger was soon past and the occasion of it quickly forgotten. He was remarkable above all things for that uncompromising love of truth which often manifested itself in direct, and sometimes rash and injudicious, attacks upon the cherished doctrines and established beliefs of his age.

Galileo's house at Arcetri, called "il Giojello" (the Jewel), still stands, about a mile to the south-east of Florence, and it has been preserved in as nearly as possible the same condition as at the day of his death.

> Sacred be
> His villa—justly was it called the Jewel;
> Sacred the lawn where many a cypress threw
> Its length of shadow, while he watched the stars!

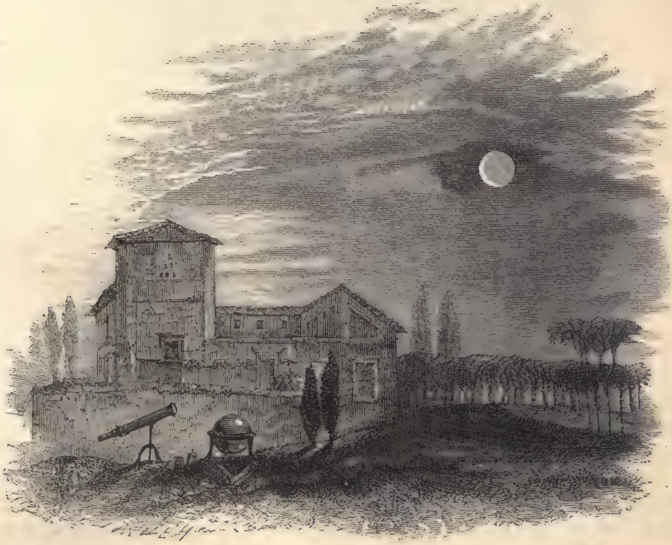

FIG. 50.—GALILEO'S VILLA.

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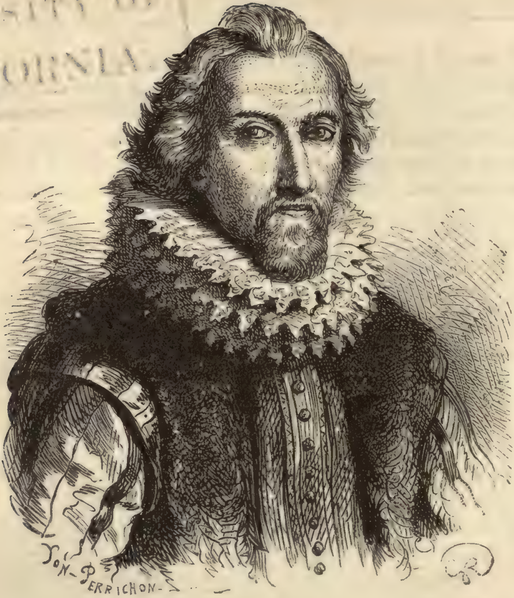

FIG. 51.—FRANCIS BACON.

# CHAPTER VI.

## FRANCIS BACON.

WE have already seen how the teaching and discoveries of Copernicus, Kepler, and Galileo shook the authority of Aristotle, who for two thousand years had reigned paramount in the schools. The doctrines which for so many ages men had been taught to believe as the fundamental truths of nature—such as the unchangeability of the heavens, and the existence of crystalline orbs carrying the planets round the earth—had not only been called in question, but had been proved to have no foundation in fact. And now a contemporary of the renowned astronomers whose names have just been mentioned was preparing the final overthrow of the scholastic philosophy by showing men how to study nature, instead of attending to a mere jargon of words, and eternally wrangling over abstractions called *quiddity*, *individuality*, *formality*, *infinity*, etc., etc. It was indeed time to break the protracted spell, and exorcise the delusive phantasms which had so long beguiled the world by the semblance of scientific truth.

FRANCIS BACON was born in 1561. He entered as a student at

Trinity College, Cambridge, in 1573, and we find him at the age of twenty-three speaking in the House of Commons. Soon afterwards he published his "Essays," by which he at once became famous. He was shortly afterwards appointed Queen's Counsel, and at the trial of the unfortunate Earl of Essex, in 1599, Bacon supported the charge of treason with all his ability, in despite of the bonds of friendship and gratitude. He reached the climax of his ambitious political career under James I., by whom he was appointed Lord Chancellor in 1618, and made a peer with the title of Baron Verulam, which was afterwards changed to that of Viscount St. Albans. In 1620 Bacon was accused of corruption in the exercise of his office, and he at once pleaded guilty. He was removed from his office, and declared incapable of sitting in Parliament and of holding any state appointment. Just before his fall the "*Novum Organum*" was published, and in the period following that event his leisure gave opportunity for the exercise of his literary activity in the composition of "A History of England under the Tudors," and in the preparation of a Digest of the Laws of England, a Philosophical Romance, and other works. He occupied himself also with experiments, and it is to an illness contracted in the prosecution of an inquiry on the effects of cold in preventing putrefaction that Bacon's death is ascribed. On a very cold day, early in the spring of 1626, he alighted from his coach at Highgate in order to try an experiment which had occurred to him. "He went into a cottage, bought a fowl, and with his own hands stuffed it with snow. While thus engaged, he felt a sudden chill, and was so much indisposed that it was impossible for him to return to Gray's Inn. The Earl of Arundel, with whom he was well acquainted, had a house at Highgate: to that house Bacon was carried. The earl was absent, but the servants who were in charge of the place showed great respect and attention to the illustrious guest. Here, after an illness of about a week, he expired early on the morning of Easter Day, 1626. His mind appears to have retained its strength and liveliness to the end. He did not even forget the fowl which caused his death. In the last letter that he ever wrote, with fingers which could not steadily hold the pen, he did not omit to mention that the experiment of the snow had succeeded 'excellently well.'" Thus, as Macaulay remarks, the great apostle of experimental philosophy was destined to be its martyr. It is curious that Bacon's last experiment has, in our own time, borne that fruit of practical utility which he declared should be the great object of science: we allude to the fact that large quantities of meat preserved by cold are now brought across the Atlantic and delivered to the consumers in England in a perfectly fresh condition.

That Bacon's writings exercised a great influence on the development of science is generally admitted, but the nature of that influence has often been misunderstood, and by some its value has been underestimated. Thus it is an error to suppose that Bacon was the inventor of a new process for arriving at truth which is called the Inductive Method. The principles of the inductive philosophy were not only acted upon but distinctly expressed by many authors before Bacon. As we have already stated, Gilbert's treatise on magnetism is an admirable example of the inductive method. We have seen Palissy the potter consciously practising the same method in his researches; it was by the inductive road that Copernicus obtained his results; Tycho Brahe advised Kepler "first to lay a solid foundation for his views by actual observation, and then, ascending from these, to strive to reach the causes of things." Leonardo da Vinci expresses clearly the necessity of following the same method. "In treating any particular subject, I would first of all make some experiments, because my design is first to refer to experiment, and then to demonstrate why bodies are constrained to act in such a manner. This is the method we ought to follow in investigating the phenomena of nature. It is very true that nature begins by reasoning and ends with experiment, but it matters not; we must take the opposite course; as I have said, we must begin by experiment, and endeavour by its means to discover general principles. Theory is the general, experiments are the soldiers. The interpreter of the works of nature is experiment; that is never wrong. It is our judgment which is sometimes deceived, because we are expecting results which experiment refuses to give. We must consult experiment, and vary the circumstances till we have deduced general rules, for it alone can furnish us with them. But you will ask, What is the use of these general rules? I answer, that they direct us in our inquiries into nature and the operations of art. They keep us from deceiving ourselves and others by promising ourselves results which we can never obtain." We find some of the ancient philosophers distinctly describing inductive methods, but, as we have seen, they had no idea of their importance in science. Bacon was therefore by no means the first person who explained the inductive method, and, so far as its use is concerned, it can easily be shown that it has been practised by every human being since the world began. It is precisely this mental process which is applied to the most ordinary questions of daily life. The brilliant author from whose essay on Lord Bacon we have already quoted a few sentences, presents this truth in an amusing way: "A plain man finds his stomach out of order. He never heard Lord Bacon's name; but he proceeds in the strictest conformity with the rules laid down in the second book of the '*Novum Organum*,' and satisfies himself that minced pies have done the mischief. 'I ate minced pies on Monday and Wednesday, and I was kept awake by indigestion all night.' This is the *comparentia ad intellectum instantiarum convenientium.* 'I did not eat any on Tuesday and Friday, and I was quite well.' This is the *comparentia instantiarum in proximo quæ natura data privantur.* 'I ate very sparingly of them on Sunday, and was very slightly indisposed in the evening. But on Christmas Day I almost dined on them, and was so ill that I was in great danger.' This is the *comparentia instantiarum secundum magis et minus.* 'It cannot have been the brandy which I took with them, for I have drunk brandy daily for years without being the worse for it.' This is the *rejectio naturarum.* Our invalid then proceeds to what is termed by Bacon the *Vindemiatio*, and pronounces that minced pies do not agree with him."

Bacon, however, was certainly the first to analyse the inductive methods of reasoning with minuteness and accuracy, to bring into the form of a declared system those principles which, though they had been adopted in practice, had never before been viewed in their mutual relation and dependence. It is no detraction from the merit of Bacon's work that no succeeding philosopher perhaps ever expressly marshalled his facts under the heads of *instantiæ, prærogativæ*, etc. The value of a grammar of a language is not lessened by the fact that its rules are never quoted by the writer of the language. A system of rhetoric may possess the highest excellence, although there may be many an able orator altogether unconscious of the contents or existence of any analysis of his art. Bacon doubtless set too high a value upon his rules when he supposed that a mere acquaintance with them would certainly guide a person, otherwise unpossessed of the genius for scientific discovery, to the knowledge of new truths. Indeed, he altogether failed himself to make any discovery, although as an illustration of his methods he applied his rules to the results of many experiments he made on heat and cold. This is not to be wondered at, if we consider the dearth of sufficient data in facts and experiments. At that time, for instance, the thermometer had not come into use, and it was in fact nearly a century afterwards before this instrument was graduated and made a comparable measure of temperatures. But it is surprising that Bacon should have failed to recognize the best inductive labours of his own time; for he rejected the Copernican theory, and refused to acknowledge the discoveries of Gilbert.

The true service which Bacon rendered to science consisted not only in the completeness of his analysis of the inductive reasoning, but more particularly in his clear recognition and weighty declaration of the principle that induction is the *only* basis upon which scientific truths can rest, and that all who desire to arrive at useful discoveries must travel by that road. "He was not the maker of that road; he was not the discoverer of that road; he was not the person who first surveyed and mapped that road. But he was the person who first called the public attention to an inexhaustible mine of wealth which had been utterly neglected, and which was accessible by that road alone." The ancient philosophers and their successors could dispense with induction, or afford to use it carelessly, because their speculations were never brought to the test of a comparison with fact. The objects they had in view did not lead them to seek for any verification of their doctrines in the world around them. They pursued science as a purely intellectual culture, and sought to raise the mind by contemplations withdrawn altogether from the sphere of visible and tangible things. In the Baconian philosophy a strict correspondence of the conclusions with the facts of nature was the essential thing, and only by that could the object sought be obtained. For the end and aim of Bacon's philosophy was practical. He expressly declares that he designed it to contain nothing abstract or useless, and to be concerned with only those things which tend to ameliorate the conditions of human life.

It was this new character given to science, rather than his analysis of inductive reasoning, which makes the appearance of Bacon's great work form an epoch in the history of our subject. When Archimedes applied his mathematical knowledge to practice, and produced contrivances which were the wonder and admiration of his time, he considered that he was descending from the dignity of the philosophical character, and spoke apologetically of his inventions. Bacon, on the other hand, declares that mathematics could claim no higher position than to be the handmaid of other branches of knowledge which contribute directly to the welfare of our race. Macaulay contrasts the philosophy of Plato with that of Bacon, and after giving abundant illustrations of the opposition of their opinions, he thus continues: "To sum up the whole, we should say that the aim of the Platonic philosophy was to exalt man into a god; the aim of the Baconian philosophy was to provide man with what he requires while he continues to be man. The aim of the Platonic philosophy was to raise us above vulgar wants; the aim of the Baconian philosophy was to supply our vulgar wants. The former aim was noble, but the latter was attainable. Plato drew a good bow, but, like Acestes in Virgil, he aimed at the stars; and therefore, though there was no want of strength and skill, the shot was thrown away. His arrow was indeed followed by a track of dazzling radiance, but it struck nothing. Bacon fixed his eye on a mark which was placed on the earth, and within bowshot, and he hit it in the white. The philosophy of Plato began in words and ended in words—noble words, indeed—words such as were to be expected from the finest of human intellects exercising boundless dominion over the finest of human languages. The philosophy of Bacon began in observations and ended in arts."

The greatest of Bacon's works, written, according to the practice of his age, in Latin, is entitled "*Novum Organon Scientiarum*;" literally, *The New Instrument of the Sciences*, or, as we should perhaps say, *A New Method of Studying the Sciences*. In this work the author advocates and explains the method of *Induction* as the true plan for the investigation of nature. The word *Induction* literally means *a bringing in*, or *a laying together*, and the word is significant of the careful and patient bringing together for comparison of the facts and instances, in order to discover some general principle applicable to the whole of them. Most of the notions which in Bacon's time passed current as science would not bear examination on inductive principles. At the best they were but attempts at generalization, founded on vague and insufficient observations; and often they were nothing better than preconceived ideas and assumptions, so fantastical that nothing but the prescription of authority and the sanction of antiquity could ever have secured their acceptance by successive generations of thinking men.

In showing the importance of the inductive method, Bacon lays down this fundamental principle regarding the interpretation of nature: "*Man, as the minister and interpreter of nature, can act and understand only in proportion as he observes the order of nature; more he can neither know or do.*" Now, before this principle could find acceptance a mass of prejudice had to be cleared away, for men could not at once abandon their vast and elaborate, but empty and baseless systems, to seek a solid foundation in ample accumulations of the particular facts which the older philosophy altogether disdained. Bacon accordingly devotes the first part of the "*Novum Organum*" to an attempt to remove ancient prejudices, in order to prepare for the reception of the second part, in which the new method is unfolded. Of these two divisions of the work we shall now present a short account.

In some prefatory observations Bacon points out that the mode of searching for truth which had been in vogue was at the best by hasty observation of a few particulars, from which a general conclusion was immediately jumped at, and from the propositions thus rashly adopted everything was deduced. The process was thus rapid and compendious, and was excellently suited for disputations, as the supposed general principles formed so many pivots round which the arguments could turn continually. But by this method it was impossible that knowledge and science could advance; nay, the true method was the very reverse of this, namely, to advance gradually from the perceptions which the senses give us of the particular instances, to some principles of a generality one degree higher; from these to proceed to other principles of still higher generality; and so until at length we reach some universal principle as a grand and final conclusion. Thus only can we arrive at clear and well-defined principles, which Nature herself will not refuse to acknowledge.

In his examination of the sources of the prejudices and errors that oppose the progress of truth, and which Bacon with characteristic quaintness calls "the *idols*, or false notions of the mind," we find a keenness of insight which has never been surpassed. He divides the *idols* into four classes, termed respectively by him idols of the tribe, idols of the den, idols of the market-place, and idols of the theatre. What is the nature of the classification which Bacon intended by these fanciful titles will appear when we briefly mention some of the sources of error he discusses under each head, adding such further illustrations as may serve to show the application of his views. The enumeration will be the more valuable to the general reader, as the idols at which

Bacon aimed his blows belong not more to the temple of science than to the inner sanctuary of the human mind. Seekers after knowledge in other regions than that of physical science have often paid to the idols the homage which ought to have been offered only at the shrine of truth; and the history of science in times past, as well as in our own, will often show us the honest inquirer unconsciously bowing the knee to one or other of the false images.

The *idols of the tribe* (*idola tribûs*) are so named because they are common to the whole tribe or race of mankind, inasmuch as they arise from the very nature of the human understanding. Our author illustrates his meaning by a beautiful simile, rising almost into poetry: "The understanding of man is like a mirror whose surface is not true, and so mixing its own imperfection with the nature of things, it distorts and perverts them." He instances our tendency to expect in nature uniformities and correspondences, which are in reality only the reflections of our own conceptions. Thus the ancient astronomers supposed that the planets must move in perfect circles and with uniform velocity, and strove hard to reconcile their observations with this gratuitous assumption. Many current axioms—it may here be remarked—are in reality fallacies arising from our assuming that the order of our ideas must be the order of nature, or, in other words, supposing that the subjective laws which obtain in our own mind apply likewise to the world of external things. Such are the sayings: "Nature always acts by the simplest means;" "whatever can be thought of apart exists apart;" "a thing cannot act where it is not." Of this nature, perhaps, was the fallacy which led Plato to ascribe real external existence to the mental abstractions he called "the Ideas." The tendency which still so often misleads men of science into subordinating the true interpretation of facts to a well-rounded symmetrical system may be ranked under this head. To the idols of the tribe belong also those prepossessions with regard to some favourite notion; as when men have perceived in the fulfilment of dreams, prophecies, and astrological predictions, a confirmation of the validity of such things, because they have simply overlooked the far more numerous cases in which no fulfilment took place. Another cause of error mentioned by Bacon under this head is the influence of the feelings on the will and the intellect. "The light of the understanding," he says, "is not a dry or pure light, but it receives a tincture from the will and the affections; and it forms the sciences accordingly, for men are most willing to believe what they most desire." In the restless activity which prompts our minds to grasp at what is beyond their power lies another source of our errors. We weary our understandings with attempts to comprehend such ideas as space, time, eternity, and infinity, and by seeking to discover uses, ends, or final causes of things. The tendency of the mind towards abstractions and generalizations is also mentioned by Bacon, among the idols of the tribe. The imperfection of our senses and means of observation he names as another cause of error.

The *idols of the cave* (*idola specûs*) are those prejudices which are peculiar to each individual mind. "For, in addition to the general waywardness of human nature, every man has his own peculiar *den* or *cavern*, which breaks or corrupts the light of nature, either on account of his constitution and disposition of mind, his education and the society he keeps, his course of reading and the authorities he most respects, his peculiar impressions, as made on a mind that is pre-occupied and prepossessed, or is in a calm and unbiassed frame; so that the human spirit, as it is differently disposed in different individuals, is a thing fluctuating, disorderly, and almost accidental." Speaking elsewhere of this class of prejudices, Bacon adopts his former metaphor, and compares each mind to "a glass with its surface differently cut, so as differently to receive, reflect, and refract the rays of light that fall upon it." Some minds, he remarks, are quick at perceiving the differences of things, others catch the similarities; each of these tendencies may run into excess. The particular studies, also, to which a man is devoted, may warp his judgment in other pursuits.

The third class of prejudices are denominated the *idols of the market-place* (*idola fori*), and under this title Bacon ranges the errors that arise in the use of language as the means of intercourse between men. He pronounces this class of prejudices the most troublesome of all. Words are, for the most part, accommodated to the notions of the common people, and they define things only by those particulars that are most obvious to ordinary understandings. Perhaps no form of error has been and is more persistent than the tendency to suppose that whatever has a name has a separate and distinct existence. Such words as *fate*, *chance*, *fortune*, *nature*, etc., are not the names of real beings, as the reader will perceive on reflection. *Time* is another word which is very commonly but wrongly supposed to indicate some real *thing*, whereas it implies merely a *relation*.

The *idols of the theatre* (*idola theatri*) are the illusions which arise from the various systems of philosophy, which Bacon compares to so many stage plays, exhibiting nothing but fictitious and theatrical worlds; and there may, he says, still be invented many other fables of this kind. The idols of this class do not naturally possess men's minds, but are therein set up by their own labour and study. Bacon divides these visionary systems of philosophy into three kinds—the *sophistical*, the *empirical*, and the *superstitious*. The *sophistical* philosophies are those which are founded on a few hasty observations, the inventor supplying the greater part of the system from his own mind: the physical theories of Aristotle are of this kind. The *empirical* systems may repose on well-ordered experiments, but their speculations are carried far beyond the range warranted by the ascertained facts: thus, the older chemists founded on a very few simple experiments vast speculative systems of knowledge concerning the four elements. The *superstitious* philosophies are those in which scientific theories are blended with theological systems. As an example of the delusions wrought by this *idolum*, we may refer to cases in which persons have attempted to deduce a physical theory of the world from the Mosaic cosmogony.

The second part of the "*Novum Organum*" treats of the method by which the laws of nature are to be sought. Bacon borrows from the Platonists the word "form" (*forma*); but, unlike Plato, he seeks his forms in the material world, and not out of it. By "forms" Bacon means the causes or general laws of phenomena: thus, the law or mode of action by which such things as heat, light, transparency, etc., are severally produced, are the "forms" of these phenomena. "The form of any nature is such, that where *it* is, the given nature must infallibly be. The form is perpetually present where that nature is present, ascertains it universally and accompanies it everywhere. Again, this form is such that, when removed, the given nature infallibly vanishes; therefore the form is perpetually wanting where that nature is wanting, and thus confirms its presence or absence, and comes and goes with that nature alone."

The discovery of the "forms" of phenomena involves, according to Bacon, the discovery of the *latent process* (*latens processus*) and of the *hidden structure* (*latens schematismus*). What he means by these terms may be thus exemplified: When a cannon is fired, the action appears instantaneous; yet we know that in reality there must be between the application of the match and the expulsion of the projectile a series of actions; and we can theoretically resolve the discharge of the piece into chemical separation of the atoms of the saltpetre, and combination of some of these with carbon and sulphur atoms, accompanied by a change from the solid to the gaseous state, the movement and impulse of the molecules of gaseous matter, and motion thereby communicated to the projectile, which moves forward with a velocity gradually increasing from zero to that with which it leaves the gun. It is this continuous series of actions which Bacon would in this case call the *latent process*. Again, we may consider the form of a crystal, its transparency, colour, optical properties, etc., as depending upon the shape and arrangement of its unseen and ultimate parts and atoms. This inner structure of bodies Bacon denominates their *latent schematism;* and not only did he consider that the knowledge of this was attainable, but he declared that it could only be by such knowledge that men could "superinduce a new nature on a given body so as to change it into another body." It by no means follows, however, that if we were acquainted, for instance, with the nature of that inner structure of glass or of crystals which causes these substances to be transparent, we should have it in our power to impart at pleasure this structure to any given substances. As a matter of fact, however, we are as ignorant of the *latens schematismus* which is concerned in the colour, transparency, crystalline form, and other physical properties of bodies, as in Bacon's time; and the *latens processus* of the phenomena of gravitation, magnetism, electricity, is completely unknown. And the real ultimate latent processes are doubtless beyond the reach of human intelligence altogether, for the knowledge of them would be the knowledge of things absolutely or as they are in themselves—an admitted impossibility. Indeed, the reflective reader will perceive that in the illustration we have given of the *latent process*, our knowledge amounts to nothing more than the sequences we term cause and effect, and that between each of these sequences there must be unknown latent processes, and so on *ad infinitum*. A similar observation will apply to the *latent structure*. Much ingenuity is constantly being expended in devising schemes of atomic and molecular structure which may account for observed phenomena, and the entirely hypothetical nature of these conceptions is liable to be lost sight of, the hypotheses being treated as realities of nature.

Bacon illustrates the inductive method by an inquiry of his own into the *form of heat;* in other words, by an investigation into the *cause of heat*. The first step consists in arranging in certain lists or *tables* all the experiments and observations relating to heat. The first table enumerates those instances which "agree in possessing the nature of heat." Such are the sun's rays, fiery meteors, lightning, flame, hot springs, sparks struck out by collision, green and moist plants when pressed together (as hay, etc.), slaked lime, the bodies of animals. The second table is negative, that is, it is a list of things which have a near relation and resemblance to the things mentioned in the first table, heat alone excepted, in which these instances are to all sense wanting. Thus to the affirmative instance of the sun's rays in the first table there are the parallel negative instances of the rays of the moon, of stars, and of comets, since these, like the rays of the sun, are luminous, but are without heat. The third table consists of a comparison of the degrees of heat found in different substances. The first things considered are such as discover no heat to the touch, but yet seem to have "a disposition and preparation toward actual heat." Quicklime, green plants, acrid vegetables, etc., are put down as examples. The first degree of heat sensible to the touch Bacon considers to be the warmth of animals, and he inquires into the comparative heat of the different kinds of animals. The degrees of heat in various flames are then considered, as in the flames of alcohol, of wood, of oil, of sulphur, of gunpowder, and of lightning.

The three tables containing a great number of (1st) positive, (2nd) negative, and (3rd) comparative examples of heat are designed "to present a view of instances to the understanding;" and this view having been obtained, *induction* is put in practice. "Upon a particular and general view of all the instances, some quality or property is to be discovered, on which the nature of the thing in question depends, and which may continually be present or absent, and always increase and decrease with that nature." The first work in the discovery of forms is to throw out or exclude such particular natures as are not found in any instance, or such as are in any instance found where that nature is absent; or again, such as are not found to increase or decrease with the given nature; "and then, after this rejection or exclusion is duly made, the affirmative, solid, true, and well-defined form will remain as the result of the operation, while the volatile opinions go off, as it were, in fume." This exclusion contracts the field of inquiry, and brings the true explanation of the case more within reach. Thus, suppose the subject in question be the "form of transparency," or that quality which is the cause of transparency in bodies: now, since the diamond is transparent, we immediately exclude *fluidity* and also *porosity*, because the diamond is a solid and a dense substance. In a *fourth table* Bacon accordingly proposes to exhibit an example of this exclusion or rejection of natures from the *form of heat*. Thus, as both the sun's rays and a common fire are hot, he excludes both "the terrestrial and the celestial natures" as causes of heat. *Luminosity* is also excluded, because the rays of the moon and stars present light without sensible heat. *Tenuity* as a cause is rejected, because a dense substance like gold can readily be made hot, while air as a thin and subtile substance is generally cool. Other things there are which may also be excluded; but Bacon does not design these tables to be perfect, but to be merely examples. He adds that "the business of exclusion lays the foundation for a genuine induction, which, however, is not perfected till it terminates in the affirmative; for a negative conclusion cannot possibly be perfect. By the guidance of these tables, however, affirmative conclusions may be provisionally put forward, subject to subsequent revision and verification." These affirmative conclusions occupy the *fifth table*, which Bacon quaintly styles "*The First Vintage concerning the Form of Heat*," and he thus concludes it and his example of an investigation: "Let this serve for what we call the first vintage, or an attempt towards interpreting the form of heat which the understanding makes, as we said, by way of permission. The fruit of the first vintage is, in short: Heat is an expansive bridled motion, struggling in the small particles of bodies. But this expansion is modified, so that, while it spreads in circumference, it has a greater tendency upwards. It is also vigorous and active; and as to practice, if in any natural body a motion can be excited which shall dilate or expand, and again recoil or turn back upon itself, so that the dilation shall not proceed equally, but partly prevail and partly be checked, any man may doubtless produce heat; and this may serve as an example of our method of investigating Forms."

It is interesting to observe that Bacon's guess as to the cause or form of heat being motion of the small particles of bodies, has, in modern times, received an ample verification; for the dynamical theory of heat has triumphantly prevailed over all other hypotheses, and proved itself one of the most fertile conceptions of physical science. Unlike Bacon's conclusion, however, the dynamical theory of heat rests on a vast mass of exact experimental research, and is supported by many converging lines of inquiry, as we shall afterwards have occasion to show.

Bacon devotes a part of his great work to illustrations of the various kinds of facts which are of most importance in an inductive inquiry, and such truths he denominates *Prerogative Instances*, dividing them into classes ranged under the three heads of—1st. Instances addressing themselves to the understanding. 2nd. Those which assist the senses. 3rd. Those which conduce to practice. The total number of classes enumerated by Bacon amount to no fewer than twenty-seven; but the distinctions sometimes appear to be arbitrary and fanciful, and sometimes the relations of a given truth to a given inquiry are such that the instance may with equal propriety be referred to several of Bacon's classes. We shall quote the characters of some of the more important of the Prerogative Instances, without following the laborious details of our author.

The first class are the *solitary instances*, and this includes, perhaps, the class of facts most important of all in an inductive inquiry—namely, examples in which the quality or nature under investigation occurs in bodies otherwise as unlike each other as possible. This limits the field of inquiry into the cause of the quality, for the cause must be some one of the few circumstances in which the bodies are alike. For instance, bright colours may be seen in prismatic pieces of glass, in crystals, in ice, in dew-drops, etc. These instances, having nothing in common with coloured bodies such as flowers, minerals, etc., are called *solitary instances* by Bacon, who draws a remarkable conclusion, wherein he appears to be anticipating the results of Newton's famous experiments, for he infers that colours in crystals are nothing but modifications of the rays of light depending upon the degrees of incidences with which these fall upon the crystal, and that the fixed colours of other bodies are due to certain textures or configurations of their surfaces. The class of *solitary instances* comprises also the instances of the kind converse to those just named, viz., those in which bodies resemble each other in every way but in respect to the quality under consideration, which one possesses and the other does not possess. It is plain that in such cases the *form* or cause cannot exist in those things wherein the bodies agree. Bacon illustrates this kind of *solitary* instance by black veins and white veins in marble, and by the variety of colours observed in flowers of the same species; and his infenerce as regards the cause of colours is again that they do not depend on the internal and essential properties of bodies. We may here remark that, while it is comparatively rare to find in nature instances agreeing in all but one particular, it is the special end and aim of the investigator who proceeds by experiments to produce precisely this condition. This is the fundamental principle of the art of experiment, which art was, in Bacon's time, in its infancy, and he seems to have had no adequate conception of its power in the investigation of nature. Hence, in his elaborate enumeration of instances, he refers in general only to facts known by observation. Under one head, *Instantiæ Polychiestæ*, or things generally useful, he refers to experiments and apparatus, and proposes to treat particularly of these in a subsequent part of his work.

Two correlative classes of instances are the *Glaring* (*instantiæ ostensivæ*, *eluscentiæ*, or *predominantes*) and the *Obscure* (*instantiæ clandestinæ*, also fancifully called *instantiæ crepusculi*, twilight instances). The former class are those which exhibit the quality in its greatest intensity, predominating over all others which usually obscure or counteract it. The latter includes the cases in which the quality shows itself in the faintest manner and most imperfect state. These two classes of instances come under what are now called *the extreme cases*, and the importance of instances of this kind in verifying an induction has been fully recognized; for, "by placing our conclusions, as it were, in violent circumstances, they try their temper and bring their vigour to the test." An example—not drawn, however, from Bacon himself—will enable the reader to perceive the force of an extreme case. When Galileo had concluded, from the comparison of a great number of facts, that the accelerating power of gravity is the same in every kind of substance, he endeavoured to demonstrate the fact by letting light and heavy bodies of different kinds fall from a tower at the same instant, when they were observed to strike the ground nearly at the same time, the heavier being, however, somewhat in advance of the others. But when the bodies were extremely light substances, such as cork or feathers, the experiment appeared to contradict the induction instead of confirming it; but Galileo properly attributed the difference to an interfering course, namely, the greater proportional resistance of the air. And as there was no means then known by which this interfering cause might be removed, the experiment could not be tried at all in what would have been the most convincing case of all, *the extreme case*. But after the invention of the air-pump, the beautiful experiment, now called that of the *guinea and feather*, was devised. This experiment may be easily understood, even by one who has not witnessed it, by an inspection of Figs. 52 and 53. A gold coin and a feather—the extremes of heaviness and lightness—are placed on little stages, from which they are simultaneously let fall in the tall glass from which the air has been exhausted. It is very curious and convincing to observe the feather falling like lead, and striking the bottom at the same instant as the coin.

A very important class of cases are the *Crucial Instances* (*instantiæ crucis*), which Bacon quaintly names after the *crosses* or guide-posts used to point out roads. "These instances are of such a kind, that when in search of any cause the mind comes to an equilibrium, or is suspended between two or more causes, these facts decide the question by rejecting all the causes but one." No term employed by Bacon has passed into such general use as that by which he designates these instances: we speak in ordinary language of *crucial* tests and crucial *experiments.* One of the crucial experiments which Bacon suggested in illustration of his *instantiæ crucis* is very remarkable, the point to be determined being whether the tendency of bodies downwards to the earth is the result of some mechanism in the bodies themselves, or whether it is the consequence of an attractive power in the earth "by the corporeal mass thereof, or by a collection of bodies of the same nature." He proposes to solve the question by ascertaining whether bodies fall to the earth with greater velocity when nearer to it, and *vice versâ;* and his experiment consists in comparing the time required for the slow descent of the *weight* in a fly-clock when the instrument was placed on a high building and in a deep mine, the standard of comparison being another similar clock, actuated not by

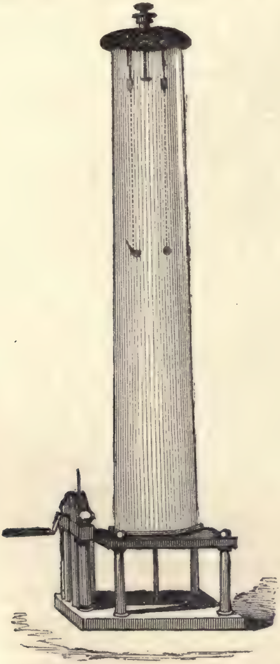

FIG. 52.—THE GUINEA AND FEATHER EXPERIMENT.

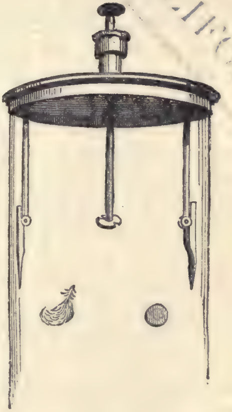

FIG. 53.

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a weight but by a spring. Experiments, which in principle are identical with those proposed by Bacon, have since been carried out by means of pendulums with conclusive results.

Among the "practical instances" we may especially cite the *Instances of Admeasurement* (*instantiæ mensuræ*), subdivided into several classes, such as the *instantiæ radii*, instances of the measuring-rod, indicating measurements of spaces; *instantiæ curriculi*, instances of the course referring to measurement of time; *instantiæ quanti*, relating to measurement of quantity. Now, in practical science this is the kind of experiment most largely adopted and most convincing in its results. In fact, sciences have become exact in proportion as their phenomena have admitted of being expressed in quantitative relations. Hence it has come to pass that in modern science the essential data are precise measurements of time, space, pressures, etc.

Some of the general reflections with which the "*Novum Organum*" closes are among the finest sayings of the great Lord Chancellor. He declares that, among all human actions, noble inventions and the discovery of truth hold the most exalted place. Scientific inventions, he observes, "benefit mankind to the end of time; while the advantages conferred by warriors and statesmen may last in many cases but for a few ages, and may sometimes derive their origin from tumults and the most terrible desolations of war." The advancement of science he pronounces to be an object of ambition nobler by far than private aggrandizement or even patriotism itself. "The first is vulgar and degenerate; the second, that is, the ambition of those who endeavour to raise their own country in the scale of nations, is more noble, but has not less of cupidity. But if any one should labour to restore and enlarge the power and dominion of the whole race of man over the universe of things, this kind of ambition—if we may so call it—is without doubt more wise and dignified than the rest. Now, this power of man over things is entirely founded on arts and sciences." These passages exhibit the great characteristic of the Baconian philosophy, which distinguished it from all the philosophies that preceded it, namely, its practical aim—"works for the benefit of human life."

In examples of the various *prerogatives of instances* we may observe some passages which show that Bacon held some views that appear to anticipate in a remarkable manner more than one famous discovery of his successors, and which certainly show much clearness of perception. Thus, in speaking of the forces acting between bodies at a distance from each other, he says, "It is to be inquired whether there be any magnetic force which acts mutually between the globe and heavy bodies, or between the moon and the sea, or between the starry heaven and the planets, by which they are called and raised to their apogee." Here we seem to have an anticipation of Newton's grand idea of gravitation; and in another passage we have a speculation upon the subsequently proved fact of light requiring time to traverse

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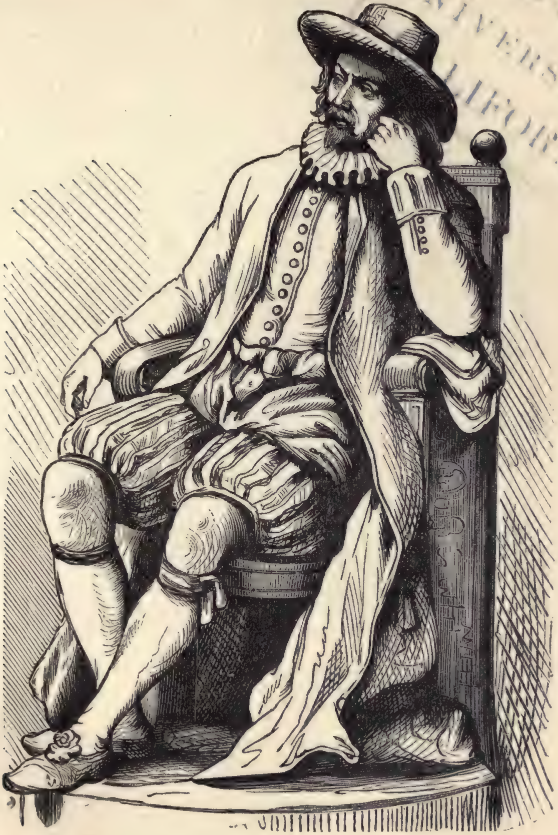

FIG. 54.—STATUE OF FRANCIS BACON IN WESTMINSTER ABBEY.

space: "The consideration of these things produced in me a doubt altogether astounding; viz., whether the face of the serene and starry heavens be seen at the instant it really exists, or not till some time later; and whether there be not, with respect to the heavenly bodies, a true and an apparent time, no less than a true place and an apparent place, as astronomers say, on account of parallax. For it seems impossible that the *species* and rays of the celestial bodies can pass through the immense interval between them and us in an instant, or that they do not even require some considerable portion of time." On the other hand, Bacon strangely rejected the astronomy of Copernicus, and despised the magnetic researches of Gilbert; while his methods failed in his own hands to lead him to any great discoveries. This failure was indeed inevitable from the nature of the object of his investigations, which was to discover the causes of such qualities in bodies as density, colour, porosity, heat. Now, as a matter of fact, our experimental sciences have advanced by investigations into the effects of causes, and not by the search for the unknown causes of effects. Bacon's investigations were, however, put forward rather as examples of his method, constructed with confessedly rude and imperfect materials, than as finished demonstrations of truths. His system was in advance of its subject, and he had to deal in anticipation with possible and theoretical collections of facts, and to examine every conceivable method of attaining scientific truths. We cannot wonder that his treatment of the logic of induction should, under such disadvantages, have been, as we now perceive, inadequate. But it must be remembered that in this nineteenth century we contemplate his work from a point of view vastly different from any that could have been reached in the sixteenth. We are raised, as it were, to a commanding stand-point by the lofty fabric of science which has been created since Bacon's time, and of which he undoubtedly contributed to lay the foundation. Cowley, in his "Ode to the Royal Society," has a fine image, which appears so truly to represent Bacon's position in the history of science, that we cannot forbear to quote the lines :

From these and all long errors of the way,
  In which our wandering predecessors went,
And, like the old Hebrews, many years did stray
  In deserts but of small extent,—
Bacon, like Moses, led us forth at last ;
The barren wilderness he passed ;
Did on the very border stand
Of the blest promised land.
And from the mountain's top of his exalted wit
Saw it himself, and showed us it.
But life did never to one man allow
Time to discover worlds, and conquer too ;
Nor can so short a time sufficient be
To fathom the vast depths of Nature's sea.
The work he did we ought t' admire,
And we 're unjust if we should more require
From his few years, divided 'twixt the excess
Of low affliction and high happiness ;
For who on things remote can fix his sight
That 's always in a triumph or a fight?

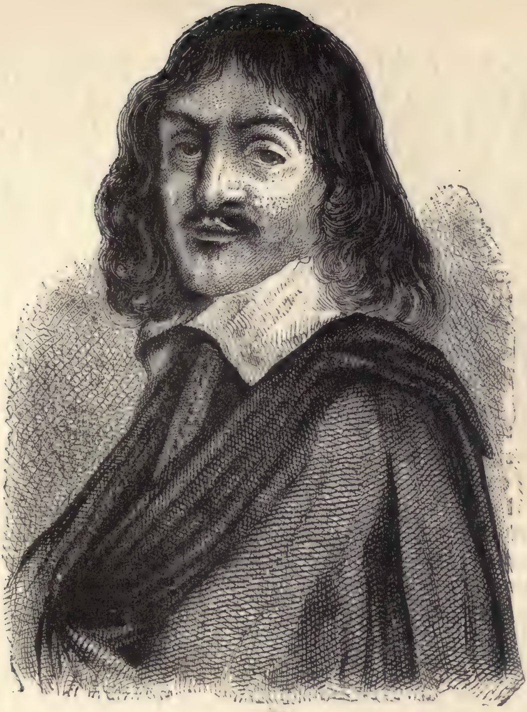

FIG. 55.—DESCARTES.

# CHAPTER VII.

## MATHEMATICAL SCIENCES OF THE SEVENTEENTH CENTURY.

THE progress of astronomy, and of those other branches of physical science which have sometimes been termed *mixed mathematics* (mechanics, hydrostatics, etc.), already in full career by the close of the sixteenth century, received a vast impetus from certain grand discoveries in pure mathematics which distinguish the seventeenth century. This century may indeed be justly termed the golden age of mathematics, for it witnessed the introduction of new methods, which extended the resources of the science in a manner truly marvellous. Before speaking of these capital discoveries and of the men who made them, it may be expedient to mention some of the more important advances made in mathematics since the time of Regiomontanus.

The study of mathematics was introduced into Italy chiefly by a

Franciscan friar, called usually LUCAS DE BORGO. He travelled in the East, and afterwards taught mathematics at Naples, Venice, and Milan. He published, about the middle of the sixteenth century, a book on arithmetic, algebra, and geometry; another on regular figures, polygons, etc.; and a third on the division of a line into sections in extreme and mean ratio. BENEDETTO, another Italian mathematician, published at Turin, in 1585, a treatise on geometrical analysis. Many translations of the works of the ancient geometers, and commentaries on those works, appeared in the sixteenth century. The editors and the commentators contributed greatly to the progress of their science, forming, as they did, a body of pioneers who undertook much useful and necessary work, though it was not of a kind to require any detailed notice in these pages.

A great problem for algebraists has been the discovery of some general methods of solving equations of the higher degrees. Equations of first and second degrees—that is, those in which the unknown quantities enter simply, or under the second power, which are otherwise called respectively *simple* and *quadratic* equations—were plainly capable of solution in every case. But, when attempts were made to solve equations of the third degree (*cubic equations*), it was found that, except in certain obvious cases, no general method could be assigned. The most persevering efforts have been made by mathematicians in vain, to discover a general method of solving equations of every degree; they have hitherto had to content themselves with approximative methods, which however are sufficient in practice. The theory and solution of equations received attention at the hands of the early algebraists. A method of solving a large class of cubic equations was first given to the world by CARDAN in 1545, and this circumstance will perpetuate his name in connection with mathematics, although the discovery was not his own. It is related that Cardan first obtained his knowledge of the method from NICOLO TARTAGLIA, a teacher of mathematics at Venice, who, having revealed it to Cardan under an oath of secrecy, was greatly provoked when Cardan published it as his own discovery. The rule, as explained by Tartaglia, was, however, extended by Cardan, who also supplied a demonstration of its truth, and pointed out a method of approximation for those cases to which the rule could not be applied. The problem of the exact solution of cubic equations has not been carried further than Carden brought it. BOMBELLI, an Italian who also wrote on algebra, pointed out that those problems which involved cubic equations not reducible by Cardan's rule, admit of a geometrical construction by the trisection of a circular arc. The signs for *plus* and *minus*, + —, were first used in a work published at Nuremberg in 1544, and in the first treatise in the English language on algebra, published by Robert Recorde in 1557, the sign of *equality* = appears for the first time. Another great improvement in algebra was made by VIETA, a native of Fontenoy, in France. He held an official appointment in Paris, and yet found time to cultivate mathematics so as to become the great ornament of the French science of his time. He introduced the use of letters of the alphabet to stand for the *known* quantities, and though this may seem a small matter, all those who are practically acquainted with the science will understand how much this contributed to the progress of algebra, which then for the first time became capable of expressing general truths. Vieta also advanced the theory of equation, and introduced some considerable improvements into trigonometry. His mathematical works were published about 1590, and he died in Paris in 1603. THOMAS HARRIOT, an Englishman, made the discovery that all the higher equations are composed of so many simple equations multiplied together as are equal in number to the highest index of the unknown quantity. Hence, the number of roots in an equation is expressed by the index of its highest power: thus, a cubic equation has three roots; an equation of the fourth degree four roots; and so on.

One of the inventions or improvements, which we must consider of the first importance, from its general utility and the conspicuous part it plays in the higher mathematics, was due to JOHN NAPIER, of Merchiston, near Edinburgh. Napier was born of a noble family in 1550, and had the advantage of the best education that could be obtained in his time. He appears to have, at an early age, been attracted to arithmetical and astronomical studies, and experiencing the wearisomeness of making the laborious calculations which astronomical investigations required, he bent his mind towards the discovery of some method of facilitating trigonometrical computations. Here we have an example of one of the ways in which the advancement of one branch of science reacts upon others. In all preceding ages mathematics stood apart from every other learning; and this very abstraction was the boast of its cultivators. But when in the hands of Kepler, Galileo, and others, astronomical, mechanical, and other physical phenomena were reduced to strict quantitative estimation, the problems which presented themselves for investigation found the resources of mathematicians inadequate for their solution; hence mathematical genius was stimulated to supply the deficiencies by the invention of more powerful methods. The mere labour of calculation by such methods as were in use at the beginning of the seventeenth century would have greatly impeded the progress of physical science but for Napier's timely discovery. One of his first devices was for facilitating multiplication by means of certain scales or rules, which were afterwards known as Napier's Bones, from the material of which they were made. But he soon discovered a higher mathematical principle, which he at once reduced to a practical form as simple as it was powerful. And these combinations of simplicity with power will always command the admiration of those who understand the principle and the use of Napier's invention. It is remarkable, also, that the invention came perfect from the hands of its author, whereas most other inventions and discoveries have originally presented themselves in a more or less crude form, and have advanced to perfect and practical shapes by the labour of those who successively improved upon the work of the originators. It has been truly remarked that the subsequent advance of science has found no better method of facilitating computations than *logarithms*, and has only extended the field of their application.

It would, of course, be impossible to explain fully to the non-mathematical reader all the properties and uses of logarithms, but perhaps enough can be stated in a few sentences to give him some idea of the nature of the invention and of some of its uses, although we necessarily adopt only the simplest form which will illustrate the principle. Let him write down, first, a series of numbers which form an *arithmetical progression*—that is, each term is formed by the *addition* of a constant, say 1, to the preceding term; and over the several terms of the series so formed, let him write the terms of a *geometrical* series—that is, one in which each term is formed by *multiplying* the preceding term by a constant number. Supposing, for simplicity, that the multiplier is 2, he will then have the two progressions thus:—

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| 1 | 2 | 4 | 8 | 16 | 32 | 64 | 128 | 256 |
| 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 |

These series should be carried out by the reader to a greater number of terms for the sake of illustration. It will be found that many arithmetical problems regarding the numbers in the upper series can be solved with the greatest ease by very simple operations with the corresponding terms of the lower series. Examples will make this clear. Multiplication and division—To find the product of 32 and 4: $5+2=7$, and above 7 is 128, the product required. To divide 256 by 32: $8-5=3$, and over 3 is 8, the quotient required. Involution and evolution—To find the square of 16: $4\times2=8$, and over 8 is the required square. To extract the square root of 64: $6\div2=3$, and over 3 is 8, the root required. To find the cube of 256: $8\times3=24$, and over 24 in the lower series would be found the required cube, if the progressions are carried out. What is the fourth root of 256? $8\div4=2$, and over 2 is 4, the required fourth root. If the reader attempts a few problems of this kind with an extended series, he will soon be convinced that all calculations into which the numbers of the upper series enter will be greatly facilitated by thus using the lower series; for multiplication then becomes addition; division, subtraction; involution, a single multiplication; evolution, a simple division. He will further see that if the gaps in the upper series were filled up, so that he had 3 between 2 and 4; 5, 6, 7 between 4 and 8, etc., and the arithmetical series were also filled in with its corresponding numbers; it would then be possible to perform arithmetical operations on all numbers with the greatest ease. Nor will it be difficult for him to see that the proper figures for the lower series *could* be obtained with any required degree of approximation by the rules of *ordinary arithmetic.* Suppose, for instance, the ratio or multiplier of the upper series were not 2, but $\sqrt{2}$, then the two series would become

$$1 : \sqrt{2} : 2 : 2\sqrt{2} : 4 : 4\sqrt{2} : 8 : 8\sqrt{2} : 16 : \text{etc.}$$

$$0 : 0{\cdot}5 : 1 : 1{\cdot}5 : 2 : 2{\cdot}5 : 3 : 3{\cdot}5 : 4 : \text{etc.}$$

The term $2\sqrt{2}$ does not equal 3; but it is plain that by continuing sufficiently far the process of finding mean proportionals between the terms of the upper series, and writing under them the corresponding arithmetical means of the terms in the lower series, we can obtain a number as near 3 as we choose, and its corresponding number in the lower series. From this last the arithmetical terms for the numbers 6, 9, 12, 18, etc., could at once be found, and so on with the rest. The lower series of numbers thus found would constitute a system of *logarithms*, so called from λογος, *ratio,* and αριθμος, number. Different systems of logarithms are formed according to the law of the progressions which may be selected. Thus in the common system of logarithms, for good reasons the series run thus, if we write down only the logarithms which are whole numbers:

| | | | | | |
|---|---|---|---|---|---|
| *Numbers:* | 1 | 10 | 100 | 1,000 | 10,000. |
| *Logarithms:* | 0 | 1 | 2 | 3 | 4. |

That is, the logarithm of 10 is one, that of 100 is two, etc.; and it will be observed that the logarithm is the *index* of that *power* of 10 which is equal to the numbers. Intermediate numbers have logarithms which are mixed numbers. For example, the logarithm of 40 is 1·602, and as such fractional numbers now enter also into the notion of *indices* in algebra, we can say $40 = 10^{1{\cdot}602}$. The theory of *indices* was, however, not understood in Napier's time, and therefore greater were Napier's originality and merit in having conceived the idea, and in having discovered the means of finding these numbers.

The logarithms which first presented themselves to Napier's mind were those we now term hyperbolic logarithms, and these were adapted to trigonometrical calculations. It often occurred to him that the system in which the logarithm of 10 is 1, that of 100 2, etc., would be more convenient, and just before his death, which happened in 1614, he explained to Henry Briggs, the Gresham Professor of Mathematics, the nature of his new plan. In 1618 Briggs published a table of the logarithms of all the natural numbers up to 1,000, and in 1624 the logarithms of all numbers from 1 to 20,000, and from 90,000 to 100,000, calculated to fourteen decimal places. Briggs was one of the first professors in the institution, which, about the beginning of the seventeenth century, was founded by a wealthy London merchant named Gresham as a college of science. The Gresham College could for several generations boast of having some of the greatest men of science of the day for its professors; but subsequently the institution lapsed into obscurity, and its emoluments were pocketed by persons who were contented to repeat a merely formal discourse, which until within the last few years was delivered only in the Latin language, and for which it was difficult to obtain any auditors at all!

Passing over many names and many discoveries of mathematical science, of which the plan of the present work will hardly admit space for the mere mention, although they belong to the history of mathematics, we may now proceed to another of the most important steps by which that science advanced during the seventeenth century; and in doing so we come to one of those great names which form the landmarks, as it were, of the intellectual progress of mankind. RENÉ DESCARTES was born at La Haye in Touraine in 1596, and at an early age showed great aptitude in various departments of science and literature. He passed through the usual course of instruction in the Jesuit's College at La Flèche, and he then adopted the profession of arms; and first as a soldier and afterwards as a student he visited in turn nearly every country in Europe, Spain and Portugal being perhaps the only exceptions. He pursued with eagerness a great variety of different studies, including mathematics, physics, metaphysics, anatomy, and physiology. His fame now rests mainly upon his philosophical works and upon the great inventions in geometry which he first announced to the world in a work published in Holland in 1637.

Descartes' philosophy, of which we may here say a few words before relating his mathematical inventions, concurred with Bacon's in destroying the previous systems which relied upon authority. But while Bacon proposed to discover truths for himself by *inductive reasoning* founded on experiences of the external world, Descartes' path to truth was by *deductive* reasoning from his own internal consciousness. The fundamental rule and criterion of truth which Descartes immediately deduces from his celebrated first principle, "*I think, and therefore I exist,*" is that "the things which we conceive very clearly and very distinctly are all true;" only, he naïvely remarks, there is some difficulty in observing which are the things that we do conceive clearly. Descartes, by making deduction the great instrument for his search after truth, was, in a sense, the complement of Bacon; and again, while the former loved mathematics, and placed that science in the first rank, the latter disliked it and considered it as merely the handmaid of physical science. Descartes agreed with Bacon in rejecting authority. He teaches that if you wish to discover truth, you must train your mind to form correct judgments: the rules he gives to this end being that the intellect must be exercised only on those matters to which it is capable by itself of attaining a knowledge. You are not to seek for what others have written or thought before you, but are to rely only upon that which you yourself can recognize as evident truth. Descartes was as declared a foe to prejudices as Bacon himself, and as convinced of the obstinacy with which men cling to them, to the hindering of true knowledge. "I perceived," he remarks, "that a man can more easily burn down his own house than get rid of his prejudices." The biography of Descartes acquaints us step by step with the road along which his intellect travelled in pursuit of truth. At the early age of fifteen he began to doubt; for in the lessons of his masters he found not truths, but only opinions, and he perceived that everything was a matter for disputation. At seventeen he reviewed his stock of acquired truths, and was so dissatisfied that he renounced science altogether, in the conviction that true knowledge was unattainable. But at nineteen he resumed the study of mathematics, for which he had always entertained much inclination. At twenty-one he began to travel, in order to study mankind, the result being that seeing men everywhere deceived by example, custom, and opinion, in order to escape such trammels he at twenty-three betook himself to complete solitude, resolved to clear his mind from all opinions and prejudices. At twenty-four he determined to dispense with all books, and even with all intercourse with men of learning, and to betake himself exclusively to reading the great book of nature, or, in other words, to multiply his own observations and experiments. But again, at the age of twenty-seven he became dissatisfied with physical science and also with mathematics: the former appeared too uncertain, the latter too abstruse. He took up the study of the moral sciences, but again reverted to mathematical study, and for several years devoted himself alternately to abstract sciences and to moral science. Finally, at the age of thirty-two, he set himself to the task of reconstructing philosophy on a new basis.

No book of its size ever gained so much renown for its author or produced a more powerful effect on the progress of science than the short treatise on Geometry which Descartes first published in 1637. Such of our readers as possess a little knowledge of mathematics will of course be already acquainted with the Cartesian geometry, but we think it desirable that our non-mathematical readers should and might form some idea of the nature of Descartes' invention, even if they are unable to realize its full power and significance. A little effort may therefore be profitably devoted to acquiring some notion of the nature of the Cartesian geometry, and, to assist the unlearned reader, we shall select some simple illustrations of the principle, without reference to the method in which the subject is presented in Descartes' own work.

The ancient geometry recognized a few regular curved lines, as the circle, the ellipse, etc., each of which results from a particular mode of construction, and its properties are investigated on principles perfectly independent of the other curves. Hence the invention of a new curve and the investigation of its properties required a very large amount of research and intellectual effort. But when Descartes brought geometry under the dominion of algebra, the former science at once overleapt the narrow bounds which had circumscribed it for ages, and acquired a range of unlimited extent. Instead of the few special curves which had hitherto been the subjects of study, the geometer could now investigate at one and the same time the common properties of large classes of curves, and the number of various curves that might be studied became infinite. The *equation to a curve* was found to be a compendious formula embodying all the properties of the curve, and by general rules, which were the same for all curves whatever, these properties could be deduced from the equation.

In illustration of the manner in which, on the principle of Cartesian geometry, an algebraical equation may represent a curve, and *vice versâ*, we offer these following considerations for the benefit of our non-mathematical readers. Let O Y, O X be two straight lines at right angles to each other, and indefinitely prolonged towards Y and X. We say (purposely limiting the proposition) that any *circle* in the same plane as O Y, O X can be represented by an algebraical equation. Suppose a circle placed as in the diagram Fig. 56, its radius being 5 inches, the distance of C, its centre, from the line O Y being say 11 inches, and from O X, 13 inches. Through C draw line E C perpendicular to O Y, and line C B perpendicular to O X. From P, any point in the circumference (which for simplicity in the first instance we may suppose to be situated in the quadrant of the circle opposite to O), draw lines P F and P D perpendicular to O Y and O X, the last cutting E C at A. Join P C. Then P A C being a right angle, it will be clear that, granting the truth of proposition mentioned on page 13, $\overline{CA}^2+\overline{PA}^2=\overline{CP}^2$, which is evidently the same as saying that $(PF-EC)^2+(PD-BC)^2=\overline{CP}^2$. If instead of P we take some other point in the circumference, as for example P′, and draw lines from it in the same way as for P, the same relations will hold, the only difference being that P′ F′ will be of a different length from P F, and P′ D′ different from P D. If we express these distances, which vary as we pass from one point in the circumference to another, by the letters $x$ and $y$, and express E C, B C, and $\overline{CP}^2$ by their fixed and known values, the equation just given will be written thus:

$$(x-11)^2+(y-13)^2=25\,;$$

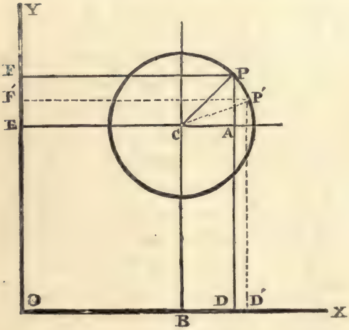

FIG. 56.

and this relation will be true for $x$ and $y$, when these letters represent the length of lines drawn from P′, and also when they represent the same thing for any point whatever in the circumference. So much is plain by mere inspection of the figure as regards any point in the same quadrant as P; and that the equation just given also truly expresses the relations between the two lines drawn upon O X and O Y from *any* point in circumference will become equally obvious on reflection. Hence the equation is true for each and every point in the circumference of the particular circle, and it is an *impossible* equation for any point out of that circumference. Also by putting suitable figures for those which express the distances of the centre from O X and O Y, and for the square of the radius, we can make our equation represent any circle whatever.

We should recommend the reader also to find practically the curve from its equation. Let him draw on a piece of paper two lines at right angles to each other, and from a scale of equal parts set off distances, and draw perpendiculars, so that he thus may obtain as many points as he chooses in the curve. He will first observe that the possible values of $x$ and $y$ lie within certain limits, thus: $y$ may equal any number between 8 and 18. When some assigned value of $x$ replaces that letter in the equation, the corresponding value or *values* of $y$ may be found. Thus: suppose $y$ to be 16, the equation becomes

$$(x-11)^2+(16-13)^2=25$$

and this equation will be found to be satisfied when $x=7$, and also when $x=15$. There are therefore two points in the curve which are both 16 inches from O X, and one of these points is 7 inches and the other 15 inches from O Y. By assigning another definite value to $y$, the corresponding values of $x$ may be found by solving the equation; and thus any number of points in the curve can be found by setting off, parallel to O X and O Y respectively, the several values of $x$ and $y$. Lines drawn from any point in the same manner as P F and P D in Fig. 56, are called the *abscissa* and *ordinate* of that point, and the equation given above expresses an invariable relation between the pairs of *co-ordinates* of any point whatever in the particular circle represented, and a relation which is true only of points in the circumference of that circle. It will not be difficult to see that if, instead of writing 11 and 13, which are the co-ordinates of the centre, and 25, which is the square of the radius of the circle represented in the figure, we substitute a pair of co-ordinates corresponding to the centre, and the square of the radius of any other circle, we can equally represent that circle also by an equation. Therefore, putting generally $a$ and $b$ to stand for the co-ordinates of the centre, and $r$ for the radius of any circle, we know now that every circle may be represented by an equation of this form:

$$(x-a)^2+(y-b)^2=r^2$$

The reader will still more fully appreciate the connection between an equation and a curve if he will practically construct some other curves for himself. Any equation whatever expressing a relation between two variable quantities which admit of an indeterminate number of solutions, corresponds to some curve which may actually be traced on paper. Let us suppose that the two lines O X and O Y have been drawn as in Fig. 57, and that the reader is provided with a scale of equal parts, and that he desires to construct the curve corresponding to the equation

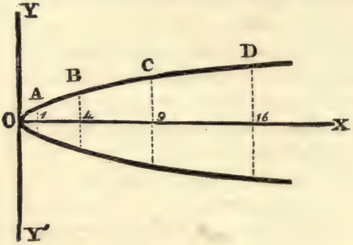

FIG. 57.

$$y^2 = x.$$

Assigning to $x$ the successive values 0, 1, 4, 9, 16, 36, etc., the corresponding *numerical* values of $y$ are plainly 0, 1, 2, 3, 4, 6, etc. These co-ordinates being set off by means of the scale of equal parts, the points O, A, B, C, D will be obtained, and any required number of co-ordinates may be obtained by solving the equation for other values of $x$; thus, when $x = 2$, we have $y = \sqrt{2}$, etc. The curve thus obtained is one branch of a *parabola*, and the other branch, which would be symmetrically situated *below* O Y, is also given by the equation, since $y^2$ has always *two roots numerically equal but of opposite sign.*

The ellipse and the hyperbola are represented by equations which, like those already given for the circle and the parabola, include $x^2$ and $y^2$. And, conversely, an equation of the *second degree* can represent no other curve than one of these four. Equations of the *first degree* represent straight lines. By equations of the *third degree* nearly one hundred different varieties of curves may be represented; while those of the *fourth degree* are capable of representing some thousands of curves of different kinds. The number of possible curves is, in fact, infinite; for no equation between two quantities of the kind called indeterminate, can be proposed, but a corresponding curve can be traced, and its properties deduced from the equation.

The principles of co-ordinate geometry are by no means confined to lines in one plane. Equations can be used to represent curves which do not lie in a plane—as, for instance, a spiral line, like a corkscrew.

In this case *three* co-ordinates are required for each point, and their relations are expressed by *two* equations, each involving a pair of the variables. By another extension of Descartes' idea, *curved surfaces* may be represented, and thus the geometry of solids also is brought within the grasp of algebraical analysis. To explain to the non-mathematical reader the manner in which the geometrical properties of curves, etc., are deducible from their equations, would be to write a treatise on analytical geometry. It must suffice to say, that a great part of such investigation consists in determining the directions, etc., of tangents, and that the geometry of co-ordinates easily furnishes general methods for such problems.

Descartes not only reconstructed the science of geometry and made important improvements in algebraical analysis, but he was also the author of a famous "System of the World," and he published besides treatises on optics, on anatomy, and various metaphysical subjects. His theory of the world was an attempt to explain the causes of all phenomena by principles which were deduced from a few axioms first assumed as the simplest and most obvious propositions. From the fundamental axioms we have already mentioned (page 148), and certain ideas as to the attributes of the Deity, Descartes supposed that he could reason down to the law of nature. He does not, however, wholly reject experiment and observation, to which, of course, inductive methods must be applied. The problem he proposes is the converse of Bacon's, who from effects sought to infer the cause. Descartes expressly says: "We employ experiment, but not as a reason by which anything is proved; for we wish to *deduce* effects from their causes, and not causes from their effects. We appeal to experience only in order that we may be able, out of the innumerable effects which may be produced by the same cause, to direct our attention to one rather than the other." Descartes appears, nevertheless, to have admitted the value of Bacon's method, for in some of his writings he distinctly refers to Bacon in terms of approval. Possibly, in experimenting, he applied the inductive method so far as the object he had in view admitted; but this kind of research was, with Descartes, merely an occasional and subordinate means of supporting his *à priori* theories. He introduced some original views into his speculations on mechanical science—as, for example, the idea of *inertia* regarded not as mere passive resistance to motion, but as a really acting power. In this way he first pointed out with distinctness, that in every case of motion in a circle there must be some *deflecting* force, since, without the operation of such a force, the moving body would move in a straight line; that is, if the deflecting force ceased at any instant to act, the body would, from that instant, move in the direction of the tangent. His notions of inertia, etc., are deduced from a theoretical principle of the permanence of such qualities, and by regarding motion as in some cases a kind of latent quality, he lays it down that the quantity of motion in the universe is always the same. This proposition is nearly equivalent to one of those great generalizations which modern science has produced. Be it observed, however, that Descartes, so far from reaching it by inductions from experiments and measurements, derives it by a short process of deductions from the immutability of the divine attributes, as he conceives them. Such modes of reasoning are, of course, productive of many fallacious conclusions in other cases, and Descartes' writings abound in such fallacies.

It was from his ideas of motion, and of matter endued with certain attributes, as extension, impenetrability, inertia, etc., that Descartes evolved his famous theory of the planetary movements. He conceived that all space was filled with matter, the parts of which had originally been endued with motion in an infinite variety of directions; and as it was impossible that motion in a straight line could result from combination of such various motions, there must have been continued deflections from the rectilinear directions. Hence arose circular motion and its attendant centrifugal force. At length matter came to gather itself for the most part into whirling eddies, or vortexes, in which also the more solid concretions were swept round. And, according to Descartes, the solar system is formed by a vast whirlpool or vortex, sweeping round the sun and carrying with it the planets, which move with different velocities according to the position they occupy in the vortex. Those planets which have satellites are themselves the centres of smaller vortexes, which float in the great vortex. This theory appeared simple, and it was certainly a great improvement on the crystalline spheres of Ptolemy, or the intelligence which Kepler supposed to be seated in each planet in order to guide it in its course. It seemed also to present itself to the understanding with that clearness of conception which the author and his followers esteemed the best criterion of truth. It was discussed, defended, and finally taught in nearly every place of learning in Europe, supplanting in the English universities the doctrines of Aristotle himself. A book embodying Descartes' physical theories was, in fact, for long afterwards the established text-book at the University of Cambridge. Although the vortex theory appeared to account for the planetary motions by a single principle, and that one quite intelligible to the popular mind, for everybody could witness the formation of eddies and whirlpools in water, and observe how in these floating bodies are carried round, its simplicity was apparent only. For several facts were known which the theory in its simple form failed to explain. For instance, Kepler had shown that the planetary orbits were elliptical, hence the vortex of the solar system must also be elliptical. Descartes tries to explain this by the pressure of adjoining vortexes. But the planetary elliptical orbits have not their longest axes in the same direction, and the position of the sun is in a *focus* of each ellipse, and not in the *centre* of each ellipse, Besides, the planets do not revolve in the plane of the sun's equator.

or in any one plane. Again, the facts which are embodied in Kepler's laws (p. 91) remain inconsistent with the motions of vortexes. Much ingenuity has been expended in the endeavour to make the Cartesian theory agree with or explain these and other facts. The original idea was so modified, so many secondary hypotheses had to be superimposed upon it, that all the simplicity which commended the theory to the popular mind was really lost. The hold which the Cartesian theory had taken upon men's minds is apparent from the fact that the vortexes found defenders even after Newton had brought forward his grand conception of universal gravitation. An eminent French *savant*, the author of a history of the *Academie des Sciences*, published so late as 1753 an elaborate defence of Descartes' theory.

The scientific writings of Descartes comprise treatises on music, on anatomy, on meteors, on mechanics, and on optics. In the last-named treatise he gives a very clear expression of the law according to which light is refracted in passing from one transparent medium to another. The first discoverer of the law of refraction was not Descartes, but a Dutchman named WILLEBRORD SNELL. Descartes makes no mention of Snell or any previous investigator of this subject, but it is probable that he may have made the discovery independently. He certainly expresses the law in a more direct manner than Snell; and as this law is the foundation of a very extensive branch of optical science, we will state it as laid down by Descartes. The shaded part of Fig. 58 represents water, the surface of which is supposed to be at right angles to the plane of the paper. Let the line *a c* be a beam of light falling upon the water at *c*. This beam, instead of continuing its course in a straight line, is abruptly bent on entering the water, within which it pursues the path *c b*. Suppose that a line *d e* perpendicular to the surface of the water is drawn through *c*, and that *c* is made the centre of a circle, which cuts off from the incident and refracted rays the equal portions *c a* and *c b*. From *a* draw *a d*, and from *b* draw *b e*, both perpendicular to *d e*. The law of refraction asserts that, whatever may be the inclination of *a c* to the surface, the length of *a d* will have one fixed proportion to the length of *e b*. If the experiment be tried with a ray passing from air into water, it will be found that the length of *e b* is always three-fourths of the length of *a d*; or we may adopt a more concise expression. Premising that the angle *a c d*, which may now be called the

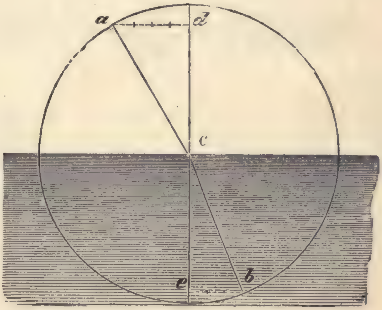

FIG. 58.

angle of incidence, is represented by I, and that the angle *b c e*, similarly denominated the angle of refraction, is represented by R, we may use the language of trigonometry (page 61) and say that $\frac{\sin \text{I}}{\sin \text{R}} = \frac{4}{3} = 1{\cdot}33$. The number 1·33 is called the Refractive Index for air and water, and the refractive index is, of course, always the same for the same pair of media. The general law of refraction is therefore summed up by saying that for *the same media the ratio of the sines of the angles of incidence and of refraction is constant.* It is characteristic of Descartes that this law of refraction was not arrived at by comparing the results of a number of experiments, but was deduced from certain assumed hypothetical principles concerning moving bodies. It may serve to show the danger of thinking that hypotheses are true merely because correct conclusions can be deduced from them, to mention that while Descartes assumed that light moves more rapidly in the denser medium, Fermat, another French mathematician, deduced the law of the sines from the opposite assumption, and a very long discussion was raised upon these so-called demonstrations. Amid the din raised by the contentions of the *à priori* philosophers, a physicist named Petit bethought himself of putting the law of the sines to the test of experiment, and he announced that his results completely verified the law.

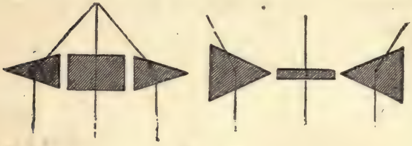

FIG. 59.

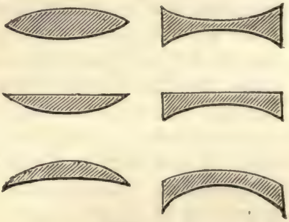

FIG. 60.

In the treatise on dioptrics Descartes also investigates many problems relating to refraction by lenses, and he gives some elegant demonstrations of the shapes which lenses should have, in order that rays falling upon them parallel to their axes should meet in one point. The inventions of the telescope and microscope, which both date from the beginning of the seventeenth century, have in the highest degree aided the progress of scientific research. We have already seen what the telescope could effect in the hands of Galileo, and the microscope was soon afterwards to reveal another world of wonders. Considering the importance of the refracting telescope and microscope in the history of scientific discovery, a brief explanation of the principle of the lens, and of the effect of a combination of lenses, may not here be out of place, and will give us an opportunity of indicating the nature of Descartes' proposed improvement in the lenses of such instruments. Assuming that the reader is familiar with the refractions of a ray of light in passing obliquely from air into glass, and *vice versâ*, he will perceive, by a simple inspection of Fig. 59, the general effect of convex and concave lenses in bending incident parallel rays toward and from the axis. For on comparing these figures with the sections of the various kinds of lenses, Fig. 60, it will be observed that the pieces shown in Fig. 59 accurately represent zones or portions of the two upper lenses. In Fig. 61 the paths of three rays (out of an indefinite number), emerging from the point *b*, are traced through the double convex lens, by which these three (and all the rest emerging from *b*) are made to meet together and cross each other in the point *a*. The point *a* is said to be the focus of the rays from *b*. Though the lines in the diagram are not continued, the rays of light would of course pass through *a*, continuing their passage in straight lines, so that *a* would *in effect* be a luminous point like *b*, but limited as to the direction of the emitted rays. The remaining line in the diagram represents what is called the *axis* of the lens, an imaginary line passing through the centre. The focus of incident rays emerging from a point is situated in the axis, only when the point of emergence is also in the axis; in all other cases the focus is on the side of the axis opposite to the luminous point. This much being understood, the reader can have no difficulty in tracing the formation of images by a lens. Thus in Fig. 62 we have the inverted image of a candle thrown on a wall. Here the image is larger than the object; but by changing the position of the lens to a certain point, an image smaller than the object may be obtained, as shown in Fig. 63. It is easy to verify these facts by a common eyeglass, or a lens of a pair of spectacles. The effect of a combination of two lenses may be studied in Fig. 64; but here the entire pencils of rays emergent from each point must be supplied in

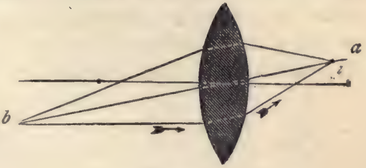

FIG. 61.

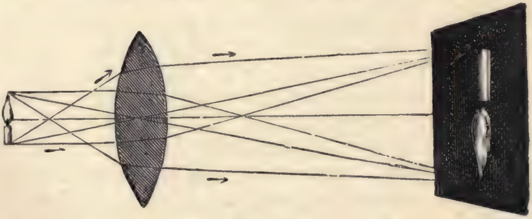

FIG. 62.

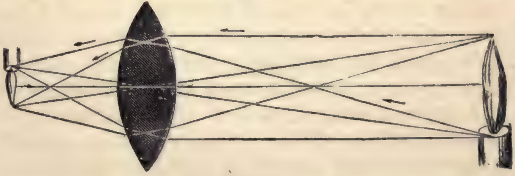

FIG. 63.

imagination, for only the centre rays of three pencils from the points of the object A O B are shown. An inverted image is formed at A′ B′, and is viewed through the second lens as if it were an object. The diagram here does not show the whole course of the rays; in reality, the rays originally emergent from B, for instance, come to a focus at B′, and passing on, enter the second lens, by which they are so refracted that their course after leaving the lens is the same as *as if* they diverged from the point B″. Similarly of rays from the points A and O. Thus, an eye placed behind the second lens is affected in the same way as if the object itself coincided with A″ O″ B″.

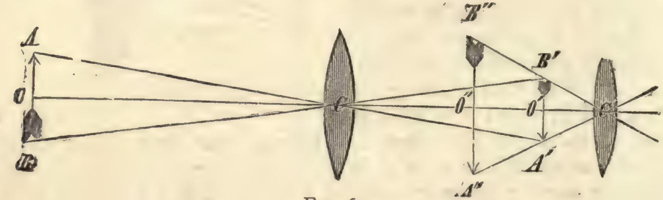

FIG. 64.

In the account of the lens just given we have assumed something as true absolutely which is true only approximately. The rays from the point *b*, Fig. 61, do not all meet together, after passing through the lens, exactly at one point. They do so approximately if the lenses are formed of only small segments of spherical surfaces; but when the curvature is considerable, the aberration is very great. Descartes entered into an investigation of the nature of the curved surface which must be given to the glass in order that parallel rays falling upon it may meet precisely in one point. He found that the section of the required surfaces would be peculiar oval curves, instead of circles as in ordinary lenses, and that in certain cases these ovals became hyperbolas or ellipses. Descartes contrived machines by the aid of which he hoped to grind lenses into the required forms. The practical difficulties were, however, found insurmountable, and although other eminent mathematicians have proposed different arrangements, hyperbolic lenses have never yet been shaped for any useful purpose. This failure is the less to be regretted, as the hyperbolic lens could only be applicable in certain cases, leaving the advantage in others to the spherical form; and the subsequent discovery of the achromatic principle so far removed a more serious defect of the spherical lens, that the greatest obstacle to distinct definition of the images was overcome. The fabrication of the ordinary lens (a spectacle lens, for example) is a comparatively simple operation. It consists in grinding a disc of glass on a revolving piece of iron of a spherical form. Fig. 65 shows a workman engaged in this operation. With his left hand he turns a handle which by means of an endless band gives a very rapid rotatory motion to a convex piece of iron. He applies the disc of glass, previously cemented to a piece of cork, to the revolving tool, which is represented on a large scale in Fig. 66, being either convex as at B or concave as at A. Water and emery powder of progressive degrees of fineness are used until the glass has finally attained its required form and polish.

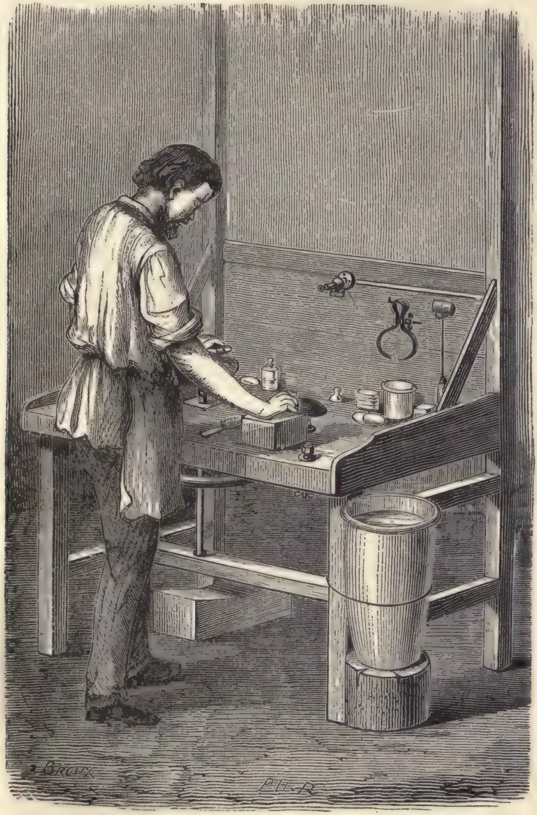

FIG. 65.

To Descartes we are indebted for a much more complete explanation of the rainbow than that formerly given by De Dominis. In his treatises on Meteors, Descartes traced the paths of the rays, and showed why rainbows always appear under certain angles only. He attributes the colours to refraction, and describes the manner in which a coloured band like the rainbow may be thrown on white paper by means of a glass prism.

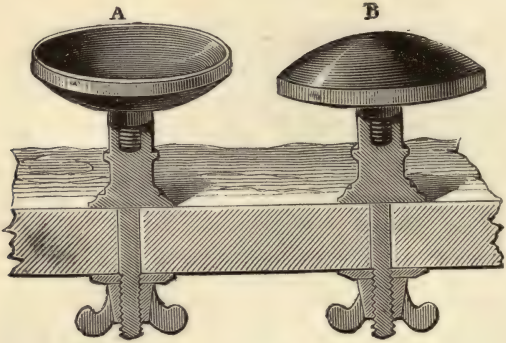

FIG. 66.

How well Descartes could employ the Baconian method may be seen by the way in which he arrived at the conclusion that the colours of the rainbow are due to refraction and to no other cause. He sought, he says, whether there were any other thing in which these colours appear in the same manner, in order that, by comparing one with the other, he might better judge of the cause of the colours. "Then, recollecting that a prism or triangle exhibits similar colours, I examined one like M N P (Fig. 67), where two surfaces, N M and N P, are quite flat, and incline one to another at an angle of about thirty or forty degrees; so that, if the rays of the sun pass through N M at right angles, and thus undergo no sensible refraction, they must undergo a considerable refraction in passing out through N P. Covering one of these two surfaces with a dark body, in which there was a rather narrow opening, D E, I noticed that the rays, passing through this opening and thence falling upon a white cloth or paper F G H, depicted there all the colours of the rainbow, and the red was always depicted towards F and the blue or violet towards H. From this I learnt, first, that the curvature of the surfaces of the drops of water is not necessary for the production of these colours, for the surfaces of the prism are quite flat; nor is a particular magnitude of the angles of refraction necessary, for

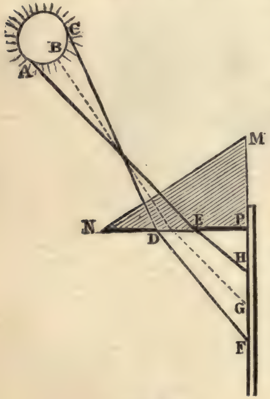

FIG. 67.

this can be altered without changing the colours, and the rays which go towards F may even be bent more than those which are here going towards H, but still they exhibit red colour, while those towards H show blue; nor is reflection necessary, for here there is none; nor a number of refractions, for here there is but one. But I judged that at least one was necessary, and one the effect of which must not be destroyed by a contrary refraction; for experience shows that if the surfaces M N and N P were parallel, the rays, being as much made straight by one as they are bent aside by the other, would produce no colours. I did not doubt that light also was necessary, for without that nothing is seen; and, besides that, I observed that there must be shade or limitation of that light; for, if the opaque body on N P be removed, the colour no longer appears; if the opening be made wider, the colours are not extended, but the middle space on the screen becomes white." Descartes almost seems to be here approaching to Newton's discovery of the composite nature of white light; but he leaves the path of experiment and induction to explain deductively the production of the colours by his theory of light, which he regards as the movement of a very subtile matter, the parts of which must be imagined as little balls rolling into the pores of terrestrial bodies. Rotations, various in direction and velocity, are impressed on these balls, and the greater or less velocity of their rotations affects the eye as different colours.

In a short tract on Mechanics, published in 1668, Descartes states in the clearest terms the true general principle of the so-called mechanical powers and of other machines. He says that all the engines and machines, by help of which a heavy load can be moved by the application of a small power, are founded on one single principle. "The same force which is able to raise a weight, for example, of one hundred pounds, to the height of two feet, can also raise a weight of two hundred pounds to the height of one foot, or one of four hundred pounds the height of half a foot, and so on." The proof of this which he advances is based on the assumed principle that "the effect must always be proportional to the cause;" so that, if it is necessary to employ the action which raises one hundred pounds two feet high to the raising only one foot high of some other weight, that weight must weigh two hundred pounds; for it is the same to raise two hundred pounds one foot as to raise one hundred pounds first one foot, and then one hundred pounds again one foot. Descartes illustrates the general principle by considering the cases of the pulley, the inclined plane, the wedge, the wheel and axle, the screw, and the lever.

In the chapter devoted to Galileo we refrained from giving an account of certain discoveries in mechanical science for which the great Florentine is justly celebrated. This was done with a view of here presenting a more connected account of the rise of modern physical science, with which the names of Galileo and his disciples must be associated. While Galileo was still a young man studying at the

University of Pisa, and as yet ignorant of mathematics, medicine being his intended profession, he happened one day to observe in the cathedral of that city the swinging of a chandelier hanging from the lofty roof. The young philosopher was struck by a circumstance which would have seemed to an ordinary observer a thing of no importance whatever. That was, that the oscillation of the chandelier, whether of smaller or greater extent, appeared to occupy equal intervals of time. He put this observation to the test while still in the church by counting the beats of his pulse occurring during the swing of the chandelier. After having verified the correctness of his observation by repeated experiments, it occurred to him that oscillation of a pendulous body might conversely be advantageously employed by physicians for comparing the rates of the pulses of their patients. He constructed the first pendulum when he put this idea into practice. His arrangement was simply a weight fastened to a string, which was held in the hand at such a point that the vibrations of the weight coincided with the beats of the patient's pulse, when the length of the string was ascertained by a graduated rule. The scale and cord were afterwards connected in one instrument, which under various forms soon came into common use among physicians, and was called a *pulsilogia*. Though Galileo thus proposed the pendulum as a time-measurer, and afterwards employed it for astronomical purposes, its application to clocks was not due to him. The merit of having first made this application has been claimed for several persons, but is generally attributed to Huyghens. Galileo, however, suggested in 1637 a plan by which the pendulum might be made to count its own vibrations. The suggestion was that a projection from the moving part of the pendulum should in passing touch the tooth of a very light wheel, so as at each vibration to move the wheel onwards by a known fraction of its circumference.

Galileo was the first to investigate the laws of motion and force by a union of experimental researches with mathematical reasoning. He proved experimentally that a body falling from rest passes through spaces which are proportional to the *square* of the time of descent. Here is the account of his experiments :—" In a plank of wood about twelve yards long, half a yard broad one way and three-quarters the other, we made upon the narrow side or edge a groove of little more than an inch wide : we cut it very straight, and to make it very smooth we glued upon it a piece of vellum, polished and smoothed as exactly as possible ; and in that we let fall a very hard, round, and smooth brass ball, raising one of the ends of the plank a yard or two at pleasure above the horizontal plane. We observed, in the manner that I shall tell you presently, the time which it spent in running down, and repeated the same observation again and again to assure ourselves of the time, in which we never found any difference ; no, not so much as the tenth part of one beat of the pulse. Having made and settled this experiment, we let the same ball descend through a fourth part only of the length of the groove, and found the measured time to be exactly half the former. Continuing our experiments with other portions of the length, comparing the fall through the whole with the fall through half, two-thirds, three-fourths,—in short, with the fall through any part,—we found by many hundred experiments that the spaces passed over were as the squares of the times, and that this was the case in all inclinations of the plank . . . . As to the estimation of the time, we hung up a great bucket full of water, which through a very small hole pierced in the bottom squirted out a fine thread of water, which we caught in a small glass during the whole time of the different descents; then weighing from time to time in an exact pair of scales the quantity of water caught in this way, the differences and proportions in their weights gave the differences and proportions of the times; and this with such exactness that, as I said before, although the experiments were repeated again and again, they never differed in any degree worth noticing."

From the fact that the spaces fallen through are proportional to the squares of the time, Galileo was able to prove that upon the principle that the projectile falls independently of its forward motion, the path of a projectile must be a parabola. Anticipating objections, he explains that the resistance of the air and the convergence of the action of gravity towards the earth's centre will cause the course of the projectile to be not strictly parabolic. The latter cause will, he remarks, not sensibly affect our experiments. The futility of some objections which had been urged against his experimental proof of the falsity of Aristotle's law of falling bodies is thus shown in the "Dialogue on Motion," where one of the interlocutors is made to assert that if a cannon-ball weighing two hundred pounds, and a musket-ball weighing half a pound, be dropped together from a lofty tower, the former will not anticipate the latter by so much as a foot; "and I would not have you do as some are wont, who fasten upon some saying of mine that may want a hair's breadth of the truth, and under this hair they seek to hide another man's blunder as by a cable. Aristotle says that an iron ball weighing a hundred pounds will fall from a height of a hundred yards while one weighing one pound falls but one yard: I say they will reach the ground together. They find the bigger to anticipate the less by two inches, and under these two inches they seek to hide Aristotle's ninety-nine yards."

Galileo's theory of falling bodies is thus expressed: "A heavy body has by nature an intrinsic principle of moving towards the common centre of heavy things, that is to say, towards the centre of our terrestrial globe, with a motion continually accelerated in such manner that in equal times there are always equal additions of velocity. This is to be understood as holding true only when all accidental and external impediments are removed, amongst which is one that we cannot obviate, namely, the resistance of the medium." He goes on to state that the resistance of the medium (the atmosphere) continually increasing would at length prevent further acceleration of the velocity of the moving body, which would continue its motion at a uniform velocity.

In a treatise, published in 1592, Galileo sets in the clearest light the real advantages of machines, "which," he says, "I have thought it necessary to do, because, if I mistake not, I see almost all mechanics deceiving themselves in the belief that, by the help of a machine, they can raise a greater weight than they are able to lift by the exertion of the same effort without it. Now, if we take any determinate weight, and any force, and any distance whatever, it is beyond doubt that we can move the weight to that distance by means of that force, because, even although the force may be exceedingly small, if we divide the weight into a number of fragments, each of which is not too much for our force, and carry these pieces one by one, at length we shall have removed the whole weight; nor can we reasonably say at the end of our work that this great weight has been removed and carried away by a force less than itself, unless we add that the force has passed several times over the space through which the whole weight has gone but once. From which it appears that the velocity of the force (understanding by velocity the space gone through in a given time) has been as many times greater than that of the weight as the weight is greater than the force; nor can we, on that account, say that a great force is overcome by a small one, contrary to nature: then only might we say that nature is overcome when a small force moves a great weight as swiftly as itself, which we assert to be absolutely impossible with any machine either already or hereafter to be contrived. But since it may occasionally happen that we have but a small force, and want to move a great weight without dividing it into pieces, then we must have recourse to a machine, by means of which we shall remove the given weight with the given force through the required space. But, nevertheless, the force, as before, will have to travel over that very same space as many times repeated as the weight surpasses its power, so that at the end of our work we shall find that we have derived no other benefit from our machine than that we have carried away the same weight altogether which, if divided into pieces, we could have carried without the machine, by the same force, through the same space, in the same time. This is one of the advantages of a machine, because it often happens that we have a lack of force but abundance of time, and that we wish to move great weights all at once." This compensation of force and velocity has been fancifully expressed by saying that Nature cannot be cheated. The same principle is treated of in systematic treatises on mechanics, under the name of "Virtual Velocities."

Galileo explained the concords and dissonances of musical sounds by the concurrence or opposition of vibrations in the air striking upon the drum of the ear. He says that the vibrations due to the sound may be made visible by rubbing the finger round a glass set in a large vessel of water, and if by pressure the note is made suddenly to rise to the octave above, every one of the undulations, which will be seen regularly spreading round the glass, will suddenly split into two, proving that the vibrations that occasion the octave are double those belonging to the single note. He then describes how he accidentally discovered a method of more accurately measuring the relative lengths of sound-waves. He was scraping a brass plate with an iron chisel, and, moving the tool rapidly upon the plate, he occasionally heard a hissing and whistling sound, very shrill and loud, and he observed that whenever this occurred, and then only, the light dust on the plate arranged itself in a long row of small parallel streaks equidistant from each other. In repeated experiments he produced different tones by scraping with greater or less velocity, and he remarked that the streaks produced by the acute sounds stood closer together than those produced by the low notes. Among the sounds were two which, by comparison with the notes of a violin, were found to differ by an exact fifth; and counting the streaks in each case, he found thirty of the one to occupy the same space as forty-five of the other, which is exactly the known proportion of the lengths of identical strings that will yield notes having the interval of a fifth. We shall have occasion on a subsequent page to revert to the subject of vibrating plates in mentioning Chladni's acoustic researches. Galileo goes on to give the general outline of the theory of concords and discords. "The immediate cause of the form of musical intervals is neither the length, the tension, nor the thickness, but the proportion of the number of the undulations of the air which strike upon the drum of the ear, and make it vibrate in the same intervals. Hence we may gather a plausible reason of the different sensations occasioned to us by different couples of sounds, of which we hear some with great pleasure, some with less, and call them accordingly concords, more or less perfect, whilst some excite in us great dissatisfaction, and are called discords. The disagreeable sensation belonging to the latter probably arises from the disorderly manner in which the vibrations strike upon the drum of the ear; so that, for instance, a most cruel discord would be produced by sounding together two strings of which the lengths are to each other as the side and diagonal of a square, which is the discord of the false fifth. On the contrary, agreeable consonances will result from strings of which the number of vibrations made in the same time are commensurable, so that the cartilage of the drum may not undergo the incessant torture of a double inflexion from disagreeing percussions."

Galileo, in his Dialogues, uses an expression invented by the older school of philosophers, namely, *Nature's abhorrence of a vacuum;* but he must not be understood as putting this phrase—which is convenient enough as representing the difficulty of obtaining a vacuous

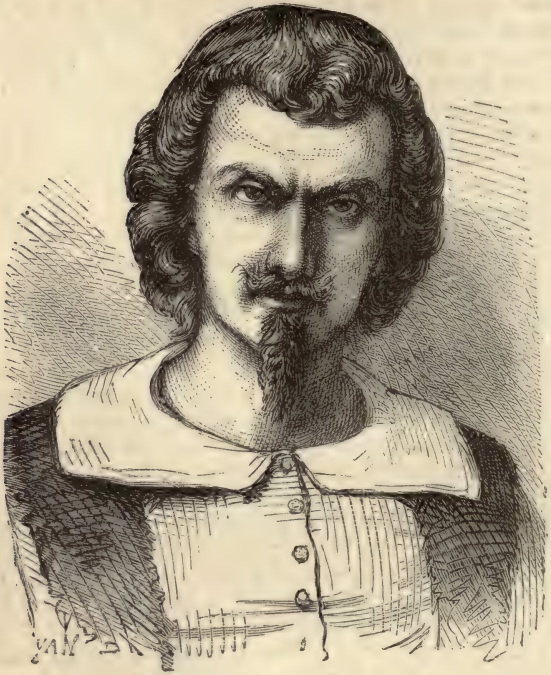

FIG. 68.—TORRICELLI.

space—in the place of the cause of phenomena which the facts known in his time did not sufficiently explain. He even describes a piece of apparatus by which a vacuum might be produced, and the force necessary to produce it might be measured. A story is told of Galileo in connection with the circumstance of a pump failing to draw water from a well more than 33 feet deep. The details of the incident are differently given by different writers. Sometimes it is the pump-maker, who, when Galileo complained of the pump, informs the philosopher that pumps of the ordinary construction would never draw water from a well of a greater depth than 33 feet; sometimes it is Galileo that enlightens the well-sinkers. The illustration opposite is intended to represent that version of the story which speaks of the workmen of the Grand Duke as having constructed a well 40 feet in depth, for the ducal palace, and, the lift-pump having failed to raise the water to the surface, Galileo was applied to, when he informed the well-sinkers that this phenomenon was invariable, adding that Nature's abhorrence of a vacuum did not

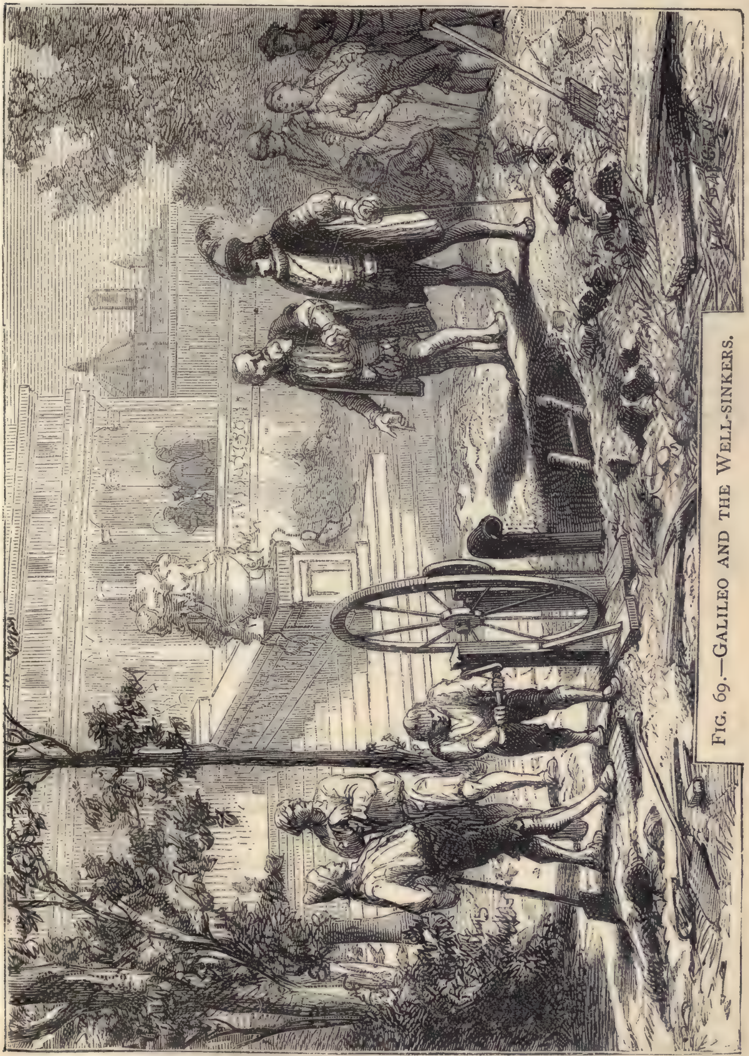

Fig. 69.—Galileo and the Well-sinkers.

extend beyond 33 feet. It has been supposed by some that this was said by the philosopher by way of a sneer at the scholastic philosophy of the vacuum; but it is more probable that Galileo, if indeed he used the expression at all, intended seriously to modify the established formula in accordance with facts. His usual sagacity appears to have been at fault in the explanation of the phenomenon which he suggests in his Dialogues, where he compares the column of water in the barrel of the pump to a rod of metal suspended from its upper end, which may be lengthened until it breaks with its own weight.

The true explanation of the rise of the water in the common pump was first given by a friend and pupil of Galileo, the distinguished TORRICELLI (1608—1647). Torricelli made some additional discoveries in the mechanics of moving bodies, and he investigated some general theorems relating to the centre of gravity and the equilibrium of bodies. He showed also that water issues from a hole in the side or bottom of a vessel with the same velocity that it would acquire in falling from the level of the orifice. The famous experiment which is called by Torricelli's name was devised by him in order to verify his explanation of the rise of the water in an exhausted tube to the height of 33 feet and no more. The column of water, said Torricelli, ascends to such a height that its weight exactly balances the pressure of the atmosphere. In other words, the atmosphere, having weight, presses on the surface of the water in the well; and when this pressure is removed from the liquid within the pump-barrel, the liquid is forced up by the atmospheric pressure outside, until the weight of the column of water produces an equal and counter-pressure. In order to show that the height to which a liquid would rise in an exhausted tube depended on the weight of the liquid, Torricelli devised his famous experiments, which may be easily understood by the aid of Fig. 70. He took a strong glass tube about 3 feet long, and closed at one end. This tube he completely filled with quicksilver, which is a liquid thirteen times heavier than water. Having closed the mouth of the tube by his thumb, pressed against it as shown on the right-hand side of the figure, he inverted it, and plunged its mouth beneath the surface of some more quicksilver contained in a basin *b*, before withdrawing his thumb. When the thumb was then removed from the surface of the tube, the quicksilver immediately descended, until it stood at a certain height within the tube, as at *a*, where it was about 30 inches above the level of the surface of the

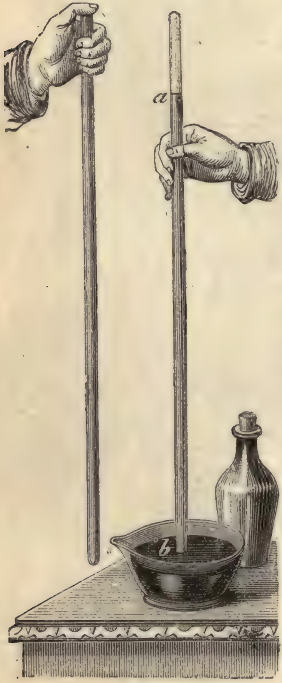

FIG. 70.

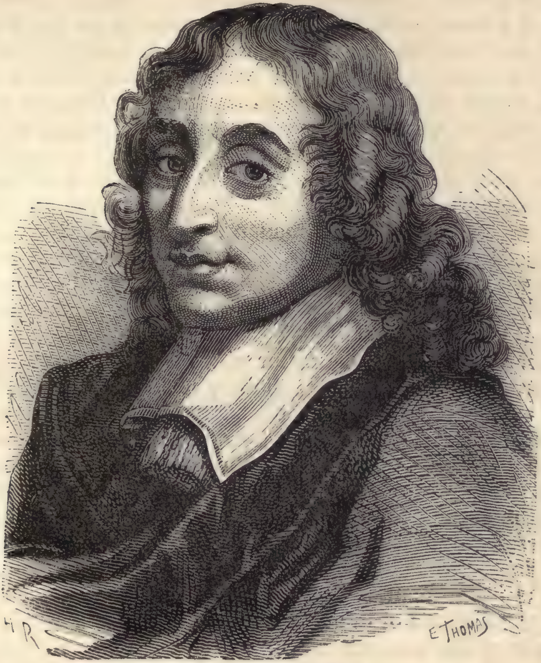

FIG. 71.—BLAISE PASCAL.

liquid in the basin. This confirmed Torricelli's explanation, and abolished at once such notions as suction or Nature's abhorrence of a vacuum. But the experiment created not a little controversy, for the mercury, in descending from the top of the tube, leaves behind it the space known as the Torricellian Vacuum. This vacuum was a stumbling-block to some of the philosophers of that day, and a great dispute arose as to whether Torricelli was right in affirming that the column of mercury was sustained by the pressure of the atmosphere. Torricelli, however, soon remarked certain variations in the height of the column of mercury, and he did not hesitate to attribute these to variations in the atmospheric pressure. We need hardly remind the reader that Torricelli had, in fact, invented the Barometer,—that instrument which has proved of vast utility by giving us the means of measuring the variations of the atmospheric pressure.

The doubts which had been entertained as to Torricelli's explanation were soon set at rest by some experiments instituted by the celebrated BLAISE PASCAL (1623—1662), then a very young man residing at Rouen. Pascal, having learnt the details of Torricelli's experiment, resolved to undertake further researches on this subject, and he made known his results in a treatise entitled "New Experiments on the Vacuum." One of his experiments consisted in using wine, a liquid lighter than water, instead of mercury, the result being that the liquid was maintained at a height proportionately greater than in the case of water. But Pascal caused a crucial experiment to be performed, in order to set at rest the question of the validity of Torricelli's explanation. If, Pascal reasoned, it is the pressure of the air which balances the column of mercury in the tube, the height of the column would be less were the experiment performed on the summit of a mountain. Pascal induced his brother-in-law, Perier, who possessed some acquaintance with scientific experiments, to undertake the application of this test. The mountain selected was the Pay-de-Dôme in Auvergne, and Perier accordingly proceeded to Clermont, and there remained for some months waiting for weather favourable to his object. On the morning of the 20th September, 1648, Perier, accompanied by several other persons, set out on his expedition, taking with him two similar glass tubes, about 4 feet long, and closed at one end, filled with mercury. With these the Torricellian experiment was performed, and the levels of the mercury within and without the tubes having been marked on them with a diamond, the differences of level were measured several times. These differences were found in both tubes to be 26 Paris inches, 3½ lines. One of the tubes was then fixed, and left in the condition of the experiment, and one of the Fathers of the convent was appointed to take care of it, and notice any changes that might occur in the height of the mercury during the day.

The party of observers, carrying with them the second tube, now empty, and a supply of mercury, etc., then set out for the summit of the mountain. Here the Torricellian experiment was repeated exactly as before, and the height of the column of mercury was again carefully measured. It was now only 23 inches, 2 lines. There was thus a difference between the two measurements of 3 inches, 1½ lines (about 3⅓ English inches). The measurement of the column of mercury was several times repeated, under certain variations of the surrounding circumstances, in order to ascertain whether its height was affected by any of these. It was measured in the open air; it was measured under cover of a little chapel; it was measured in a shower; it was measured in a mist; but the result remained unchanged. The party then began to descend, and when they had come about half-way down, it was thought advisable to repeat the experiment, in order to ascertain whether the decrease of the height of the mercurial column was proportional to the elevation at which the experiment was tried. The mercury was found now to have a height of 25 inches; and several other like determinations showed that the mercury stood higher as the level of the place was lower. Towards the close of the day, the party arrived at the convent at Clermont, and there found Father Chastin

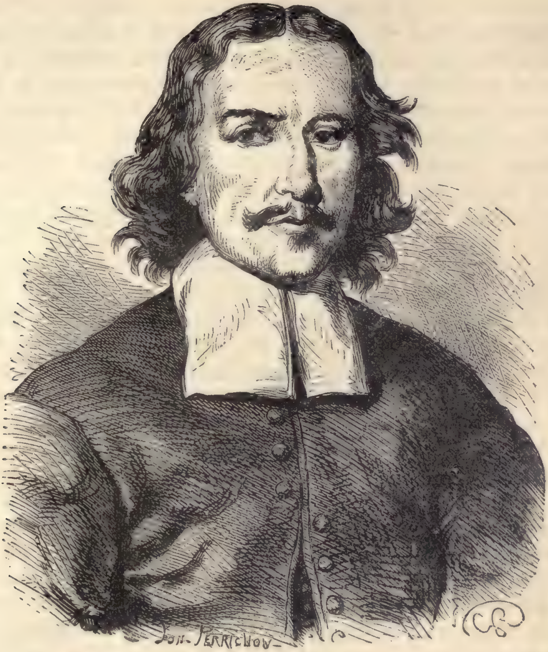

FIG. 72.—VON GUERICKE.

continuing his patient observation of the instrument they had left. He assured them that during their absence no variation had occurred in the height of the mercury in the glass tube. Perier then once more repeated the Torricellian experiment with the apparatus which had been carried up the mountain. The mercury now stood in this tube at the same height as in the morning. The next day a suggestion was made that measurement should be taken of the mercurial column at the top and at the base respectively of the loftiest tower of the Cathedral of Clermont. This was done, and a difference of one-sixth of an inch was observed. Pascal himself, subsequently repeating similar experiments at Paris, was able to observe a difference when the instrument was carried from the cellar to the attic of a private house.

The question as to the pressure of the atmosphere being the cause of the suspension of the mercury in the Torricellian tube was now virtually settled, and thus there was acquired to science an instrument of the greatest value—the barometer. The space above the mercury attracted a great share of the attention of philosophers, and much dis-

-cussion arose as to the possible existence of a vacuum. It was therefore natural that some should busy themselves to devise a contrivance by which a vacuum could be produced. OTTO VON GUERICKE, burgomaster of Magdeburg, it was who solved the problem, and gave us another scientific instrument in the Air-Pump. Like every other discovery of importance, this one was arrived at only by repeated trials and failures. The first idea was to fill a barrel with water, and having completely closed the vessel, except that a tube connected the interior with a pump, to remove the water and thus leave an empty space. When this was done with a barrel strong enough to resist the external pressure, Guericke found that the air passed through the pores of the wood. He then bethought himself of enclosing the barrel in a larger one and filling the space between the two with water. The air still found its way into the interior of the vessel. He then had made a large hollow metallic globe, and without using water this time, he connected this with a pump. The atmospheric pressure was perceived in the effort required to work the pump: the resistance, trifling at first, increased at every stroke, until the united strength of two men could hardly accomplish the task. But when the exhaustion had been carried to a considerable extent, the vessel suddenly collapsed with a loud report, and was crushed as completely as if it had been thrown down from the top of a lofty tower. A second apparatus more carefully constructed gave, however, the result sought for. When finally a strong glass globe had been substituted for the metallic sphere, in order that the effects of a vacuum might be observed, the invention of the air-pump as a scientific instrument was complete. The air-pump as arranged by Guericke consisted of a glass globe connected with the barrel of the pump placed vertically beneath the globe. The pump was worked by a lever, and the whole was supported on a strong frame, as represented in Fig. 73.

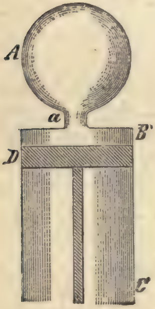

FIG. 73.

The principle on which this apparatus acted will easily be understood by help of the diagram Fig. 73, where A represents the globe to be exhausted, and B C the cylinder, in which the piston D moves up and down. If the piston be drawn down from B to C, the air which before only occupied the space A B expands so as to fill the cylinder as well as the globe. If we suppose the cylinder to have twice the capacity of the globe, the air will thus fill three times its original space. When the piston is at C, let us imagine that all communication between the cylinder and the globe is cut off at *a*. It is plain that the globe would then contain only one-third of the original air, and if the piston is then brought to its former position by pushing it to the top of the barrel, after having opened at B a communication with the external air, in order to allow the air imprisoned in the cylinder to escape as the piston rises, the apparatus is, after the opening at B is closed again, in a position again to remove from the globe A two-thirds of its remaining air by the descent of the piston. Each time that the piston is drawn, two-thirds of whatever air may be in the globe being thus removed, we should have remaining

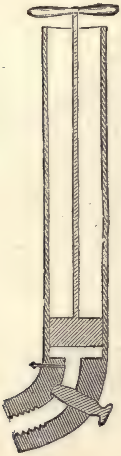

FIG. 74.

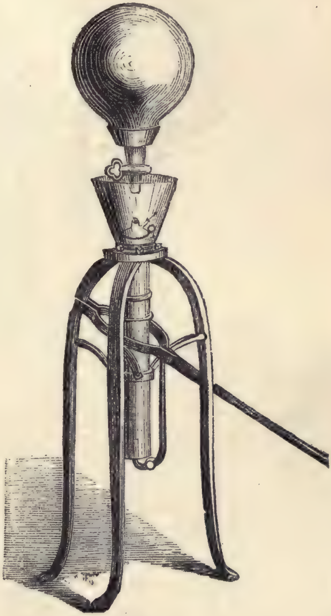

FIG. 75.

after the successive strokes $\frac{1}{3}$, $\frac{1}{9}$, $\frac{1}{27}$, etc., of the original quantity. The mode in which Guericke cut off the communication with the globe was by a stop-cock as shown in Fig. 75, and the air expelled from the cylinder on the rise of the piston escaped through a valve at the top. The stop-cock and valve were surrounded by oil, which filled the conical vessel above the barrel, in order to prevent access of air, which was liable to find its way between the parts of the apparatus. In Fig. 75 the globe is separated and raised a little above the position it would occupy when in connection with the cylinder. Fig. 74 shows another arrangement of the barrel of the air-pump, dispensing with the oil-vessel, and replacing the valve by a small lateral opening, which is closed by hand each time the piston is drawn out. The air-pump subsequently received many improvements, and has been constructed in many different forms. Fig. 76 is a form of air-pump now often seen in cabinets of physical apparatus. Here the pistons contain valves opening upwards, and the cylinders are of glass, in order to display the action of the valves and pistons.

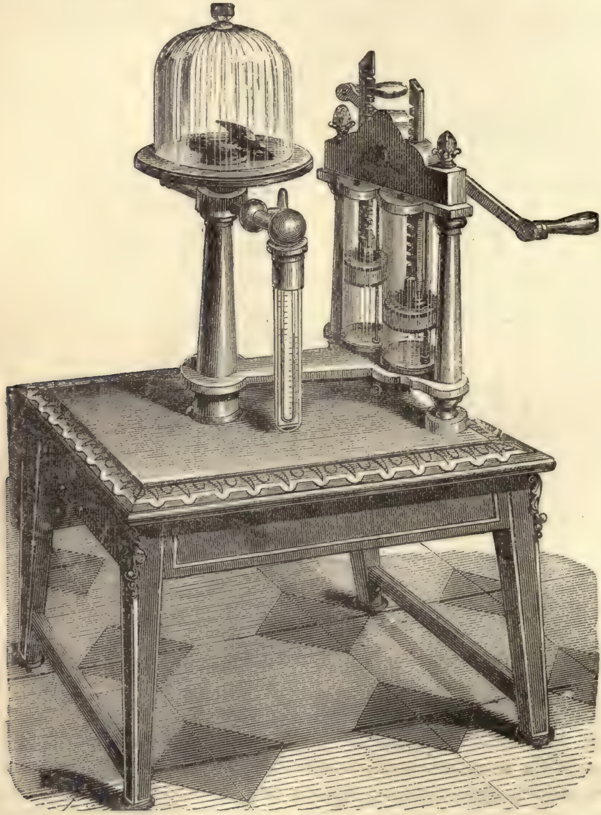

FIG. 76.

The original air-pump was in many ways an extremely imperfect instrument, but in the hands of its inventor it soon served to demonstrate some important scientific truths. Among these we may name the direct proof of the weight of air, and of the pressure exercised by the atmosphere in consequence of that weight. Guericke's experiments in illustration of the pressure of the atmosphere excited the greatest astonishment in the beholders. One of the experiments which most attracted attention has continued to enter into every course of experimental physics since that time. It is still known as the experiment of the *Magdeburg Hemispheres*, and was first performed by Guericke by means of two hollow metallic hemispheres fitted to one another at their edges. When these had been pressed together with a ring of oiled leather, interposed in order to render the junction airtight, the air was withdrawn by the pump from the interior of the globe, and then the pressure of the atmosphere, no longer opposed with an equivalent force by air within, manifested itself by the resistance it opposed to the separation of the hemispheres. The hemispheres used in Guericke's experiments had a diameter of three-fourths of a Magdeburg ell, and the united force of eight horses was unable to separate them after the air had been pumped out of the interior. The subject of Fig. 77 is a representation of the same experiment with larger hemispheres, and is taken from the engraving in the Latin work wherein Guericke announced his discoveries. This book was entitled "*Experimenta nova Magdeburgica de vacuo spatio*," and was published in 1672. The illustration shows a number of horses harnessed to the conjoined hemispheres, dragging them in opposite directions, without being able to separate them.

These experiments excited great interest amongst the scientific men of the time, and especially attracted the attention of Boyle, Hawksbee, Hooke, and Mariotte to the study of the laws which govern the pressures of air and gases. It is notable, too, that in the Magdeburg experiment we have the first direct demonstration of the great force of the atmospheric pressure, which, in the next century, was utilized by Watt. For in Watt's steam-engine it is the pressure of the atmosphere which is the acting power, steam being employed merely as an expedient for removing the atmospheric pressure from one surface of the piston, so that the same pressure, acting on the other side, may manifest its effects unopposed. In other words, Watt used steam only to obtain a vacuum within the cylinder of his engine, and derived the immediate motive power of his machine from the same force which held together the Magdeburg hemispheres.

Enough has been already related to show that with the seventeenth century began that wonderful development of modern physical science which offers so extraordinary a contrast to its unprogressive condition during the preceding ages. We have seen the great changes which the teaching and discoveries of Copernicus and Galileo produced on the notions of mankind concerning the universe, and we have seen how the seventeenth century found science possessed of certain instruments—the telescope, the air-pump, the barometer, and the electrical machine—each of which became the fertile means of discovery by supplying new and important data. Another instrument of like importance, the invention of which also belongs to this period, remains yet to be noticed. The instrument we allude to is the now well-known Thermometer, but the merit of its invention, or rather the application of the principle on

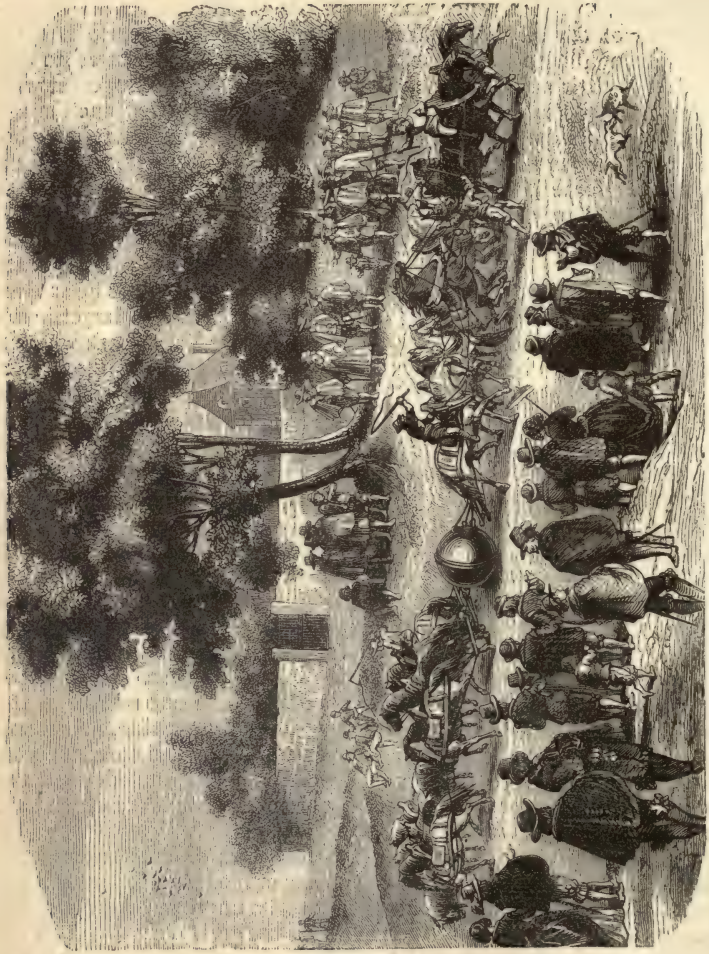

FIG. 77.—THE MAGDEBURG EXPERIMENT ON THE LARGE SCALE.

which it rests, has been claimed for several different persons. Some find evidence that the idea originally occurred to Hero, the Greek mathematician; others claim it for Galileo; but the names of SANTORIO and of DREBBEL are most commonly associated with the invention. In the form of the air-thermometer it is a simple arrangement which might easily present itself independently to the experimenters of the seventeenth century. In a letter written in 1638, Castelli stated that he remembered an experiment shown to him by Galileo more than thirty-five years before. He describes Galileo as taking a small glass bottle, which had a neck 22 inches long and as narrow as a straw. Having warmed the empty bulb or bottle by holding it for some time in his hands, Galileo immersed the mouth of the inverted bottle in water, and then allowing it to become cold by removing his hands, the water rose in the neck of the bottle more than 11 inches above its level in the vessel containing it. Castelli then expressly states that Galileo applied this principle in the construction of an instrument for measuring heat and cold. In 1613, a friend and former pupil of Galileo's wrote to him in the following words: "I have brought the instrument which you invented for measuring heat into several convenient and perfect forms, so that the difference of temperature between two rooms is shown by a scale of 100 degrees." The arrangement in Galileo's instrument is identical with that adopted by Drebbel, the Dutch physician, and by Santorio, the Italian physician, who are severally accredited with the invention. Fig. 47, p. 106 represents such a thermometer, which consists of a bulb *A* prolonged downwards into a narrow tube, the lower end of which is open, but dips into some coloured liquid contained in the vessel *B*. The bulb *A* is warmed, when the atmospheric air contained in it becomes expanded, and a portion escapes in bubbles through the liquid from the open end of the tube. When the apparatus is allowed to cool again, the remaining air contracts its volume, or rather, diminishes its elastic force, and, as a consequence, the pressure of the external atmosphere forces some of the liquid up in the tube until it reaches a point *m*, where the balance is restored. If now the temperature of the air in A rises from any cause, the enclosed air increases its volume by expelling some of the liquid, the level of which in the tube is therefore lowered. In colder weather, on the other hand, the liquid will stand at a higher level by the contraction of the air in A. It will be observed by the reflective reader that this instrument is, to a certain extent, a barometer also, inasmuch as a change in the external atmospheric pressure will affect the level of the liquid *m* independently of any change of temperature. For this reason its indications cannot be relied upon. Galileo's pupil and subsequent patron, Ferdinand II., effected some improvements in the thermometer; and that prince's younger brother, Leopold de Medici, invented the modern plan of filling the bulb and tube with spirits of wine, which is then boiled in the bulb, so that all the air is expelled; and, while the instrument contains nothing but the liquid and its vapour, the end of the tube is sealed up. Another improvement, advocated by Lana in 1670, was the employment of quicksilver instead of spirits of wine, and this brought the thermometer into the form which is commonly used at the present day.

The seventeenth century is memorable in the annals of science for the establishment of associations having the investigation of nature

1638
35

for their object. The advantages which such unions offer to the cultivators of science are almost too obvious to need mention. The interchange of knowledge,—the mutual aid,—the stimulus supplied by the sympathy arising from objects pursued in common,—the economy of resources, material and intellectual,—the regular registration and publication of results,—at once suggest themselves as some of the desirable conditions which were secured by the association of men of science into recognized bodies. Such associations appear to have been first formed in Italy, where even in the fourteenth century there existed a philosophical society bearing the name of "THE HUMORISTS." Cesi, Marchese di Monticelli, formed the plan of a scientific society in Rome, in the year 1603, which at first consisted but of four members, and a few years later this was expanded into the LYNCEAN SOCIETY, which Galileo was invited to join when he first visited Rome. The regulations of this association declare that it desires for its academicians "philosophers eager for real knowledge, who will give themselves to the study of nature, and especially to mathematics; at the same time it will not neglect the ornaments of elegant literature and philology, which, like a graceful garment, adorn the whole body of science."

In 1657 another scientific association which attained celebrity was founded in Florence, and held its meetings in the palace of Leopold di Medici. Its members were chiefly, if not entirely, pupils or friends of Galileo, and its title was the ACADEMIA DEL CIMENTO, or EXPERIMENTAL ACADEMY. This society kept up a correspondence with the chief men of science throughout Europe, and undertook to repeat and extend the experiments of the illustrious philosopher of Pisa.

The year 1645 witnessed the virtual foundation of a scientific association that, under the name by which it was some years afterwards formally constituted, has attained a fame unsurpassed by any similar association. We allude to the ROYAL SOCIETY OF LONDON, the origin of which is traceable to the weekly meetings which in 1645 a few men of science began to hold once a week at Gresham College, for the discussion of subjects connected with mathematics and natural philosophy. Similar meetings were instituted soon afterwards at Oxford, where several members of the original society happened to receive appointments in the university. On the 28th of November, 1660, a score of gentlemen, assembled at one of the meetings in Gresham College, agreed to constitute themselves into a society for the prosecution of all kinds of experiments. A set of regulations was drawn up, by which the number of members was limited to fifty-five, who each contributed one shilling per week to defray the expenses of the experimental investigations. The number of members was afterwards extended, and at length every person who might be considered a proper candidate was eligible for admission. One of the members was appointed, at a fixed salary, to arrange for the performance of experiments in the presence of the society. On the 15th of July, 1662, Charles II. constituted the society into a body corporate by a Royal Charter, in which the association is entitled "The Royal Society of London for the Improving of Natural Knowledge." The same year witnessed the establishment of the IMPERIAL ACADEMY in Germany; and in 1666 the celebrated "ACADEMY OF SCIENCES" was constituted by legislative enactment in France.

The establishment of these learned associations coincides with a period marked by the complete overthrow of the scholastic philosophy. The discoveries of Galileo and those of his disciples and contemporaries had put an end to the intellectual darkness which had characterized the Middle Ages, that "opake of nature and of soul." The spirit of inquiry, which at the first revival of the study of nature had animated but a few minds, had now gathered strength, and had extended to all the principal countries of Europe. The new method of Bacon, and, perhaps still more, the striking results which the early experimentalists of the seventeenth century had obtained, showed that a vast field was waiting to be explored. Men's minds were filled with enthusiasm for scientific research; and no wonder, since almost every new experiment brought unexpected truths to light, and suggested new regions of discovery. The declared object of the Royal Society of London was especially the encouragement of those studies which by direct and "sure experiments strive to work out the new philosophy or to perfect the old." As may be well supposed, every branch of science participated in these awakening influences, and many new lines of investigation were thrown out, all marked by a rapid course of discovery.

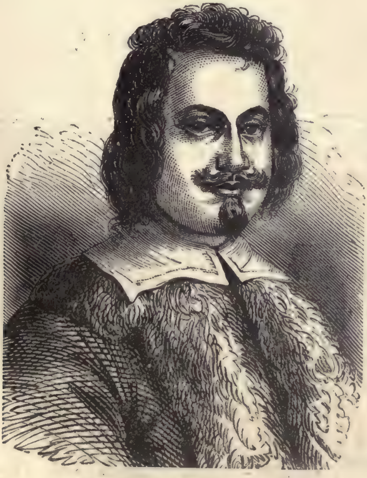

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FIG. 78.—TORRICELLI *(another Portrait).*

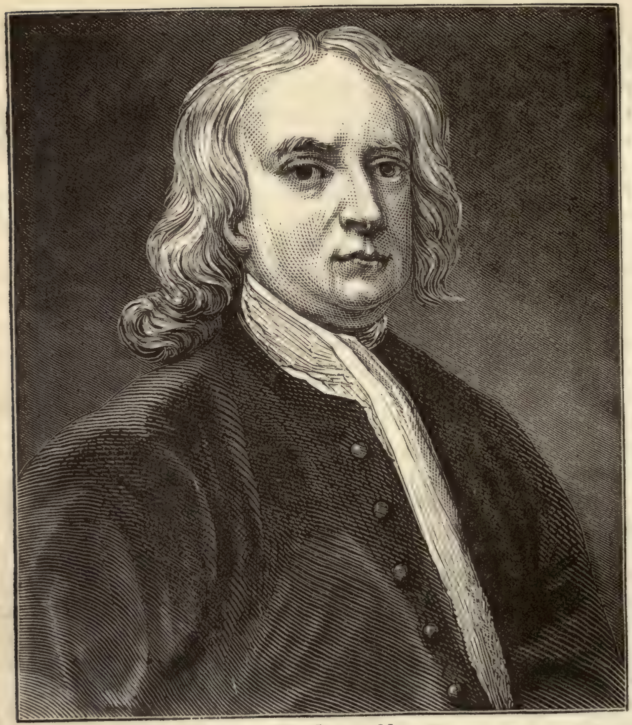

FIG. 79.—SIR ISAAC NEWTON.

# CHAPTER VIII.

## NEWTON.

ISAAC NEWTON was born at Woolsthorpe in Lincolnshire, on the 25th December, 1642 (old style), the year in which Galileo died. Newton's boyhood was remarkable for the decided taste he showed for mechanical and scientific pursuits. Instead of employing his play-hours in sports and games, he was making models of machinery, water-clocks, and sun-dials. Until he had reached his eighteenth year he attended a school at Grantham, where he showed great fondness for books, and is described as "a sober, silent, thinking lad." But he does not seem to have given any extraordinary attention to mathematical study until his admission into Trinity College, Cambridge, where he took up his residence in 1660. At Cambridge Newton had the advantage of the instruction of Dr. Barrow as the professor of mathematics, and here he began his studies with Euclid's "Elements," Descartes' "Geometry," Kepler's "Optics," and Wallis's "*Arithmetica*

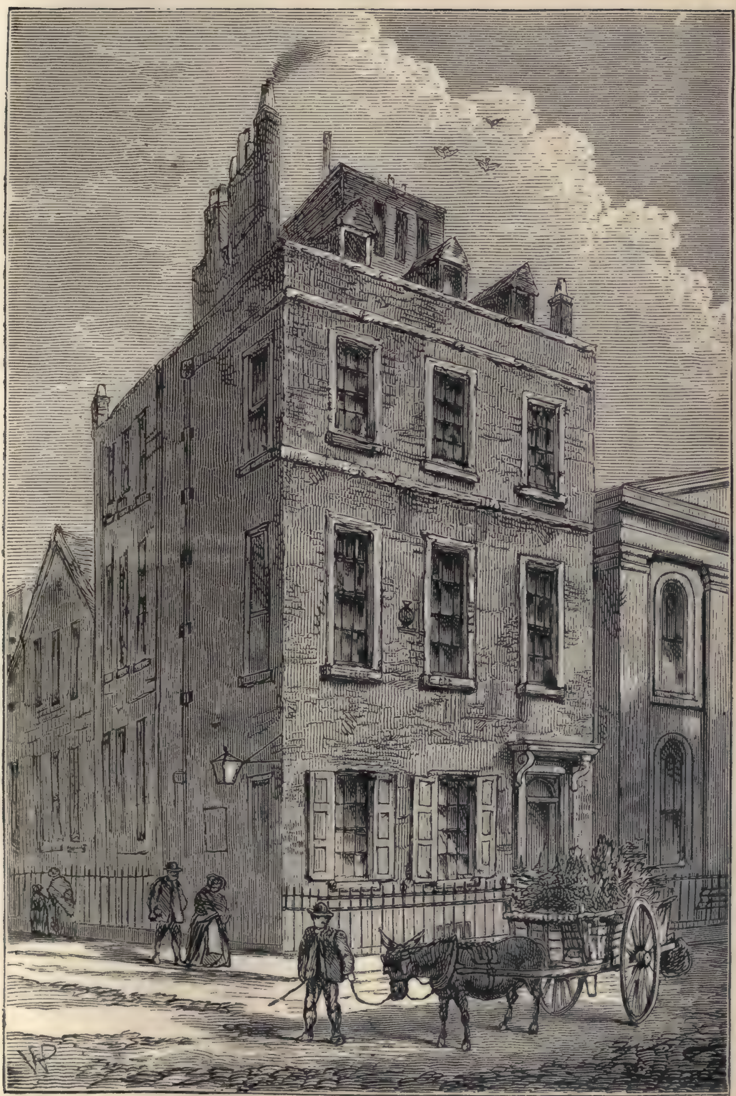

PLATE VI.—SIR ISAAC NEWTON'S HOUSE, ORANGE AND ST. MARTIN'S STREETS.

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*Infinitorum.*" His study of these works was very different from the routine of the ordinary learner: he is said to have taken in Euclid as if by intuition, and he was so far from being satisfied with simply understanding the modern authors, that he carried his own views of the subjects beyond their range. Before he had completed his twenty-third year Newton had made some of his most famous discoveries in mathematics and in physical science.

In 1665 he quitted Cambridge to avoid the plague, and returned to Woolsthorpe, where he had leisure, without interruption, to give himself to philosophical meditations. It was of this period of his life that the oft-repeated story is told of the fall of the apple which suggested the train of thought that led to the theory of gravitation. The danger of plague having ceased, Newton returned to Cambridge in 1666; but he did not make known any of his optical and mathematical discoveries until 1668. In the year following Dr. Barrow retired from the mathematical lectureship to make room for Newton, then hardly twenty-seven years of age. In 1672 Newton was elected a Fellow of the Royal Society, having qualified himself for election by sending to the Society a paper describing that arrangement of the reflecting telescope which has since been called by his name. The "Transactions of the Royal Society" were soon afterwards enriched by the first of Newton's famous papers on Light, and this was followed from time to time by communications announcing his discoveries. The year 1687 is memorable in science as that in which Newton's great work, the "*Principia*," was published at London. This work is in Latin, and its full title is "*Philosophiæ Naturalis Principia Mathematica.*" In 1687 Newton was elected by his university as their representative in the Convention Parliament which called William of Orange to the throne of England. He continued a member of this Parliament until its dissolution, but he took no prominent part in its deliberations. In 1696 Newton was appointed to the honourable and lucrative position of Warden of the Mint, and in this office rendered the country signal service, for he was peculiarly qualified for the post by his mathematical and chemical knowledge. It appears that from the time when as a boy he lived at an apothecary's house at Grantham, he had always taken great interest in chemistry, and in various parts of his works many important chemical observations are to be met with. In 1703 Newton was elected President of the Royal Society of London, and continued to the end of his life, a period of twenty-five years, to fill this office. He was made a knight by Queen Anne in 1705, soon after the publication of his "Treatise of Optics."

Newton continued to occupy himself with scientific subjects until within about ten years of his death. To this period belongs that beautiful saying which he is reputed to have uttered in reply to his friends who spoke of the admiration his great discoveries had everywhere excited:—"I know not what the world will think of my labours, but to myself it seems that I have been but as a child playing on the seashore; now finding some pebble rather more polished, and now some shell rather more agreeably variegated than another; while the immense ocean of truth extended before me unexplored." Newton enjoyed excellent health until he was nearly eighty years of age. He never used spectacles, and two days before his decease he read the morning papers. He died on the 20th of March, 1727 (old style), at the age of eighty-five years and three months. His country was desirous to show her sense of the greatness of the illustrious philosopher who had departed, and his body lay in state in the Jerusalem Chamber, and was thence conveyed for sepulture to Westminster Abbey. The funeral ceremony was attended by a great concourse; six peers of the realm bore the pall, and every honour was paid to the remains.

Newton was never married. From the manner in which his life was spent, immersed as he was in profound studies when not engaged in the most responsible official duties, he felt no want of domestic society. His niece, who with her husband lived in his house, supplied the place of children, and with filial care tended his declining years. From the large emoluments of his office, his prudence, and his simple habits of life, Newton became very rich, and he employed his riches in doing good. His generosity took the form, not of legacies, but of presents, gifts to relations or to friends in want. His physiognomy is described as calm rather than expressive, and his manner as languid rather than animated.

There is no name on the roll of scientific fame more illustrious than that of Newton. The number and importance of his discoveries, and the early age at which they were made, render his career almost without a parallel. Yet he declared that whatever he had accomplished was owing, not to extraordinary sagacity, but solely to industry and patient thought. At Cambridge he spent the greater part of his time in study, and even his relaxations consisted in merely turning his mind to subjects of thought, as history, chronology, chemistry, and divinity. After taking up his residence in London, all the time he had to spare from the duties of his office and from social courtesies was employed in studies, which he pursued with the greatest perseverance and patience. His modesty in reference to his great discoveries is illustrated by the saying of his which has been already quoted. This modesty did not arise from either a want of appreciation of the importance of his labours or a disregard of such fame as eminence in science confers, but from his knowledge of the vastness of the field which he left unexplored. Newton's life has been described as one continued course of labour, patience, charity, generosity, temperance, piety, goodness, and all other virtues, without a mixture of any vice whatever. He refused sinecure appointments and pensions, was scrupulously careful in expending the public money, and he was respected and honoured in all reigns and under all administrations. A handsome monument was soon afterwards erected to the memory of Newton in Westminster Abbey, the *Campo Santo* of England. The monument is inscribed with a Latin epitaph, of which the translation runs thus: "Here lies Sir Isaac Newton, knight, who by the almost supernatural powers of his mind first demonstrated the motions and figures of the planets, the paths of the comets, and the tides of the ocean. He diligently investigated the different refrangibilities of the rays of light, and the properties of the colours to which they give rise. An assiduous, sagacious, and faithful interpreter of nature, antiquity, and Holy Scripture, he asserted in his philosophy the majesty of God, and exhibited in his conduct the simplicity of the Gospel. Let mortals rejoice that there has existed such and so great an ornament of human nature. Born 25th December, 1642; died 20th March, 1727."

We have already referred to the attraction which the problem of the area of the circle presented to the minds of mathematicians, and the attention which has been bestowed upon it from ancient times. The first step towards the solution of this and similar problems was the *method of exhaustions*, as it was termed. The circle was supposed to be inscribed and circumscribed by polygons, and as the number of sides of the polygons was increased, their areas would obviously approach nearer to that of the circle, which would always be greater than the area of the inscribed and less than that of the circumscribed polygon. As the number of sides on the inner and outer polygons was increased, their areas would more and more approximate, and thus the area to the circle could be determined with any required degree of exactness. Kepler, in consequence of a dispute concerning the capacity of a cask of wine, entered upon a general investigation, in the course of which he found it convenient to conceive a circle to be formed of an infinite number of triangles, having their vertices at the centre, and their bases infinitely small, ranged together in the circumference of the circle. The ideas of *infinitely great* and *infinitely small* quantities were in this way made familiar to mathematicians, and formed the origin of the vast improvements which were effected in the science during the seventeenth century. An ingenious Italian geometer named CAVALIERI, who was born at Milan in 1598, and was appointed professor of mathematics in 1629, published in 1635 a work he called "A Treatise on Indivisibles," in which he explained a method of dealing more easily with the problems to which the *method of exhaustions* had generally been applied. He considers lines as made of an infinite number of points, surfaces of an infinite number of lines, and solids of an infinite number of surfaces. Though this is a mode of speaking at variance with the first definitions of geometry, it was adopted for convenience and to avoid prolixity, instead of the more correct expression of what is really the same idea, namely, that a line may be conceived as made up of an infinite number of infinitely short lines, a surface of an infinite number of infinitely narrow parallelograms, and a solid of an infinite number of infinitely thin solids. For example, we may suppose a number of parallelograms described in the figure A B C, Fig. 80. These fall short of the figure by the triangular spaces at their ends; but if their number were doubled, we should obviously have only about half the space left between their boundaries and that of the figure, and as this space would be again halved by doubling the number, we may suppose that the number of parallelograms has been so multiplied that their boundaries coincide as nearly as may be desired with the curve. As the parallelograms increase in number, they become thinner and thinner, and they approach more and more nearly to *lines*. Cavalieri deduced some important truths by his method, and succeeded in solving by it many problems which had defied the efforts of preceding mathematicians. While Cavalieri was applying his new conceptions to these problems, the French geometers were studying many of the higher curves, seeking methods for describing their tangents, etc. Great rivalry existed among the mathematicians of the seventeenth century for the honour of making first discoveries, and many controversies arose on claims of priority. It was customary for mathematicians to challenge the skill of their competitors for scientific fame by publicly proposing some difficult problem for solution. The properties of the cycloid were discovered in this learned rivalry of Roberval, Torricelli, Pascal, and others.

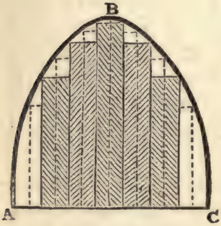

FIG. 80.

A method for the tangents of curves was devised by Roberval about 1636, founded upon a notion much akin to Newton's principle of Fluxions. He conceived a curve to be formed by a composition of motions. To fix our ideas, suppose that in Fig. 81 O Y and O X being fixed lines, a line sets out from O Y, remaining always parallel to O Y, and moving at a rate which for the present we shall suppose uniform. Let a point simultaneously move along the moving line away from O X, at a rate such that the *squares* of its distances from O X increase uniformly in equal times, then the curve O C D, described by the point, will be a parabola. The *tangent to a curve* is the direction in which the moving point generating the curve is going at the point of contact. Now, Roberval conceived that if a parallelogram be constructed the side of which, *a c*, is proportional to the velocity of the generating point moving away from O X, and the side *a d* to the velocity of the line A C

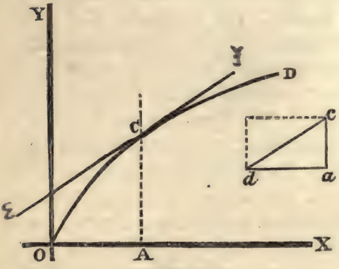

FIG. 81.

moving away from O Y, the diagonal $a'c$ would represent the direction of the tangent E F. Roberval, however, had no general method for determining the ratios of these velocities. and, in fact, applied his method only to a few particular curves, in which the ratio of the velocities was readily discoverable. Indeed, he did not present the subject as we have here done, in reference to the relation between *ordinates* and *abscissæ* (*see* page 151), but deduced the ratios for the ellipse, etc., from the properties of lines drawn from the foci. Nor did any consideration of infinitely small quanties enter into his method. FERMAT, another French mathematician, employed the conception of infinitely small quantities to determine the tangent to curves, in a manner which so nearly approached the method of the new calculus, that some of Fermat's countrymen have even claimed for him the honour of discovering the differential calculus. Dr. Barrow, Newton's predecessor at Cambridge, simplified Fermat's method of tangents by his conception of what has been called the "Incremental Triangle." This notion, like that of *indivisibles*, is expressed in language which contradicts the obvious truths of geometry, for it assumes that of any curve a portion so small may be taken that it may be rendered a straight line. It will perhaps assist a reader unaccustomed to this language to a conception of its meaning, if, having actually drawn a circle with a pair of compasses, he will draw on it a number of chords progressively smaller and smaller. He may then observe that as the chords become shorter the distances of their middle points from the arcs they cut off not only become smaller in themselves, but bear a continually lessening in proportion to the length of the chord itself. The process of drawing shorter and shorter chords may theoretically be carried so far that the chord differs from its arc by an amount less than any assigned one. When the difficulty about the phraseology used in discussing these infinitely small quantities has been surmounted, "*the incremental triangle*" may easily be understood. Let B F G, Fig. 82, be any curve, O C and C B the co-ordinates of the point B. Pass now from B to F, another point in the curve, so near to B that the portion of the curve B F may be practically a straight one. The abscissa of F is O E, its ordinate E F, greater than the abscissa and ordinate of B by the distances BD and DF (BD is parallel to OX), which

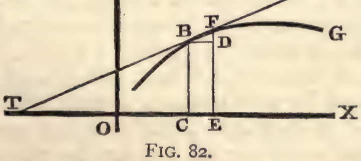

FIG. 82.

are, of course, extremely small. But, however small these distances may be, the proportion that one of them bears to the other is definite, and may be very great. In illustration observe that a number so small as the thousandth part of 1 may bear as large a proportion as you please to some other small number; for instance, the ratio of the small number just named to the millionth part of 1 is 1,000. F D, then, must have to B F a certain ratio; and, as the triangle B D F is right-angled, we give that proportion a name (*see* page 61), and say that $\frac{FD}{BD}$ is the (*trigonometrical*) tangent of the angle F D B. Observe here that the word tangent, as last used, has a sense quite other from that of the *geometrical* tangent T B N, but it defines the position of this last by giving its inclination to O X, since angle N T X obviously equals angle F B D. The triangle B F D is the *incremental triangle*.

JOHN WALLIS, who was Savillian professor of geometry at Oxford during the last half of the seventeenth century, made further advances towards the solution of problems which were now occupying the attention of geometers. He pursued the investigation into methods of greater generality for finding areas, than the comparatively few cases in which geometers had been as yet able to assign the areas bounded by curves. Descartes had given the highest degree of generality to the manner of constructing curves, and it was now sought to assign by equally general methods, the areas of the spaces bounded by these curves. This, of course, would depend upon the relation between the abscissa and the ordinate which the Cartesian equation expresses, and Wallis found that he could exhibit the quadration or area of all curves, when the one co-ordinate could be expressed in terms of the other, involving no fractional or negative indices. Wallis was the author of some ingenious researches on Series. Newton was led to one of his great mathematical discoveries by the consideration of certain *series* which are discussed in Wallis's "Arithmetic of Infinites."

Newton presented the principles of the new calculus in a clear manner, by basing his exposition on certain obvious notions regarding motion. When a body moves uniformly it has at each instant the same velocity; but when the case is otherwise, in a falling body for example, the velocity at each instant is different, but at each instant the actual velocity is measured by that uniform velocity which the body would have if, at the instant considered, the body had ceased to be affected by any force. Now, every curved line may be conceived as generated by two motions, one consisting of a movement of the ordinate receding from O Y (Fig. 83), and always parallel to it; the other, that of a point travelling along the moving ordinate and changing its distance from O X. For simplicity, suppose the motion of the ordinate to be uniform, then, if the motion of the point *in* the ordinate is also uniform, the point must describe a straight line; but if the motion be *accelerated*, a curve convex towards O X will be marked out, while if, on the other hand, the motion be *retarded*, the curve will be concave. Now, P M in the figure represents a certain position of the moving ordinate P M and moving point P. When the ordinate has arrived at the position represented by N *a*, the point P has the position marked P′, it has receded from O X at a lessening velocity. If, while the ordinate was moving from M to N, the moving point had continued to recede from O X with the *same velocity* in that direction which it had at P, then it would not be in the *curve*, but at some other point, as *a*, and in its passage from P to *a* it must necessarily have described the straight line P *a*. Suppose now that from P we draw a line parallel to O X cutting *a* N in *b*, then in Newton's phraseology *b a* is the *fluxion of the ordinate*, that is, the ordinate while passing from M to N *would have* (if uniform motion had continued) flowed on so as to increase its length by the amount *b a*. In a similar way Newton would call P *b the fluxion of the abscissa* (remember the movement along O X has been assumed to be uniform). The geometrical truth enunciated on page 13 tells us that P *a*, which is the *fluxion of the curve*, has its square equal to the sum of the squares of P *b* and *b a*. It will be perceived that the principle of the rule for tangents is related to this calculus; but we are not required to make any supposition about curves being made up of very short straight lines, nor is it necessary to consider infinitely small quantities at all. From the equation to any curve (page 150) it is always possible to deduce the ratios of the two fluxions *a b*, P *b*; but the methods of doing this would be unintelligible to readers previously unacquainted with the subject. It must suffice to show the elementary conception of Newton's *Fluxions;* and these explanations are not out of place even if the non-mathematical reader has gathered from them nothing more than a clear idea of why Newton gave this name to his method. Further, we may state that Newton used certain symbols to indicate fluxionary quantities. For instance, the *b a* and *b* P of Fig. 83 are particular cases of $\dot{y}$ and $\dot{x}$, which respectively represent fluxions of the ordinate and abscissa. The first application of the theory of fluxions is to find the tangents, and Fig. 82 shows that $\frac{\dot{y}}{\dot{x}}$ is the *trigonometrical tangent* of the angle which the *geometrical tangent* to the curve at C forms with O X.

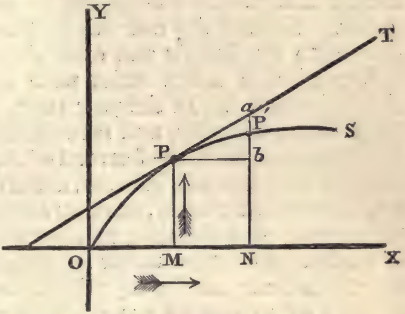

FIG. 83.

It has already been mentioned that one of the first mathematical works which Newton read was Dr. Wallis's "Arithmetic of Infinites." Some theorems given in that work led Newton to the study of certain kinds of algebraical *series*, and the discovery of facile methods of finding the terms of such series. Finally, he was led to the discovery of the celebrated Binomial Theorem, which gave a method of expressing any root of a quantity consisting of two terms, by means of a series composed of those terms combined according to a given law. This rule was immediately applicable to numberless cases of ordinary mathematical analyses, and especially to problems involving the areas of curves. Newton did not publish these discoveries to the world, but showed to Dr. Barrow and other friends, some years before 1669, a MS. treatise, in which series were employed and general methods clearly indicated. In the year 1673, Leibnitz, the distinguished mathematician, historian, and metaphysician, visited England, where he made the acquaintance of several of the most eminent mathematicians. Some years afterwards Newton, at the request of Oldenburg, the secretary of the Royal Society, wrote an account of his method of determining the areas of curved figures, together with a description of his invention of fluxions, concealed under an anagram. This was, by Oldenburg, sent to Leibnitz, and in 1677 the latter returned to Oldenburg a short account of an equally general method of calculation which he had himself invented, and which he called the *Differential Calculus*. He gave in his communication the notation and the principal rules for this calculus, but the first published account of it did not appear until 1684. Though Newton had invented *fluxions* previous to his temporarily leaving Cambridge in 1666, it was only in the first edition of the "*Principia*," which appeared in 1687, that he for the first time made public the fundamental principle of the fluxionary calculus, without giving its notation. Leibnitz, in his first account of his *differential calculus*, described the notation he had invented, and pointed out its application to drawing tangents to curves, and to determining maxima and minima. He makes a reference to a *similar* calculus of Newton, and does not claim for himself the merit of having been the first inventor of the general method. Newton, in a note in his "*Principia*," says: "In a correspondence which took place, about ten years ago, between that very skilful geometer G. G. Leibnitz and myself, I announced to him that I possessed a method of determining maxima and minima, of drawing tangents, and of performing similar operations, which was applicable to rational and irrational quantities, and I concealed the same in transposed letters involving this sentence (*data equatione quinteunque fluentes quantitates involvente fluxiones invenire et vice versa*). That illustrious man replied that he also had fallen on a method of the same kind, and he communicated to me his method, which scarcely differed from mine except in the notation and in the idea of the generation of the quantities." These statements seem to show that each of these eminent men gave the other, at that time, credit for having independently arrived at the same discovery as himself. A few years later, however, there arose between the two distinguished philosophers and their respective partizans a bitter controversy as to priority of invention, and charges of plagiarism were made and resented on either side. Those who have impartially reviewed all the circumstances connected with this dispute, have come to the conclusion that Newton and Leibnitz each independently discovered the method which the one called *fluxions* and the other the *differential calculus*. It so happened, however, that the nomenclature adopted by the German philosopher possessed special advantages, and in the hands of the Continental mathematician the calculus made rapid progress. The methods which we have seen were devised by Cavalieri and others for passing to the more recondite problems, involved the principle of *limits*, while they avoided the excessive tediousness of the methods employed by the ancient geometers. We have also explained that some of these methods were almost identical with cases of the fluxional or differential calculus. But this last was essentially distinguished by presenting a general method of investigating *all* the cases, and of offering the means of solving other problems yet unattempted. The common principle of Newton's and Leibnitz's methods is that of considering an algebraical quantity as involving in a definite manner some other quantity which receives varying values, and is hence termed the *variable*, of which the complex quantity is called the *function*. Now, when the variable changes uniformly, the function is usually such that its dependent changes proceed at a varying rate. Both Newton and Leibnitz demonstrated, each in his own manner, that the rate of the variation of the function, compared with the rate of the variable, could be expressed in terms of the variable. Leibnitz proposed to compare the infinitely *small elements* of the variable and its function generated simultaneously, and discovers the finite ratio which, as he conceived, might exist between them. Thus, for example, referring again to Fig. 83, we must suppose that when the point P, which sweeps out the curve, has advanced an extremely small distance, there will be formed the triangle like P $a\,b$, extremely (infinitely) small, so that its hypotenuse may then be conceived as coinciding with the curve; then the two sides of the triangle formed by the *increase* of the ordinate and that of the abscissa will give the same ratio as before. Leibnitz expresses this ratio by the symbol $\frac{dy}{dx}$, which he terms the *differential coefficient*.

Newton and Leibnitz give rules by which the *differential coefficient* of any function may be found, the process being more or less long or difficult according to the nature of the function. It has been indicated by the explanations already given that the *fluxion* or *differential coefficient* is really the *limit* of the ratio towards which two quantities, always connected by some definite relation, approach as the quantities themselves are diminished. Neither of the inventors of the calculus, however, expressly referred its principle to the notion of a limit. Newton expressed the principle by ideas derived from mechanical considerations, as velocity or space traversed in a given time; Leibnitz resorted to the notion of infinitely small elements.

Besides the class of problems which the application of the direct differential calculus can solve, there is another class of problems to be dealt with which require the inverse process. This inverse process, which is now termed the *Integral Calculus*, consisted, according to Leibnitz's view, in finding the sum of elementary parts accordingly to the notion which has been already explained by the method of "*indivisibles*" (page 183). It does not enter into our design to explain the methods of the infinitesimal calculus further than suffices to illustrate its objects. Let O P P′, Fig. 84, be a curved line, O Y and O X being the *axes* of the co-ordinates, and let us suppose that the area of the space M P′ P M′ is required. Let the line M M′ be divided into a number of equal parts, 1, 2, 3, etc., and lines be drawn parallel to O Y, cutting the curve in the points 1′ 2′ 3′, etc. Through the points P, 1′, 2′, etc., draw lines parallel to O X, as shown in the figure. Now, if we add the areas of all the rectangles of which the divisions of M M′ are the bases, and the heights M P, 1 1′, 2 2′, etc., their sum will evidently fall short of the true area of the figure by the sum of the *quasi* triangular spaces. The construction of the figure will show that this deficiency must be less than the rectangle P *q m n*, which is obviously equal to the sum of all the little *rectangles* of which the several triangle spaces are part. Note that if the space M M′ were divided into forty or four hundred, or any number of parts, the height of the rectangle P *m* would be unchanged, but its width *m n*, and therefore its area, would diminish as M 3′ became a smaller part of M M. Consequently, by dividing M M′ into a sufficiently great number of parts, we may find a set of rectangles the sums of whose areas shall be as near as we please to the area of the curved figure M M′ P P′. The equation to the curve is the general expression for each ordinate in terms of its abscissa (page 151), and if the sign $\phi$ be used to indicate the function in question, and $dx$ the small increment of the abscissa in passing from one rectangle to the next, we shall have for the sum of the rectangles the following series, where $a$ is the value of O M:—

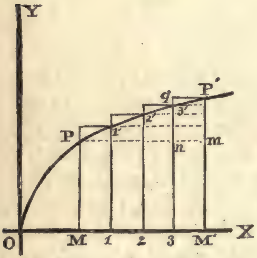

FIG. 84.

$$\phi a\times dx+\phi(a+dx)\times dx+\phi(a+2dx)\times dx+\phi(a+3dx)\times dx+\text{ etc.; or}$$

$$dx\left\{\phi a+\phi(a+dx)+\phi(a+2dx)+\phi(a+3dx)+\text{ etc.}\right\}$$

The number of terms in this series being equal to the number of parts into which the distance M M′ is supposed to be divided, and this being supposed to be increased without limit, it might be imagined by a beginner that the unlimited increase of terms in the series within the brackets would increase the value of the expression; but it must be observed that the increase of $n$, the number of terms, implies a decrease of the value of $dx$, for M M′ divided by $n$ is $dx$. It should be noticed that in the symbol $dx$ the letters do not stand in the sense of the ordinary algebraical notation, but constitute a single symbol for the small increments of $x$. The value of the expression usually approaches a fixed limit, and in the case we are supposing the limit is sufficiently obvious. That limit is the area of the figure P M′, P′ M, and is called the *integral* of $\phi x\, dx$, from $x = \mathrm{O\,M}$ to $x = \mathrm{O\,M'}$. It is proved in works on the integral calculus that the integral of a given function of a variable is that other function whose differential coefficient is the given function.

In the science of statics and dynamics Newton effected certain great improvements, which may here be named apart from his great discovery of universal gravitation. Some advances in statics effected by Stevinus and Galileo have already been noticed. It was these philosophers who generalized the principle of the lever by showing that in the case of every mechanical power, the force multiplied by its velocity is equal to the weight multiplied by *its* velocity. Wallis in 1669 published his "*Mechanica*," in which a regular system of statics is established on this principle. VARIGNON (1654—1722), a French mathematician, treated all the problems of mechanics with a higher degree of generality than had before been attained. He deduced the solution of every problem in statics from the simple principle that *when three forces are in equilibrium each is proportional to the sine* (page 61) *of the angle between the directions of the other two.* This principle, which is really involved in the propositions of Stevinus, was proposed and applied by Varignon in a work which he published 1687. Newton in his "*Principia*," which appeared in 1687, enunciated principles of statics which in effect were similar to those of Varignon; but these are noticed merely as an introduction to the more difficult inquiries relating to moving bodies. Galileo, as we have already seen, was the first who made any real advance in ascertaining the laws which govern the motions of falling bodies and of projectiles. Newton reduced to their simplest and most general expressions the facts of the motion of bodies, which had indeed been known by former writers, but not explicitly laid down as the fundamental principles of the science of dynamics. These propositions of Newton's are the well-known "Laws of Motion." He called them "*Axiomata, sive Leges Motûs*." An axiom is usually defined as a self-evident truth; but it should be observed that the "laws of motion" are axioms only to those whose experience of the phenomena is wide enough to enable them to recognize the generality of the truths expressed. We shall here give the laws of motion as Newton laid them down:—

*Law* I. Every body continues in its state of rest or of uniform motion in a straight line, except in so far as it may be compelled by impressed forces to change that state.

*Law* II. Change of motion is proportional to the impressed force, and takes place in the direction of the straight line in which the force acts.

*Law* III. To every action there is always an equal and contrary reaction, or the mutual actions of any two bodies are always equal and oppositely directed.

The beginner in science who reads these laws for the first time may fail to obtain a just idea of their meaning. The first law appears to oppose common experience as regards bodies in motion, and no direct experimental demonstration of its truth can be appealed to, since we are unable to witness that continued uniform motion in a straight line of which the law makes mention. We learn by experience, however, that all the motions which we can produce are interfered with by certain "impressed forces," which modify and destroy them. We may slide a stone along the ground for a short distance, but it soon comes to rest; but along a sheet of ice it will go much farther, and we soon recognize that the smoother the surface the more nearly is the law realized. We conclude, therefore, that if the retarding force of friction could be abolished altogether, as well as another retarding force we know to be at work, namely, the resistance of the air, the body would continue to move with a uniform velocity.

It has been held by some that the first of Newton's laws of motion amounts to nothing more than a definition of *body* and of *inertia*. In like manner the second law has been considered as nothing more than a definition of *force*, since of force there is no other criterion or measure than the change produced in a body with regard to its motion. There can, however, be no question of the merit and originality of the conceptions of Galileo and of Newton in dynamical science, and of the great utility of some such statements of its fundamental principles as Newton has given in his three laws. The vast genius of Aristotle, with all his power of dealing with verbal subtleties, failed completely, as did also the more practical mechanical science of Archimedes, to discover a single true principle of dynamics, whereas the two centuries which have passed since the publication of the ' *Principia* " have not been able to improve upon Newton's expression of these laws, as axioms or simplest truths from which the whole science of dynamics may conveniently be deduced.

The discovery of the first two laws is, however, due to Galileo, and the third was known in some of its cases to Wallis, Wren, and others, before it was announced in all its generality by Newton. This third law is perfectly clear in its statement, but presents a difficulty to persons unacquainted with the facts. The ordinary observer always thinks of the magnet as attracting the needle: the effect on the latter is plainly seen; but the reaction, namely, the fact that the needle, with precisely the same force, attracts the magnet, is only recognized after experience and reflection. We have only to give the magnet and the needle equal freedom of motion to perceive that each moves towards the other in a manner which proves that the same force is the cause of the motion. The velocity of motion and the space through which each moves will be unequal if the masses are unequal; but the *momenta—i.e.*, the products of the mass into the space—will be equal.

Newton gives a demonstration of the *composition of forces*, as a corollary to these laws of motion. He supposes a body acted upon by an impulsive force, by virtue of which it would continue to move uniformly in a straight line in a certain direction; while at the same time another force acts upon it, which would of itself draw the body in a direction inclined to the former at a given angle. It is proved that the body will follow neither of these directions, but will take an intermediate course, which is determined by drawing the diagonal of a parallelogram, two sides of which represent, in magnitude and direction, the two forces. Newton's proof is deduced from the laws of motion, which declare that the body obeys each force unaltered in its own direction, and that at any given time the position of the body, measured on a line parallel to the direction of each force, is precisely the same as if that force alone had acted. From this simple principle Newton proceeds to deduce his whole theory of central forces. The theorem by which he begins is a very interesting one, and the reasoning can easily be followed by any one conversant with the geometrical fact that triangles on the same base and between the same parallels are equal in area. Let a body be moving unacted on by any force along the line A D, Fig. 85; in that line its positions at any equal intervals of time would be certain equidistant points, say A, B, C, D. Suppose that when the body had arrived at B, a force *for an instant* acted upon it which would have of itself caused the body to move along the line B S, its position at the end of the interval of time in which it would otherwise have arrived at C will be found by drawing from C the line C E parallel to B S, and equal to the space that the impulsive force alone would have carried the body along B S in the same time. The body would therefore pass along the diagonal B E of parallelogram of which E C, C B are the sides. Draw the lines S A, S C. Now the areas of the triangles B S E, B C S, which are in the same base, S B, and between the same parallels, are equal to each other.

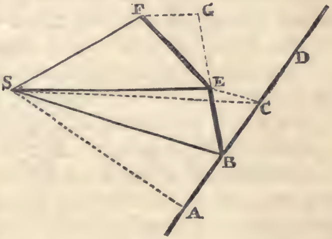

FIG. 85.

Also triangle A B S is equal to triangle B C S, and therefore triangle S B E has the same area as triangle A B S. Now, again, suppose that when the body had arrived at E, another impulse were to act upon it from S, directed along the line E S, it would follow the diagonal of another parallelogram, and at the end of the interval of time would be at F, instead of at G, where, without the impulse towards S, it would have arrived, G E being equal to B E. The same reasoning as before shows that triangle E F S equals triangle E B S. Thus, it is proved that a line joining the point S to the moving body, and moving with it, would sweep out equal areas in equal times. Now, we are to suppose that the intervals of time at which the impulses act are made shorter, and again shorter, and so on continually. The number of triangles would be increased; but, however greater their number, the same reasoning would hold: *equal areas would be swept out in equal times.* In the limit, the boundary A B E F would become a curve, and the series of impulses would coalesce into a continuous force acting from the centre S, while the area swept out by the line joining S and the body would always be proportional to the times. There is only one point of this reasoning which involves any difficulty, and that consists in the assumption that the relations which are established for the elementary quantities remain unaltered, as we pass from them to their limit,—as we pass, for instance, here from the polygonal to the curvilinear path. In the case before us the ideas are sufficiently simple; but Newton had provided for any difficulty on this score by previously discussing various instances of limits under the title of "Prime and Ultimate Ratios."

This theorem, considered by itself, would have been sufficiently curious, but Newton investigated it with a view to the higher purpose of establishing a whole system of doctrines concerning central forces. He demonstrated the inverse proposition, namely, that if a body move in one plane, so that the areas described by a line drawn to a fixed point are proportional to the time of describing them, the body is acted on by a force tending to that point. Newton, in fact, investigated all the cases of curvilinear motion, and examined the mathematical conditions of all problems arising from it. It was in this that the vast resources of Newton's geometry were particularly displayed. The very idea of motion about a centre of force was first accurately conceived by him, and he gave in the equality of the areas the only criterion of the existence of such forces. As a body projected in the direction of a straight line, and at the same time exposed to the action of a central force, will neither proceed in that straight line nor pass directly towards the centre of force, the nature of the particular intermediate curve it will describe will depend upon the ratio between the intensities of the original impulsive force and the deflecting or central force. In the investigation of the various possible cases, Newton displayed the fertility of his genius and his knowledge of the stores of mathematical truths which the labours of former times had accumulated. The properties of the *conic sections* had largely occupied the attention of the ancient geometers, and now, after these properties had remained for ages little more than mathematical curiosities, they were unexpectedly applied in the investigation of the widest and grandest laws of all the science of nature. By combining the truths long before reasoned out by the old Grecian sages with his own original methods, Newton established the following proposition—a most important one in the theory of the universe: *a body projected in a straight line, and subjected to the action of a central force, will revolve in some one of the conic sections, if the intensity of the force be inversely as the square of the distance from its centre, which will be at a focus of the curve.* Whether the particular conic section be a circle, an ellipse, or a parabola, depends on the ratio of the tangential (or projecting) to the centripetal (or deflecting) force. The illustrious author of the "*Principia*" traces out a vast number of consequences. He deduces the laws by which the velocities of bodies moving in elliptical orbits must necessarily be regulated. He shows in what periods such orbits must be completed, and demonstrates (as a necessary consequence) the remarkable relation that the squares of the periodic times are to each other as the cubes of the major axes. As half the major axis of its elliptical orbit is the mean distance of each planet from the sun, the reader will recognize in this last mathematical deduction the third of Kepler's laws (page 91), as in the equality of the areas he has already doubtless perceived the second law. Here were the observed and unexplained facts of the German astronomer deduced *à priori* by the English philosopher from simple dynamical principles.

These considerations having been founded only upon the mechanics of small particles of matter, the author of the "*Principia*" goes on to examine the effects of attractions of particles on each other; and he shows that, supposing such particles to attract each other with a force inversely proportional to the square of the distances between them, and to be aggregated in spherical masses, these spheres would be subject to the same laws of attraction, etc., as the particles, and that the resultant attraction would be directed to the centre of the spheres, and would be proportional to their masses, divided by the squares of the distances between their centres.

The special glory of Newton is the system of the world which he founded on the idea of universal gravitation. We may here leave for the present out of consideration those approaches to the notion of gravitation which had been made by several philosophers before Newton. Germs of the idea of universal gravitation may, as in the case of many other modern conceptions, be discovered among the ancients. In the poem of Lucretius, already referred to (page 44), it is assigned as a reason for supposing the world to be infinite, that otherwise all the bodies composing it would have approached each other, and all would have at length united together in one mass. Kepler, and later Hooke and Halley, entertained the idea of planetary gravitation, the last two nearly at the period at which it occurred to Newton. It was in the year 1666, after the plague had driven him from Cambridge to his house at Woolsthorpe, that Newton began to reflect upon gravitation as the cause of the planetary motions. There is a well-known story that Newton, sitting one day in his orchard, and observing an apple dropping from a tree, was led by that incident into a train of profound thought, which resulted in the discovery of the system of the world. He was led to reflect that the power which draws heavy bodies to the earth, extends some distance above its surface, even to the tops of the highest mountains; nay, above them, to the highest regions of the atmosphere. From this he conjectured that it might extend to the moon; that it might, in fact, be this very force which retained the moon in her orbit by balancing the so-called centrifugal force. He reflected, also, that although the force drawing bodies to the earth did not appear sensibly lessened at the heights to which we could reach, yet these heights were too small to enable us to conclude that the action of this force could be everywhere the same. It remained, therefore, to discover, if possible, the law by which it varied, and, in thinking of this, it occurred to Newton that, if it was the weight of the moon which retained her in her orbit round our earth, it must be a like attraction between the planet Jupiter and his satellites which retained them in their orbits. Now, the periods in which each satellite revolves about Jupiter being known, and the distances known, it was easy to calculate the relative centrifugal forces which must be balanced by the attraction. Newton found that the attractive force would vary inversely as the square of the distance. Having thus obtained the law of the diminution of the force, it remained to see whether such force of gravity, so acting between the earth and the moon, would correspond with that required for retaining the moon. Now, the moon's distance being known to be 60 semi-diameters of the earth, the effect of gravity ought at that distance to be $\frac{1}{3600}$th of that at the earth's surface. The power of gravity being represented by the space through which it will cause a body to fall in one second, or the space through which it would cause the moon to fall from rest in one second, would be less than that which it would cause a body at the surface of the earth to fall through, in proportion as the square of the distance from the centre in the latter case is less than in the former.

Now, the velocity of the moon in her orbit is known, and this velocity would of itself, the instant some other force ceased to prevent it, cause our satellite to pass off through space in a straight line. Taking the position of the moon in her orbit at any instant, the distance by which she is during the next second deflected from the straight line or tangent, in order to continue in her orbit, may be considered as the space through which she has fallen towards the earth, as if from rest. Now, the dimensions of the lunar orbit and the moon's velocity in it being known, it was not difficult for Newton to calculate the space

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through which the moon falls to the earth, and the space through which it ought to fall if his conjecture were correct. Newton made the calculation, and was disappointed to find that the result did not verify his speculation, for the force of gravity appeared by one-sixth too great for the required effect on the moon. Now, the measurements of the earth's magnitude which were available at that time were extremely imperfect, and Newton had estimated the diameter of the earth from the then commonly received measurement, which made a degree of latitude equal to 60 British miles, whereas the real length is about $69\frac{1}{10}$ miles. The difference between the moon's velocity and that calculated upon this datum might have appeared small to another person, but to Newton it seemed conclusive against his hypothesis. He imagined, however, that some yet unknown cause, analogous perhaps to the vortexes of Descartes, modified the effects of gravity. He did not therefore wholly give up his notion, but he resolved with characteristic patience to let it remain undisclosed to the world until study and reflection should bring some knowledge of the cause that could so modify a law indicated by such strong analogies.

Newton therefore refrained from publishing anything upon this subject until ten years or more afterwards, when he heard of the more accurate measurement of a degree of the meridian which had been then recently made by Picard in France. He then recommenced his former calculations of 1665 with new data. Observing as he advanced the manifest tendency of these numbers to produce the expected result, his nervous excitement became so great that he was unable to proceed with the calculation, and asked one of his friends to complete it for him. This time there was no doubt about the conformity of the computed with the observed result; and no sooner had Newton recognized the truth of his speculation, than he began to pursue it to its consequences with a boldness and vigour which had before never been shown in science. The one conclusive result he had obtained gave him the clue to the whole mechanism of the heavens. The analogy which was so manifest in the systems of satellites round Jupiter and Saturn was extended to the system of planets revolving round the sun. The greatest and most general of all physical laws was finally deduced by Newton, namely, the Law of Gravitation, which affirms that *every particle of matter in the universe attracts every other particle with a force which is inversely proportional to the squares of the distances between them.* Newton demonstrated that as a consequence of this law spheres attracting each other in their individual particles would have the same effect upon each other *as if* the resultant force resided in the centre alone. As the bodies of the solar system are very nearly spherical, they will therefore act upon one another and upon bodies near their surfaces *as if* their centres were so many centres of attraction. This consequence of the gravitating energy of the particles of which the earth is made up is so often erroneously conceived as a force actually residing at the earth's centre, that it may here be useful to mention that if we study the attraction of gravitation, not as to its action on distant bodies beyond the earth, but as to its intensity at different points within the earth itself, it is found that the intensity of the gravitating force does not constantly increase towards the centre, but after attaining a maximum at about one-sixth of the distance to the centre, the resultant force then decreases as the centre is approached, where it becomes *nil.*

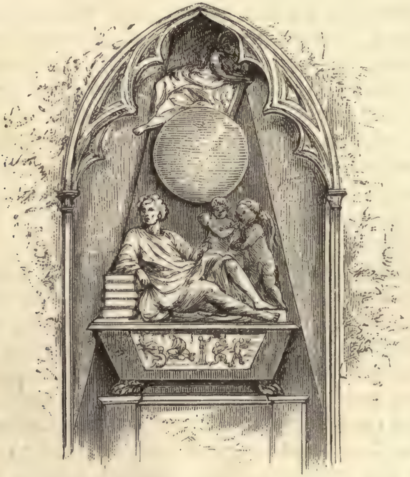

FIG. 86.—NEWTON'S MONUMENT IN WESTMINSTER ABBEY.

On the front of the monument erected in Westminster Abbey to Newton's memory are represented youths bearing emblems of his principal discoveries, and among them is one engaged in weighing the sun and planets with a steel-yard. Perhaps none of the results which Newton deduced are more striking to the popular mind than the estimation of the *weights* of the sun and planets, and the determination of even their specific gravities. Yet the weights are found on the same principle as that by which the earth's attraction on the moon was ascertained, the weight being nothing more than the gravitating power, and proportional to the quantity of matter, while the specific gravity or density of a given body is proportional to the quantity of matter it contains, and inversely proportional to its magnitude.

Among the consequences which Newton traced from the great principle he had discovered were some others which we must not pass over without mention. Newton perceived that the figure of the earth would be the result of the mutual attraction of its particles modified by the effects of its rotation. That the centrifugal force due to rotation would act most upon the part of the earth near the equator, and not at all upon the regions about the poles, is probably sufficiently obvious. Newton calculated the amount of this centrifugal force, and he found that at the equator it is the two hundred and eighty-ninth part of the force of gravitation; that is, if on a spring balance were hung at the equator a weight which marked two hundred and eighty-eight ounces on the scale, the same would, if the earth ceased to rotate, bring down the index of the scale to two hundred and eighty-nine ounces. Newton illustrated the effect of this centrifugal force on the figure of the earth by supposing that a canal extended from the pole to the centre, and from the centre another canal extended to the equator. If these canals were filled with water, the two columns of water must balance each other; that is, the whole weight or total effect of gravity must be the same in both. Taking account of the centrifugal force, Newton calculated what must be the relative lengths of these columns, and he found that the length of the equatorial must be to that of the axial column as two hundred and thirty to two hundred and twenty-nine. This amounts to a determination of the proportion the earth's equatorial radius must bear to its polar radius. At the time these determinations were made, there existed no measurements from which the earth's deviation from the perfectly spherical form could be inferred, although Newton's investigation of this question had probably been suggested by Cassini's discovery of the spheroidal form of the planet Jupiter, to which we shall presently direct the reader's attention. The spheroidal figure would add its effect to that of the centrifugal force in diminishing the force with which bodies at the surface of the earth are attracted as we pass from the polar to the equatorial regions. We have already alluded to a fact which the observations of astronomers had long before disclosed, namely, that the equinoctial points, in which the apparent annual path of the sun in the heavens intersects the equator, were affected by a slow but constant retrograde motion by which they shift their position about 50″ of arc annually, so that they make a complete circuit of the heavens in something less than 26,000 years. No astronomer had as yet been able to assign a cause for this motion. Now, Newton not only showed that the precession of the equinoxes must necessarily result from the spheroidal form of the earth, but he calculated the rate of movement which the effect of gravity on the protuberant matter about the earth's equator must produce, and the result was in perfect agreement with observation.

The concordance of the general results deduced from the principle of gravitation with the observed facts stated in Kepler's law, has been mentioned as justifying the adoption of the principle as one of the fundamental laws of nature. Yet if Kepler's laws were *strictly* true, universal gravitation cannot also be the true law of the universe, for Kepler's laws take account only of the actions between each planet and the sun, whereas the several planets must also by gravitation act upon each other. Hence the orbits cannot be absolutely but only approximatively elliptical, and the other Keplerian laws must be also only very nearly true. Now, observation showed that these divergences really existed, and while the greater phenomena furnished a general proof of Newton's theory, these less phenomena of "perturbations," as they are called, contributed confirmatory and more convincing proofs of its absolute correctness. It is true that the accurate investigation of the mutual actions of bodies, such as those of the solar system, is an intricate and difficult one, and the beautiful apparent simplicity of absolute elliptic orbits must be given up for more perplexed paths, to be traced out in detail only by laborious calculations; yet these perturbations and intricacies are essential to the truth of the principle of universal gravity, so grand in its own simplicity. The most prominent instance of perturbations presents itself in the movements of our satellite, whose elliptical orbit about the earth is disturbed by the action of the sun. The inequalities produced by this course are of sufficient amount to have been noticed even by the ancient astronomers. Newton investigated many of the effects of the mutual actions of the three bodies, the sun, earth, and moon, and although the imperfect data and means within his reach left much for his successors to do in order to reduce the moon's motions to the exact and complete forms which constitute the "lunar theory" of the present time, his results must be regarded as evidence of the unrivalled skill which enabled him to arrive at conclusions very near the truth.

There is a very common misconception arising from the manner in which the mechanism of the solar system is popularly described. There is doubtless a certain convenience in saying that the moon revolves about the earth in an elliptical orbit, and the earth itself revolves about the sun in another ellipse, of which the sun occupies one focus. As a matter of fact, however, this is by no means the case, as this mode of presenting the case leaves out of account the *reaction* referred to in Newton's third law. If the earth attracts the moon, the moon in an equal degree attracts the earth; and, indeed, the explanation of the tides presently to be given turns upon this reciprocal action. The earth is *not* stationary as regards the moon, but it and our satellite revolve together about their *common centre of gravity*. This is a point situated in the straight line joining the centres of the two bodies, and at a distance from each centre inversely proportional to the mass of
80 the body. As the earth has about forty times the mass of the moon,
3 the point in question is about six thousand miles from the earth's centre, or two thousand miles above the part of its surface nearest to the moon. Again, it is not the earth which revolves in the approximately elliptical orbit about the sun, but this common centre of gravity. The centre of the earth is thus obviously six thousand miles within the orbital ellipse at each full moon, and six thousand miles beyond

it at each new moon. Nor is it true that the sun is stationary, for, as in the case of the earth and the moon, had there been but one planet, it and the sun would each describe similar orbits round their common centres of gravity. In short, the planets do not properly revolve about the sun, but the planets and the sun also revolve about the common centre of gravity of the whole system, and the sun approaches or recedes from this point according to the positions of the planets. The mass of the central luminary is, however, so great in comparison with that of the planets, that even if these were all ranged on one side of him, the common centre of gravity would not be more than half a radius from his surface.

The elliptical orbit is not the only one compatible with the law of gravitation, for, as already stated, the form of the orbit may be any one of the conic sections, but no other curve.

Those singular bodies which in ancient times had been ranked as meteors engendered in our atmosphere, but which Galileo had proved to be celestial, had been observed to move in orbits much more curved in one part than at others; but it was reserved for Newton to show that comets were regulated by the same laws as the planets. This was a result of great importance, for as comets enter the range of the solar system from every quarter of space, and from distances inconceivably vast, it proved that the influence of gravitation pervades all space.

The connection of the moon with the tides had been observed in all ages, yet no satisfactory explanation of the phenomena had been offered, when Kepler referred the tides to the moon's attraction. But the explanation he gave was so unsatisfactory that Galileo rejected it, and put forward an erroneous one of his own. Newton conclusively proved that the tides were due to the attraction of gravitation operating between the water of the ocean and the sun and moon respectively. According to the relative position of the two luminaries, their attractions produce effects which assist or oppose each other so as to produce a greater or less rise of the waters. The action of the sun is small compared with that of the moon, hence the tidal wave appears to follow the latter round the earth; and high water always occurs about three hours after the moon passes the meridian. When the sun and moon are in a line with the earth, which occurs at full moon and at new moon, then are the highest or *spring* tides. When the directions of the two luminaries are at right angles to each other the effect is weakest, and then occur the *neap* tides. Many persons who have no difficulty in understanding the action of the moon in attracting the water on the side of the earth next to itself, find the existence of an equally high tidal wave on the side opposite to the moon not so easily understood. It seems a contradiction to maintain that the moon's attraction draws the waters of the ocean towards herself on one side, and causes them to recede on the opposite side. But this recession is only relative: it is not the water which is heaped up from the earth, but the earth which is withdrawn from the water. We may consider the earth as a mass floating in the globe of water, and as its centre is nearer to the moon than are the waters on the opposite side, it will be drawn away from these, which, being thus left behind as it were, will be in the same relative situation to the earth as if they were heaped up. By comparing the spring and neap tides, Newton found that the attractive force which the moon exerts acts upon the ocean is to that exerted by the sun as 448 to 100; and that, while the former body produces a tide of 8·6 feet, the latter produces one of 1·9 feet high, and the two combined raise a tidal wave of 10½ feet high.

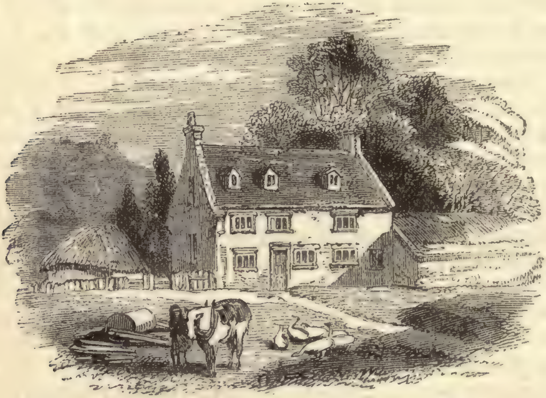

FIG. 87.—BIRTHPLACE OF NEWTON.

We have now briefly indicated the main points of the great discoveries and investigations of Newton in the fields of mathematical and astronomical science. These last constitute collectively the system of the world which was unfolded in the "*Principia.*" The phenomena of the heavens had for ages attracted the attention of men; but it was reserved for the illustrious English philosopher of the seventeenth century to combine and explain them by one great and simple law. But the materials for this, as for every great discovery, had been prepared by the labour of men of science of all times. Newton turned to account the geometry of Plato, and Euclid, and Apollonius; he combined into a few simple principles all the mechanical truths and dynamical laws discovered by Archimedes, and Galileo, and Huyghens; he developed into a new and powerful calculus the mathematical investigations of Wallis and Barrow; he availed himself of the telescopic discoveries of Galileo; and he explained the mysterious numerical relations which the labours of Kepler had brought to light.

The grand and simple principle of the system unfolded in the

"*Principia,*" and the cogency and clearness of the reasoning by which it was supported, would, it might be supposed, have caused it to be immediately received in all the schools of philosophy of Europe. It is, however, rarely the case that great truths find immediate acceptance. It was not now the philosophy of Aristotle that stood in the way. The system of Descartes had completely displaced that of Aristotle, and held possession of the learned and of the popular mind. The vortexes presented a perfectly intelligible point of view, and the cause thus assigned for the planetary motion was of so obvious a character that mathematical abstractions were less relied upon for the support of the theory than an appeal to apparently conclusive experiments. Newton's theory, on the other hand, presented ideas that could not fail to offer great difficulties, to the less trained minds at least: enormous masses of matter, so to speak, in empty space, and as planets retained in their orbits by some invisible influence connecting them with the sun. Even those accustomed to the vigour of mathematical reasoning would find in the establishing of the proofs of the Newtonian system an additional difficulty in the circumstance that a new kind of geometry, transcending the powers of the old methods, had to be invented for these investigations. It has been said that at the time the "*Principia*" appeared, there were not eight persons who understood the reasonings of the work. Even Leibnitz, misapprehending the principles of Newton's philosophy, regarded the idea of gravitation as a revival of the "occult qualities" of an ancient sect of philosophers, and he endeavoured to demonstrate astronomical truths on very different principles. Huyghens, who might be supposed well able to appreciate the new doctrine, could not admit that a mutual attraction existed between the *particles* of matter, but perceived that the law of attraction did subsist between the several planets. Bernouilli, one of the ablest mathematicians of the time, opposed the Newtonian system altogether. Cassini, Maraldi, and others, continued to adhere to most absurd methods of calculating cometary orbits after the publication of the "*Principia.*" Fontenelle, one of the most accomplished French *savans* of his time, continued to maintain the doctrines of Descartes.

The beginning of the eighteenth century, however, witnessed the introduction of the Newtonian philosophy into the universities of Great Britain, where the Cartesian theories perhaps lingered longest. Dr. Keill, the first person who publicly gave experimental lectures on natural philosophy, taught the principles of Newton's Physics in this way at Oxford as early as 1704. His plan, we are told, was to lay down very simple propositions, which he proved by experiments; from these propositions he deduced others, still confirming them by experiments. But it was at the hands of the great Continental mathematicians of the following century that the philosophy of the "*Principia*" received its complete development. From the works of Laplace, Lagrange, Biot, and others, the student becomes acquainted with the full scope of the principles Newton laid down, and learns that they range over the whole system of the universe.

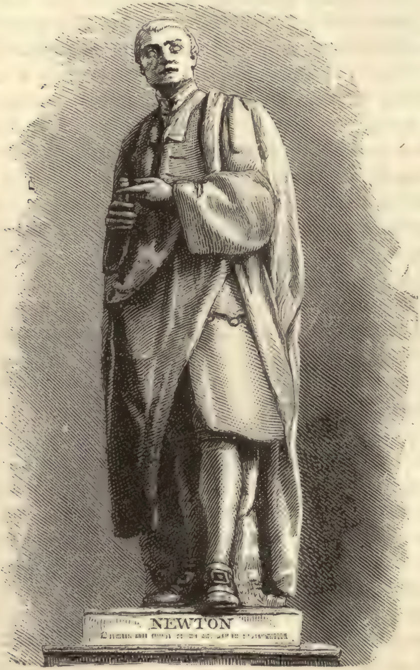

FIG. 88.—STATUE OF NEWTON, TRINITY COLLEGE, CAMBRIDGE.

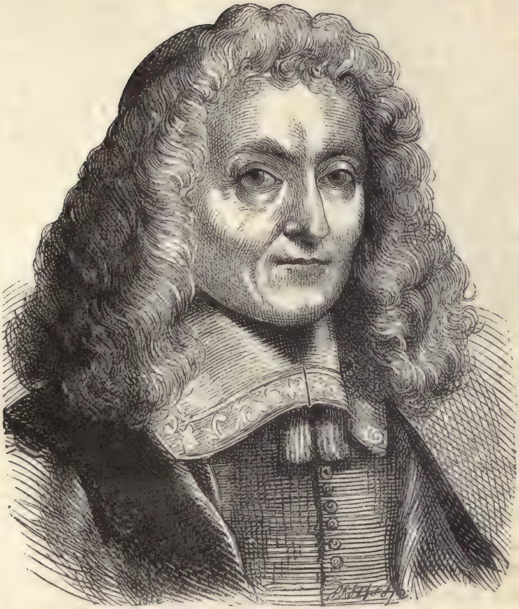

FIG. 89.—HUYGHENS.

# CHAPTER IX.

## ASTRONOMY AND PHYSICS OF THE SEVENTEENTH CENTURY.

IN the present chapter we shall mention the labours of those astronomers who followed Galileo, and either preceded or were contemporary with Newton; and we shall also describe the researches of Newton and others in general physics.

Among the most eminent of Galileo's followers was the celebrated GASSENDI, better known, however, as a philosopher than as an astronomer and physicist. In 1629 Kepler announced that astronomers should be ready to observe on the 7th of November, 1631, the transit of the planet Mercury. Among the few who succeeded in witnessing the predicted event was Gassendi, who observed the dark body of the planet as a very small round spot. Kepler had also predicted the passage of the planet Venus across the sun's disc for the 6th of December in the same year; but Gassendi in vain observed the sun several days before and after that date. A transit of Venus actually occurred, and it was seen by no mortal but by two young Englishmen, natives of Lancashire. One of these, Jeremiah Horrocks, had undoubtedly so great a genius for scientific work, that his premature death in 1640, at twenty years of age, must be regarded as a great loss to science. Horrocks had discovered some errors in the published tables, and he calculated that on the 4th of December, 1639, there would, contrary to the showing of Kepler's tables, be a transit of Venus, the precise hour for which he calculated. He wrote to a friend named Crabtree, asking him to observe the sun with a telescope at the time he named. The event realized his predictions, and these two young men had the satisfaction of observing the first transit of Venus ever witnessed. The importance of these transits of the inferior planets across the sun's disc, as phenomena confirming the Copernican theory is sufficiently obvious.

The next name to be mentioned is that of Huyghens, who held one of the highest places as a mathematician, an astronomer, and a physicist. He was born at the Hague on the 4th of April, 1629, and being intended for the legal profession, he was pursuing his studies at Leyden when he was attracted to mathematical learning, and soon made himself master of even the most difficult branches of the Cartesian geometry. His reputation in mathematics became so high that in 1665, when the Academy of Sciences at Paris was instituted, Louis XIV. invited him to reside in that capital, and he continued from 1666 to 1681 to enrich the *Mémoires* of the French Academy with a number of remarkable papers. At the revocation of the Edict of Nantes he returned to Holland, where he continued his scientific labours, and died on the 5th of June, 1695, in the sixty-fifth year of his age. One of the first of Huyghens' astronomical discoveries was that of the real figure of Saturn, the nature of which had been a puzzle to all preceding astronomers. He had given much attention to the art of grinding and polishing the lenses of telescopes, and he had succeeded in fabricating some excellent object-glasses of a very great focal length. He constructed a telescope of 23 feet focal length, with which he made the discovery of Saturn's real form, and subsequently he made other telescopes of much greater length. The difficulties of handling a telescope of so great a length were partly overcome by suppressing altogether the *tube* of the ordinary instrument, which, though the most conspicuous part of the telescope, is by no means essential.

When Galileo first turned his telescope to Saturn, he was astonished to find that planet accompanied, as it appeared to him, by two contiguous bodies, which he poetically compared to servants aiding Saturn in his old age. The nature of these adjuncts defied all his conjectures until forty years afterwards, when Huyghens saw Saturn with what appeared at first a line of light, which, as the planet passed into an-

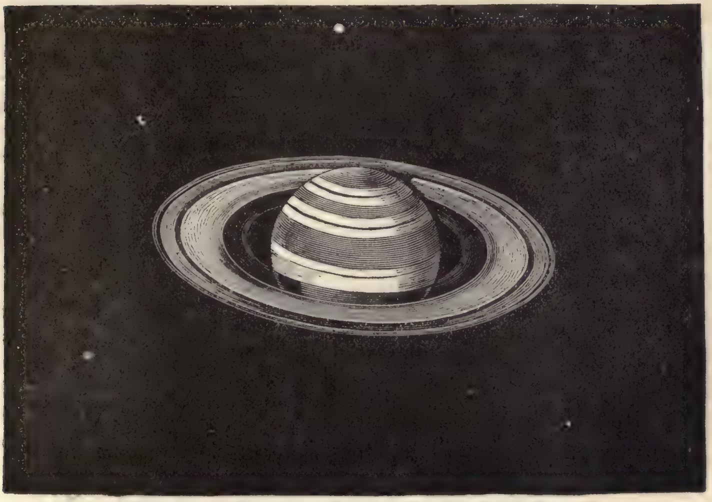

FIG. 90.—TELESCOPIC APPEARANCE OF SATURN.

other position with regard to the earth, changed into the appearance of a pair of long handles, and ultimately into the form of the ends of a narrow ellipse. In 1656 Huyghens published his discovery in the anagrammatic form which was the fashion of the time. The letters of the anagram when transposed gave the announcement, as he showed afterwards, that Saturn is surrounded by a thin flat ring inclined to the ecliptic, and not joined to the planet. Huyghens also first discovered a satellite of Saturn; and he appears to have convinced himself that there could be no more, by a reason which appears now very curious, namely, that the number of secondary planets of the solar system was by his discovery raised to six, and as that was the number of the primary planets, the number of satellites must be complete. The discovery by Cassini in 1671 of other satellites of Saturn brought this kind of speculation into discredit.

Among the discoveries and mechanical inventions of Huyghens, none is more noteworthy than his application of the pendulum to regulate the motion of clocks. The uniform duration of the oscillations of a pendulum was discovered, as we have already seen, by Galileo. It was Huyghens who devised the well-known arrangement by which the oscillations of the pendulum are made to control the motion of the clock. Fig. 91 will render the mechanism intelligible even to a person who has never examined the movements of an ordinary clock. *T T* is the rod of the pendulum attached to a fixed support at *a*, and bearing at its lower end the large mass *L*, called the *bob* of the pendulum. When a pendulum of this kind is made to swing backwards and forwards within a few degrees (say three or four) of its vertical position, its oscillations are performed in times which are practically equal, whatever may be the extent of each oscillation, provided it does not exceed some such small extent as we have named. The oscillations of the pendulum are communicated, by the arrangement shown in the figure, to the *pallets*, *m n*, which enter alternately into the spaces of the toothed wheel *R* in such a manner that at each oscillation one tooth of the wheel is allowed to pass the pallets. The wheel *R* is connected with a series of others, by which its motion is registered on the dial of the clock, and the whole train of wheels is actuated either by a weight or by a coiled spiral spring.

As the time of the oscillations of a pendulum moving in a circular arc is strictly not independent of the length of that arc, Huyghens investigated the kind of curve in which the suspended body must move in order that the oscillations might have absolutely equal periods whatever the extent of the motion. He found that the required condition was fulfilled when the pendulous body moved in a *cycloid.* The cycloid is the curve traced by a point in the circumference of a circle which rolls in a straight line. Thus if the circle P B, Fig. 92, rolls along the straight line P A, the point P in the circumference of the circle will trace out the curve P C A, which is called the cycloid. Thus a nail fixed in the circumference of a cart wheel might be made to trace a cycloid on an upright wall as the cart moved along the ground. The cycloid is one of the most interesting of geometrical curves from its properties and from its history, for while the former are very remarkable, no other geometrical line has given rise to so many disputes. The French geometer P. Mersenne and Galileo appear to have been the first to notice this curve, and it became the object of the profound study of several eminent geometers. Roberval in 1634 discovered that the area of the cycloid is exactly three times that of the generating circle. Descartes, Pascal, Torricelli, Wallis, Wren, and others, laboured successfully to solve various problems connected with this curve. Now, Huyghens discovered a property of the cycloid which we may thus illustrate: Let a board A D (Fig. 93) have a portion, E B F, cut out in the shape of an inverted cycloid, and let a groove be formed in which a marble may run by the action of gravity when the board is fixed in an upright position. Let B be the lowest point of the curve, then from whatever part of the curve the marble is allowed to commence its descent, it will arrive at B always after the *same* interval of time. It follows, therefore, that if the weight of a pendulum were constrained to follow a cycloidal path in the descent, it also would accomplish its oscillation in periods precisely equal, whatever might be their extent. Huyghens succeeded in contriving an arrangement by which this might be effected, and in doing so he discovered another curious property of the cycloid, and introduced a new idea into geometry, namely, that of *involutes.* Let C A, C D, Fig. 94, be two semi-cycloids produced by the equal circles G D and B A rolling along the straight line G B. Let a flexible but inextensible cord, C F, be attached to the point C, and let F be taken in the cord so that E F equals C G; then if the cord be kept tight

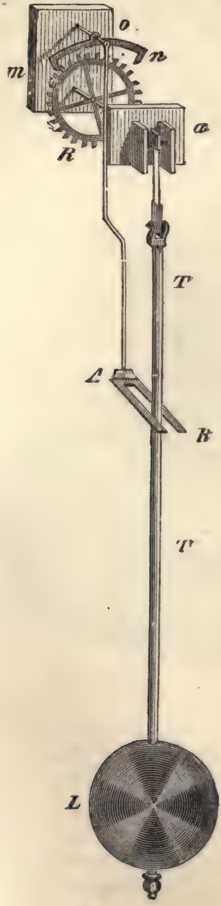

FIG. 91.

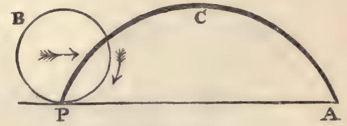

FIG. 92.

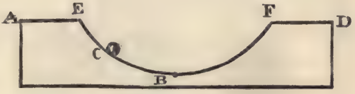

FIG. 93.

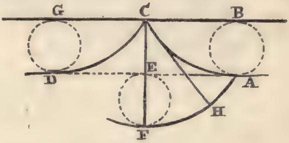

FIG. 94.

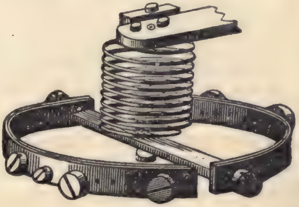

FIG. 95.

while it is wound round the curve A C, the point F will describe another cycloid, F H A, which is precisely the same as C A reversed.

Huyghens, availing himself of this singular property, suspended the rod of his pendulum by silken threads placed between cycloid blocks, like C D, C A in Fig. 94, so that the threads should apply themselves to the curved surfaces at each oscillation. This problem was the origin of the notion of *evolutes* and *involutes*. The curve F H A, formed as the manner stated above, is called the *involute* of C A, while the latter is the evolute of F H A. Elegant as is the idea of the cycloidal pendulum, it was found that the circular pendulum fulfilled more conveniently all the conditions required in practice.

Huyghens was the author of another invention of the greatest utility in the construction of timekeepers, namely, the application of the spiral spring which regulates the movements of the balance-wheel. This spring performs the same part for the balance that gravity does for a body oscillating in a cycloidal arc; for, as the wheel is moved from its position of rest, the spring tends to bring it back with a force proportional to the amount of displacement, and this is the condition necessary in order that the oscillations should be *isochronous—i.e.*, performed in equal times whatever their extent. Fig. 95 shows the balance-wheel of a chronometer, with the spiral spring A attached to the axis on which it turns. Fig. 96 shows the position of the balance-wheel in the chronometer, and its connection with the escapement by which the oscillations are made to control the motion of the train of wheels. Since the invention of the mechanism, many refinements have successively been introduced into timekeepers. The last two figures show an arrangement of the balance-wheel by which alterations in its rate of vibration, which would otherwise be occasioned by changes

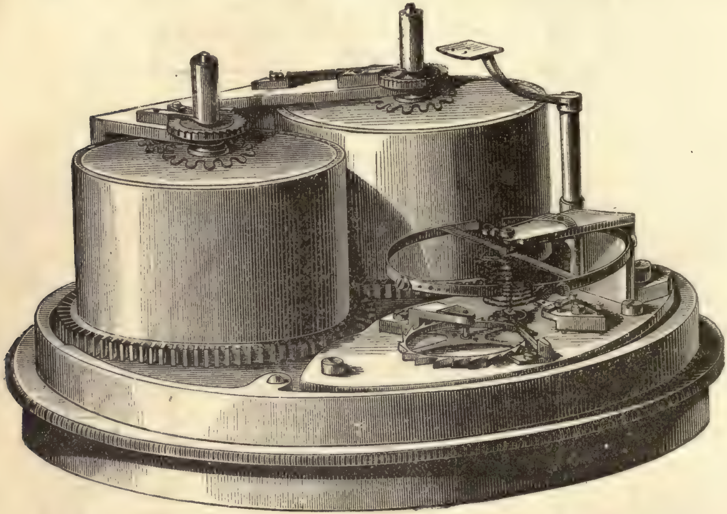

FIG. 96.

of temperature, are counteracted, or compensated as it is termed. Methods of compensating pendulums for changes of length, due to expansion and contraction of their suspending-rods, were afterwards devised. One of the forms best known is that represented in Fig. 97, and called the Harrison's Gridiron Pendulum. The weight o′ is suspended by a steel rod, which, at its upper end, is supported by a pair of brass rods; these in turn rest upon steel; and so on alternately; the lengths of the rods being so adjusted that the unequal expansibility

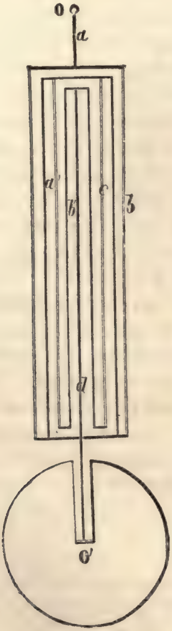

FIG. 97.

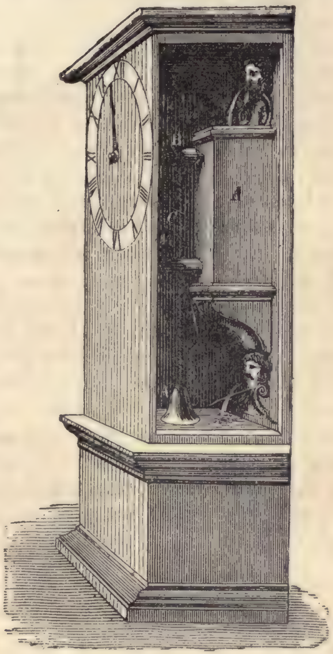

FIG. 98.

of the two metals is balanced, and the centre of gravity o′ of the pendulum weight remains always precisely at the same distance from the point of suspension o. Clocks had been made before the time of Huyghens, but they were deficient in the necessary principle of an exact regulating power. Before wheel clocks came into use, astronomers and others had to rely upon the indications of such instruments as the *clepsydra* or water-clock, Fig. 98. The clock is now an essential part of the fittings of the astronomical observatory. The positions of the stars are now determined by their height on the meridian, and by the times at which they pass it.

To Huyghens is due, also, the first idea of another instrument, which has been of vast service to astronomy. Every one knows that at the focus of the object-glass of the astronomical telescope there is an image perfectly similar to the object, and proportional in magnitude to the angle which the object subtends to the eye. The eye-piece of the telescope may be regarded as a lens for magnifying this image. Huyghens placed at that part of the telescope at which the image is formed a diaphragm with a circular opening, and he determined the angular magnitude corresponding with this opening by observing the time occupied by a star in traversing the diameter. When he wished to measure the angular magnitude of any interval, the diameter of a planet for instance, he introduced at the same part of the telescope a slip of metal which would just cover the interval. The width of the piece of metal, compared with that of the circular opening, would give the angular measurement required. The Micrometer was improved first by the introduction of fine threads, and finally the screw micrometer was introduced by Auzout in 1667. In this instrument there are two parallel threads, one of which is fixed and the other is attached to a sliding frame. The frame is moved by turning a fine screw and an index, and by the number of turns given to this the distance between the parallel threads is accurately known.

Another great improvement in the instruments of astronomy was made about this period, and its idea is due to an Englishman named Gascoigne. It consisted in applying the telescope to the graduated arcs or quadrants by which altitudes, etc., were measured. There are several advantages obtained by substituting the telescope for the plain sight. The unassisted eye cannot appreciate an angular space in the sky less than 30″; but if a telescope magnifying only thirty times is used, the position of an object may be determined to 1″. Many contrivances have been employed for obtaining accurate readings of the indications of graduated instruments, but for this purpose the *Vernier*, which was invented about this period, has proved the most effective. We specially mention these inventions for the accurate measurements of angles and lengths, as this accuracy has been one of the causes of the rapid advance of astronomy and other branches of science which has distinguished modern times from all previous ages. The vernier is a contrivance applied to the scales of all divided instruments which are required to be accurately read off. Its principle is very simple, and the mode of reading its indications may be easily described. Let the

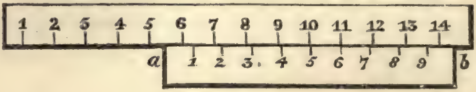

FIG. 99.

reader take a slip of cardboard, and, having marked upon it the exact length of 9 inches, as from *a* to *b*, Fig. 99, let this length be divided into ten equal parts, and let these be numbered from 0 to 10. The slip of cardboard thus divided will form a vernier to a scale of inches, such as a common foot-rule. Apply the vernier to the foot-rule, so that some one of the divisions on the cardboard shall exactly coincide with one of the inch divisions on the rule. In the figure the sixth vernier division is shown in a line with the 11th inch; and now observe that, going back on the vernier scale, each successive division is farther to the right than the corresponding inch division by 1, 2, 3, etc., tenths of an inch successively, and when you come to 0 this is distant from the 5th inch by $\frac{6}{10}$ths of an inch. We can therefore determine the position of *a*, with regard to the scale of inches, to be $5\frac{6}{10}$, by merely observing the coincidence of the *sixth* vernier division with one of the marks on the inch-rule, and similarly of any other of the vernier divisions. The thing to remark is that we can thus determine the position of *a* to $\frac{1}{10}$th of an inch, and yet employ a scale whose smallest divisions are nine times as great as the length we measure. We have here employed the inch merely as an illustration, for in practice the vernier is applied only to scales which have divisions so small that the vernier will in every place appear to have some one of its divisions coinciding with some one of the scale. The observer pays no attention to the number of this mark on the scale, but notes the number of the coinciding mark on the vernier; this gives him at once the number of tenths of a scale-division that the 0 point of the vernier is past the division of the scale which precedes it.

Continuing the account of the chief astronomical discoveries of the seventeenth century, we have to mention the labours of J. D. CASSINI (1625—1712), an Italian, who in 1669 was invited by Louis XIV. to take charge of the lately-established Observatory of Paris. One of the principal successes of Cassini was his discovery of the true theory of the movements of Jupiter's satellites. These, as everybody knows, are four in number, and they are eclipsed by the huge planet at almost each revolution, so that these phenomena are of very frequent occurrence. Though these satellites are invisible to the unassisted eye, they were attentively studied by astronomers, because, in the first place, they presented to the view a system similar to the solar system itself; and, secondly, because the frequent eclipses of the satellites furnished a means of determining the longitudes of places on the earth's surface. The eclipse of a satellite formed a signal, on seeing which the observer noted the hour at his station, and this, when compared with the hour at any other place where the same phenomenon was observed, would indicate the difference of the longitudes. Cassini was also the first who observed the rotation of Jupiter on his axis, and he made the like observation with regard to the planet Mars. He completed the discoveries of Huyghens in the Saturnian system by find-

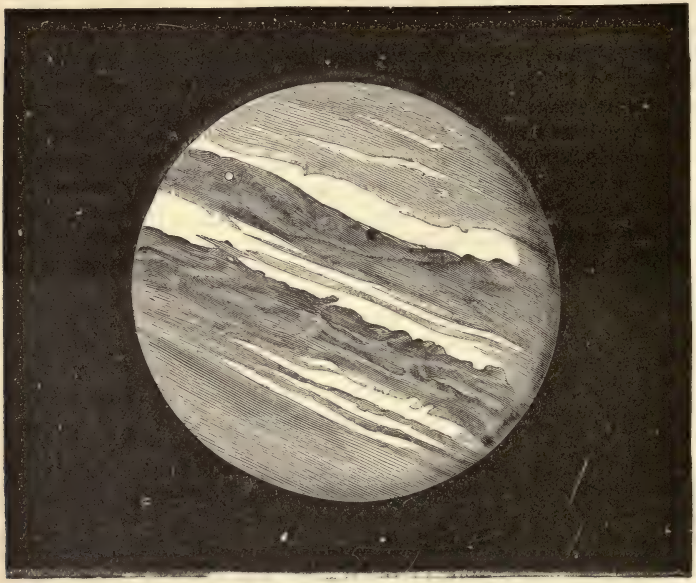

FIG. 100—TELESCOPIC APPEARANCE OF JUPITER.

ing four new satellites. The various other facts and methods which Cassini added to astronomy would still make a long list; but we shall here mention only his discovery of the *zodiacal light*, which presents the appearance of a luminous atmosphere enveloping the sun, and spread out in a lenticular form in the plane of the ecliptic.

The English astronomer, JOHN FLAMSTEAD (1649—1719) distinguished himself by a long-continued and laborious series of observations, and also by some excellent treatises on astronomical theory. Flamstead held the appointment of astronomer-royal, he being the first to fill the office when the national Observatory at Greenwich was established in 1675, some years after the foundation of the Observatory of Paris. The purposes for which our Greenwich institution was destined were of a different order from those which were aimed at in the establishment of the magnificent observatory for Tycho Brahe. The improvement of navigation and other purposes of practical utility could only be effected by an institution provided with more ample resources for the accurate observation and registration of astronomical phenomena than any individual could attain.

The successor of Flamstead at the Greenwich Observatory was EDMUND HALLEY (1656—1742), whose scientific career commenced at an early age. While quite a young man Halley undertook to form a catalogue of the stars which are too near the south pole of the heavens to be visible to European observers; and for this purpose he repaired to the island of St. Helena, where he remained for a year. An observation was made by Halley during his residence at St. Helena of a transit of the planet Mercury over the sun's disc, which suggested to him the idea of employing the transits of Mercury and Venus as the means of determining the parallax of the sun, and consequently the distance between the sun and the earth. We shall briefly explain the method of determining the sun's distance which Halley was the first to propose, and this explanation will have the more interest from the fact of the recent transit of Venus having been observed in various parts of the world with great care, in order to realize with all the refinements of modern appliances the suggestions of the illustrious astronomer of the seventeenth century. Parallax signifies that apparent change in the position of an object which is owing to a real change in the position of the observer. Two observers, who at the same instant view the sun or the moon from different places on the earth's surface, refer the luminary to different positions in the sky, just as two persons in a room would see a lamp on the table intercepting from view a different part of the opposite wall. The angle which measures this difference in the apparent positions of an object is called by astronomers its parallax, and the positions to which it is referred are usually assumed to be the centre of the earth and a point on its surface. The parallax in this sense would therefore be the same as the angle which the earth's radius would subtend when viewed from the sun; and in like manner the parallax of Venus would be the angle subtended by the earth's radius at that planet. The radius of the earth being known by the actual measurement of the length of a degree on its surface, the knowledge of the parallax of any heavenly body would give us its distance. Now, when the planet Venus comes betwixt us and the sun, she has of course a greater parallax, and an observer at one part of the earth will see the dark body of the planet crossing over the sun at a part of his disc different from that to which an observer at another part will refer the planet's path. Thus, the path for one observer may appear to cut off but a small segment of the sun's disc, as at C D, Fig. 101, while to another a parallel track, traversing perhaps a very large part of the disc, will be presented. In each case the observers notice very accurately the time occupied in the transit, and from this the parallax can be determined within the possibility of a very small error. Of course the difference of the times of passage of the planet is due to the parallax of the planet and the sun jointly; but as the *relative* distances of the sun and the planet are known by Kepler's law, and hence the *ratio* of their parallaxes, the actual parallax in each case is easily deducible from the observation which gives their *difference*. It was in

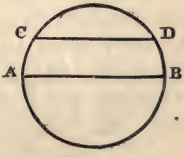

FIG. 101.

1716 that Halley drew the attention of astronomers to this elegant and accurate method; but as the next transit of Venus was calculated for 1761, he could not, being then sixty years of age, expect to live himself to witness it; but he exhorted future astronomers not to allow the opportunity to pass without uniting their efforts to obtain the best observations of this rare astronomical occurrence.

Halley devoted much attention to the moon's motions, and his improvements in the lunar tables were of great practical importance in the problem of finding the longitude. He detected the very slow but continuous acceleration of the moon's velocity, which is known as the *secular acceleration.* It was an additional glory for Halley to be the first who predicted the return of a comet. He observed that several successive appearances were recorded which presented certain elements in common, and he concluded that these might be reappearances of the same body travelling in an elliptic orbit. Newton had before in his "*Principia*" boldly assumed that comets, like planets, revolve about the sun. The comet of 1682 was identified by Halley with those of 1607 and 1531, and he predicted that it would return about the end of 1758 or the beginning of 1759. It was observed on the 25th of December, 1758, and was a conspicuous object in the heavens in the spring of 1759. It reappeared in 1836, and will not again be seen until 1912. Historical records of the appearances of great comets agree with the period of Halley's comet, and have been traced back to 11 B.C.

After the publication of the "Optics" of Descartes, this branch of science remained for a number of years without any remarkable addition, unless we reckon such inventions as that of the Magic Lantern by Kircher, 1646. But during the seventeenth century a remarkable and important series of discoveries completely changed the aspect of the science of light. JAMES GREGORY, of Aberdeen (1639—1675), an excellent mathematician, conceived the idea of the Reflecting Telescope in 1663, and the following year he visited London in order to get a speculum constructed, but he could find no artist capable of undertaking the task of grinding a speculum of a parabolic form, and he was obliged to renounce the execution of his project for the time. Gregory published a "Treatise on Optics," containing many valuable observations on optical instruments, particularly telescopes. Although Gregory failed in having his telescope carried into execution, on account of seeking for the perfect theoretical form for the mirror, the instrument as designed by him deserves notice as the first idea of the reflecting telescope. It is represented in section in Fig. 102, where O is the aperture of the tube, *a b* a concave mirror which reflects the incident rays back to the small concave mirror at N, whence they are again reflected towards a small circular opening in the centre of the principal mirror, in which is placed the eye-glass M *p*. Cassegrain, a Frenchman, modified this arrangement by substituting a convex mirror for the small concave one. Newton devoted much attention to the grinding of lenses and mirrors, and, acquainted with Gregory's proposed construction of a reflecting telescope, he in 1668 conceived that it would be an improvement not to pierce the mirror, but to reflect the rays coming from it to the side of the tube by means of a plain oval mirror. This construction is shown in Fig. 103. Newton constructed with his own hand a very small telescope in which this arrangement was adopted, and the instrument, which was only 6 inches in length, possesses the special interest of being the first reflecting telescope ever executed. The success of this attempt inspired Newton to prepare another reflecting telescope of larger size, and some time afterwards this instrument became the property of the Royal Society. It is still preserved in the Society's rooms, and its pedestal bears the inscription, "Invented by Sir Isaac Newton, and made with his own hands, 1671." Newton had afterwards the satisfaction of seeing his invention become an instrument of scientific utility.

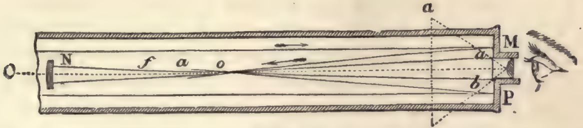

FIG. 102.

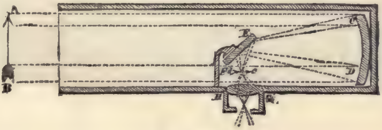

FIG. 103.

We shall now proceed to describe Newton's discoveries regarding Light—discoveries which are almost as remarkable for scientific sagacity as those which relate to universal gravitation. Newton informs us, that in 1666 he "procured a triangular glass prism, to try therewith the celebrated phenomena of colours," intending probably to repeat the experiment which Descartes had described (*see* page 160), merely as proving that the rainbow is produced by refraction. Newton's experiment was thus arranged: he made a small *round* hole in the window-shutter of a darkened room, so as to admit a ray of sunlight, A C, Fig. 104; in the path of this ray he placed the prism C, and this so refracted the ray, that on a white screen placed on the opposite wall appeared a coloured image, about five times as long as it was broad. These colours of this image or spectrum were always arranged in the same order, which, beginning from the most refracted portion, was violet, indigo, blue, green, orange, red. Newton found it at first "a very pleasing divertisement to view the vivid and intense colours produced thereby," but when he began to study the spectrum more attentively he was much struck with its great length as compared with its width. By all the laws of refraction then understood, the image on the screen should have been circular, whereas he found the sides of it rectilinear, and only in the extremities could a semicircular form be recognized. He tried, first, whether the thickness of the glass, or the size of the aperture in the window, or veins and defects in the glass, could be the cause of the elongation of the image. By substituting prisms of various angles and formed of various materials, reducing and enlarging the aperture in the shutter, and by other experiments, he found that the oblong image was not occasioned by any of these causes, and then he began to suspect that the received law of refraction (p. 155) could not be quite accurate; but this supposition had to be abandoned when he found that the length of the image was unaffected by a considerable change in the position of the prism. The removal, one after another, of these possible causes of the elongation of the image, at length led to the suggestion of an *experimentum crucis.* By receiving the image on a board having a small hole in it, he was able to intercept all the light except such as fell upon this hole, through which such light as passed was made to traverse a second prism placed behind the board before reaching the wall. By turning the first prism a little way about its axis, any part of the coloured image could be made to send its light through the opening in the board, and the observer could determine by the place on the wall how it was refracted by the second prism. Newton thus found, that in passing through the second prism, the violet light was more refracted than blue, blue than the yellow, and

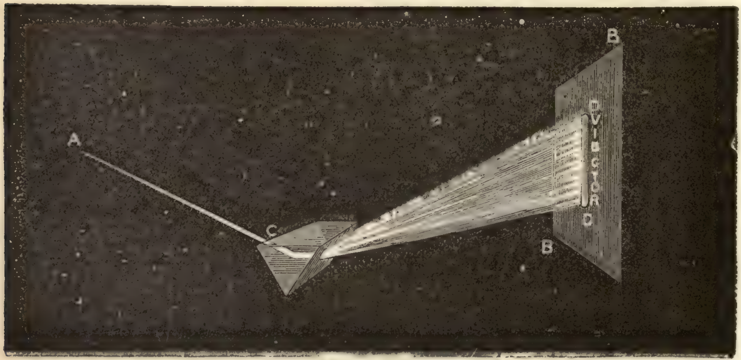

FIG. 104.

so on. Thus he demonstrated that the true cause of the lengthening of the image was no other than that light consists of rays of different refrangibility, which, though incident on the prism at one angle, were refracted in various degrees, and transmitted to different places on the wall. To the same degree of refrangibility belongs the same colour, and each colour has its own degree of refrangibility. Thus, the red rays are always the least refrangible, the violet the most refrangible; the yellow are more refrangible than the red, and less refrangible than the green rays; and so on.

Many experiments with different refracting substances were required to establish the invariability of this law; for obviously until trial had proved the contrary, there might possibly be some substances which would refract the red rays more than the violet, or the green less than the yellow. And the various coloured rays are plainly contained in the ordinary sunlight, which is thus a mixture of all those coloured rays that are separated by the prism by reason of their different refrangibilities, and dispersed in more or less diverse directions.

The colourless or white light of the sun, which was thus separated into rays of the various degrees of refrangibility, Newton conceived to be constituted of the *seven* different colours already named, viz., red, orange, yellow, green, blue, indigo, and violet: and he measured the relative lengths which these colours appeared to occupy in the spectrum. We may here remark that although it is quite easy to recognize these colours in the solar spectrum, there are innumerable other tints which would certainly have been reckoned among the constituents had these tints but had a *name* in common use. It is possible that the mystical significance which we have seen attached to certain numbers, of which seven is one, may have unconsciously influenced the enumeration of the constituents of the solar spectrum. The spectrum is, however, perfectly continuous, the tints passing from one to another by imperceptible gradations; and every part is, scientifically, equally entitled to be considered to possess a distinctive colour as those to which the above names have been assigned.

It should be observed also that the tints of the spectrum are never mixtures of other colours. Thus, for example, though the green lies between the blue and the yellow, it is not a compound of these—no further refraction will break up a ray of green light into blue and yellow. There is a very common notion, much fostered by popular teaching, that there are three primary colours, namely, red, blue, and yellow, and that all other colours result from various mixtures of these. This is quite erroneous as regards *colours* purely, but is related to certain well-known facts in connection with mixtures of *pigments*, and light transmitted through transparent coloured media.

Newton, having thus analysed light, proceeded to arrange experiments for the opposite or synthetical process of recombining the coloured rays. Thus, when all the colours of the spectrum were brought to a common focus by passing through a large convex lens, they reproduced white light. Another experiment adopted consisted in attempting to produce a white substance by mixing coloured *pigments*. But coloured powders are by their nature very different from the pure colours of the spectrum, and pigments which appear to the eye nearly of the same one colour produce very different effects when they are mixed with another. One of Newton's mixtures was formed of one part of red lead, four parts of *blue bice*, and a proper proportion of orpiment and verdigris. This mixture of red, blue, yellow, and green pigments appeared nearly of the colour of clean sawdust. A very elegant and easy method of recompounding white light is to take a circular piece of cardboard, divide into sections as shown in Fig. 105, and paint these with the colours named. When such a disc is made to revolve very rapidly on a pin passed through its centre, the disc appears very nearly white.

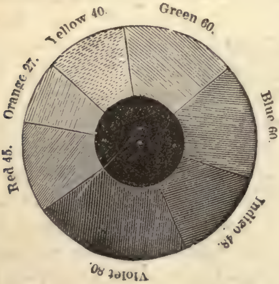

Fig. 105.

Newton deduced from the facts he had discovered that all the shades of colour which appear in objects can be imitated by intercepting certain rays of the spectrum and uniting the rest, and that a body cannot present a colour which is not contained in the incident light; and that the colours of bodies are not qualities inherent in the bodies themselves, but arise from the disposition of the particles of each body to absorb certain rays, and thus to reflect more copiously the other rays.

No sooner were these experiments and inferences announced than Newton's views were vigorously and pertinaciously attacked; but Newton triumphantly refuted all objections, though it perhaps cost him more trouble to detect his adversaries' blunders than to discover the truth they attempted to impugn.

It will be unnecessary here to enter into these and other controversies which Newton held with various persons. Among the disputants on this and other occasions was a man of very original powers, who only just failed to become one of the very greatest men of science of his time. This was DR. ROBERT HOOKE (1638—1703), who was one of the original members of the Royal Society. Hooke had great versatility of talent, and his acquirements were numerous and extensive; but he lacked fixedness of purpose. He could, but for the want of patient perseverance, and but for certain defects of temper, have rivalled Newton himself. Many are the ingenious inventions which attest the practical turn of his genius, and many more are the inventions which he left in an unfinished or imperfect condition. The like divergence in his speculations often turned him aside on the very threshold of important discoveries. Indeed, it has been said of Hooke that his brilliant endowments were so ill assorted that every one was neutralized by some other one, just as his ingenuity was marred by his versatility. Towards the close of his life, his disappointments exacerbated his defects of temper to such a degree, that he seems to have been brought to a state bordering upon derangement. His moroseness became extreme, and at last he refused to communicate his discoveries to the Royal Society, or to the public. Nevertheless, it is related of Hooke that for the last two or three years of his life he sat night and day at a table, so engrossed with his inventions that he never undressed or even went to bed. It is undoubtedly the fact that he had an inordinate desire for fame, and laid claims of priority to many great discoveries of his time; and probably from having pushed his speculations further than appeared in his published papers, he may in many cases have appeared the actual discoverer of results which his rivals were the first to perfect and publish. When Huyghens' application of the pendulum to clocks, and his idea of the cycloidal checks (p. 208) were announced, Hooke immediately claimed these for his own inventions. The spiral spring to which the balance-wheel of the watch is attached, as already explained in connection with Huyghens, has also been claimed as an invention of Hooke's. He certainly improved the diving-bell and the air-pump, and introduced more precision into the construction of astronomical instruments. His ingenious contrivances in certain details of machinery need not here be described. Indeed, so multifarious were Hooke's inventions and researches in almost every department of physical science, that his name appears in connection with most of the chief discoveries of his time. He seems to have been the only

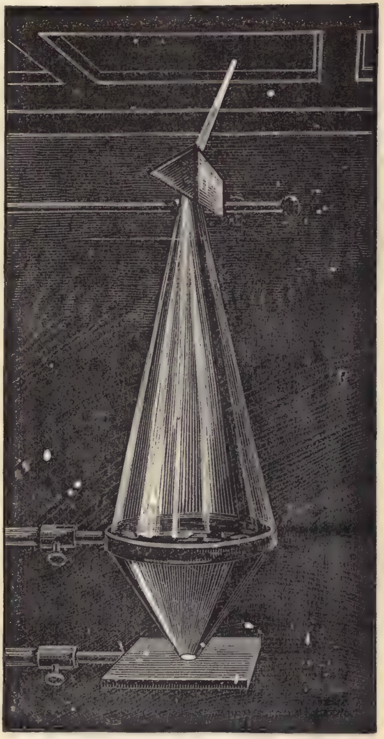

FIG. 106.—RECOMPOSITION OF LIGHT BY A LENS.

person who, previous to Newton, clearly expressed the idea of gravitation. In 1666 he describes some experiments undertaken to determine whether any variation in a weight of bodies is found when the bodies are at different distances from the earth's centre; and he was led to the idea of measuring the force of gravity by observing at different altitudes the rate of a pendulum clock. In a work published in 1674 he says, "I shall hereafter explain a system of the world differing in many particulars from any yet known, and depending on the three following suppositions: first, that all celestial bodies whatsoever have an attractive or gravitating power by which not only their parts are attracted to their own centre, but they mutually attract each other within the sphere of their activity. The second supposition is that all bodies put into a direct simple motion will so continue to move in a straight line till they are deflected by some other power, and made to move in a circle, ellipse, or other curve. The third supposition is that the attractive powers are the more powerful the nearer is the attracted body." He adds that he has not experimentally examined what law regulates the increase of the attractive power, but that he thinks an investigation into the subject would be of the greatest utility to astronomers.

When Newton presented his reflecting telescope to the Royal Society, Hooke criticised the instrument with undue severity, and boasted that he was himself acquainted with an infallible method of giving the utmost perfection to all optical instruments. The fact is that he had observed certain phenomena, which we shall presently mention, and from these he had come to the conclusion that light depended upon undulations of a highly elastic medium. The theory of colour which Hooke deduced from his speculations, was that there exist but two colours, *red* and *violet*, which correspond, as he imagined, to the two sides of a wave. Newton, on the other hand, preferred to regard light as a subtile matter thrown off from luminous bodies, but he pointed out that his doctrine of colours did not depend upon either one theory or the other.

Hooke soon afterwards laid before the Royal Society some curious observations on the colours produced by thin transparent films, such as soap-bubbles, and the excessively narrow layer of air between two plates of glass nearly in contact. He perceived that the colour depended in some way upon the thickness of the transparent film, but all his attempts to discover the relation between the thickness of the film and the colour produced were unsuccessful. He found it possible to split mica into films of extreme thinness, which gave divers brilliant colours. One film, for instance, gave a yellow colour, another a blue colour, but the two together produced a deep purple. The plates which produced these colours being so thin that at least twelve thousand of them would be required to make up the thickness of one inch, Hooke found it impossible, by any contrivance with which he was acquainted, to measure their thickness. Newton took up the investigation, and, with his usual sagacity, overcame the difficulty of measuring these extremely small spaces. He formed a film of air by pressing together two lenses of very large but known curvatures. The curvature of the convex lens was somewhat greater than that of the concave surface to which it was applied. Fig. 107 represents the lenses in section. As the radius of each curved surface is known, it is easy to calculate the distances which separate the glasses at any given distance from the central point *e*, where they are in contact. The double curvature is not essential to the success of the experiment, and the reader will be able readily to make the experiment of "Newton's Rings" by pressing an eye-glass or spectacle-lens of small curvature against a flat piece of plate glass. Fig. 108 may represent the section of the glasses in this case, and an obvious method of calculating the thickness of the separating film at any given distance, as *f g* from the point of contact, will probably suggest itself even to an ungeometrical reader, who will apply the famous theorem, mentioned on page 13, to the triangle *c g f*. The colours, as seen in the admirable experiment of Newton, present themselves in concentric rings, which, by ordinary light, appear of various colours; which, however, are not in the order of the colours of the prismatic spectrum. When only one kind of light is admitted—as, for instance, when it is first passed through red glass —a series of alternate light and dark rings are seen, as represented in Fig. 109 on a large scale. If yellow light—that, for instance, which is given off from a spirit-lamp with a salted wick—be employed, the dark and light rings will be more contracted than those formed with red light, blue light will give rings of more contracted dimensions than yellow light, and so on. If the several series of rings which each coloured light is capable of producing were superimposed, the effect would be that observed when the film is viewed in ordinary light. By examin-

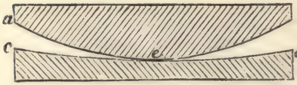

FIG. 107.

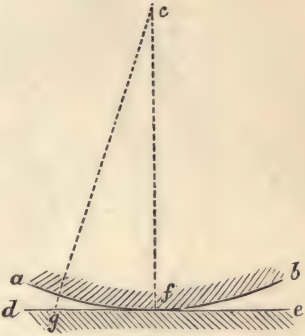

FIG. 108.

ing the phenomena in this way, Newton obtained the precise thickness of the air-film which corresponded with each simple colour. Fig. 109 represents the appearance of the light and dark rings on a large scale, when the lenticular air-film is viewed by *reflected* light. When light is allowed to pass through, another system of rings is perceived, exactly the reverse of the former, *i.e.*, the centre is now a light spot, and the space which the reflected light presented as a luminous ring now appears dark. Thus it appeared that, at those thicknesses where the light was not reflected, it was transmitted. A red ray, for example, was reflected or transmitted according as it fell upon one or another thickness of the air-film, differing from another part by an extremely small but assignable amount. Thus Newton found that at the places where certain bright rings were formed, the thicknesses were successively the following 178,000th parts of an inch—

1 3 5 7 9 etc.;

while the thicknesses corresponding with the dark rings were, reckoning in the same fraction of an inch,

2 4 6 8 10 etc.

Newton *expressed* these facts by a supposition that along a ray of light there were regularly alternating positions where the light was disposed to be in turns readily reflected and readily transmitted, or, as he expressed it, the ray had alternate *fits of easy reflection* and of *easy transmission.* Although Newton inclined to that theory of light which regards it as consisting of a material emanation—and the mode in which he has

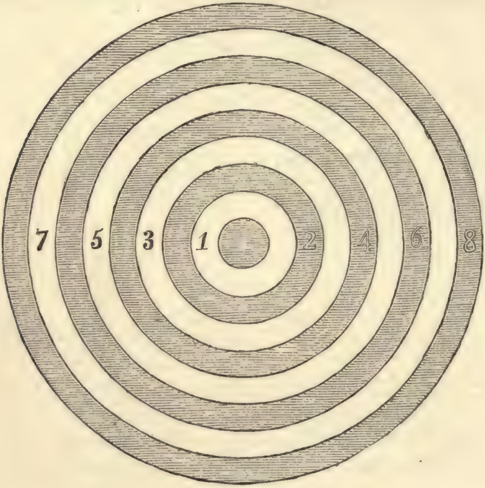

FIG. 109.

expressed the phenomena of thin films readily lends itself to that view —he expressly states that he does not mean more than that at alternate points in the ray something occurs which in one case disposes it for reflection, in the other for transmission : "what kind of disposition this is—whether it consists in a circulating or vibrating motion of the ray, or of the medium, or something else, I do not here inquire." The colours of thin plates have been much discussed since Newton's time, and numberless experiments have been made on this subject, but Newton's measurements have been found wonderfully accurate, and his determinations of the intervals along the rays retain their value, although the facts are explained by another theory of the nature of light.

By placing liquids instead of air between the glasses, Newton was able to study the colours produced in substances having greater refractive power, and he found that in proportion to the refractive power the rings contracted, that is, a less thickness of the film was required to produce them; and he expressed this, saying that the length of the *intervals* or *fits* was diminished when the light entered a more refracting medium. We shall not here be able to indicate all the various speculations connected with light and colours into which Newton entered; but one of his inferences has often been mentioned as a curious instance of an induction that had to wait more than a century for its verification. Newton had been trying experiments on the refractive powers of various transparent substances, and he observed that the refractive power for the most part followed the order of the densities of the various bodies. But he observed also a class of highly refractive substances whose power in this respect had no relation to their densities. He remarked that all these substances were what he called "unctuous and sulphureous," that is, inflammable. Finding that the high refractive power of the diamond gave it a position among these bodies, he hazarded the conjecture, which at that time seemed so improbable, that the diamond is in reality a combustible body—"an unctuous substance congealed." But although this conjecture has been verified, and phosphorus, a very inflammable substance, has also been found to possess a very high refractive power, no kind of connection between the combustibility and refractive power has been established.

An unexpected property of light was discovered by a learned Jesuit named Francis Maria Grimaldi (1619—1663), and announced to the world in a treatise published in 1665, two years after the author's death. He describes how, having admitted a beam of sunlight into a dark chamber through a pin-hole made in a sheet of lead, he found that the light diverged from the opening in a conical form to a greater degree that it should have done had it simply passed through in straight lines from the sun. He found also that the shadow of a hair placed in this light was larger than could be accounted for by supposing the light to pass in straight lines. The shadows in this case were surrounded by three coloured fringes, and within the shadow itself similar fringes might be detected. When light was allowed to stream in through two small apertures or pin-holes placed so near to each other that the cones of light intersected each other, Grimaldi observed that a spot common to the circumference of each was darker than the same spot illumined by either of the lines separately, and he announced the fact in this remarkable proposition: "*A body actually illuminated may be darkened by adding a light to that which it already receives.*" Such were the first observations of the remarkable phenomena of *diffraction* and *interference.*

In England Dr. Hooke occupied himself with experiments on this subject without knowing what had already been done by the Italian philosopher, and in 1672 he announced to the Royal Society that he had in preparation a paper "containing the discovery of a new property of light not mentioned by any previous writer on optics." Newton afterwards took up this subject, and he began by making accurate measurements of the diameter of a hair and of the breadth of the shadows, and he found that the breadth of the shadow was indeed greater than the proportional distance of the aperture from the hair. He explained the facts by supposing that rays passing near the hair are turned

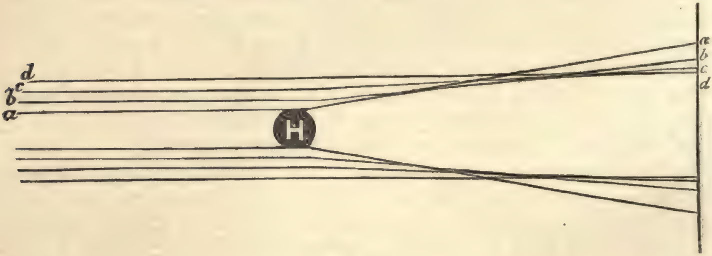

FIG. 110.

aside as if by a repulsive force. Thus H, Fig. 110, represents a section of the hair; and *a b c d* rays passing at different distances, the ray *a a* will be more deflected than *b b*, *b b* than *c c*, and so on; the result would be a crossing of the rays in such a manner that the shadow would be outwardly convex. Newton gave to this class of phenomena the name of *inflexion*. The phenomena of inflexion received, as we shall find, quite another explanation from Young and Fresnel.

The undulatory theory of light was first put forth by Huyghens in a communication addressed to the Academy of Sciences at Paris, in 1678; but afterwards he made this theory the subject of a work entitled, "*Traité de la Lumière,*" published in 1690. The original idea of Huyghens was that an inconceivably subtile and highly elastic medium, or *æther*, pervades all space and all bodies, and that within the denser transparent bodies this exists in a state of greater condensation. Waves, pulsations, or undulations are by some action of luminous bodies set up in this medium, and are propagated in it in all directions, and these pulsations reaching the eye, produce the sensation of sight. Under ordinary circumstances, the undulations spread from the point of origin in a regular spherical form, just as the wave produced by dropping a stone into still water spreads along on the surface in circles. This theory was applied by Huyghens to the explanation of all the ordinary phenomena of reflection and refraction. In reflection, the waves are thrown back from the reflecting surface in a way which is easily understood by any one who has observed the waves in a still pool when they arrive at an obstacle, such as a wall.

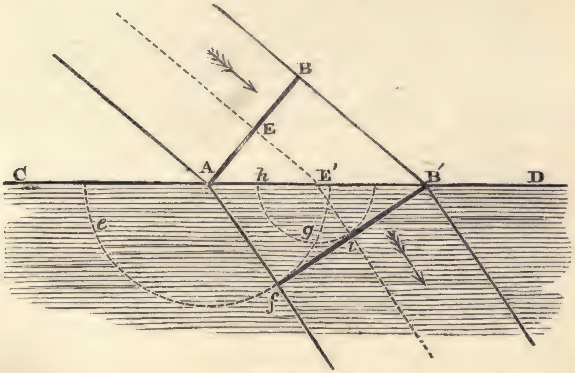

FIG. 111.

The undulatory theory explained very happily the laws of refraction; for as the light, according to this theory, is propagated with less velocity in the denser medium when a wave arrives obliquely at the common surface in passing from a rarer into a denser medium, its front will be changed, and it will move in a direction nearer to the perpendicular. Thus, if A B, Fig. 111, be the front of a wave advancing in the direction of the arrow, the parts of the wave successively entering the denser medium will move therein with a diminished velocity; and that this change of velocity must produce a change of direction, will be plain if we consider the matter with a little attention. Thus, while the part of the wave B is advancing to B′ in the rarer medium, A must have moved in the denser medium to some point which is less distant from A than B′ is from B. Suppose that the undulation advances in the denser medium with three-fourths of the velocity it has in the rarer, then if we describe from the centre A a circle, *e f g*, with a radius = three-fourths of B B′, we are sure that when B arrives at

B′, the point A must have advanced to some place in the circle *e f g*. Consider now a point E in the wave half-way between A and B: it will arrive at E′ in one-half the time required for B to pass to B′, and the other half of the time it will be moving in the denser medium; at the end of the period it must be somewhere in the circle *h i k*, which is described with a radius = three-fourths of E E′. Therefore the front of the wave when B arrives at B′ must be the straight line, B *i f*, which passes through B and touches the two circles; for it will be seen that whatever point in A B is considered, its place at the end of the interval will be determined by a circle which will touch B′ *f*.

The theory which thus admirably explained ordinary refraction was then applied by Huyghens to the case of the *extraordinary* ray in double refraction. He found that by assuming that the undulations in Iceland spar corresponding with this ray were propagated in a *spheroidal*, and not, as in the ordinary case, in a *spherical* form, a truthful representation of the observed phenomena might be obtained. But the undulatory theory was for the present advanced only as a hypothesis, to be received or rejected according to its applicability or otherwise to other facts. To the optical discoveries, however, which were made soon after the publication of Huyghens' treatise, his theory does not seem to have been applied.

We have already mentioned the attention given by astronomers to the system of satellites revolving about the planet Jupiter. A remarkable deviation from the known regularity of planetary motions was soon noticed in the periods of these satellites. Thus, Io, the satellite nearest to the planet, revolves round its primary in about 42½ hours, and at each revolution it is eclipsed by passing into the shadow of the planet. The period of the revolution of the satellite could be deduced with accuracy by the application of Kepler's laws from the known distance of the satellite from the primary planet. It was, however, observed that when the earth was at that position in which it is either nearest to or farthest from Jupiter, the interval between the eclipses agreed with the theory; but when the distance of the earth from Jupiter was *decreasing*, the eclipses occurred at shorter intervals of time; while, on the other hand, when the earth was retiring from Jupiter, the intervals of the eclipses were greater. The sum of the differences between the calculated intervals, and those observed while the earth was passing from, amount in each case to about 16½ minutes. The true explanation of this circumstance, as depending on a hitherto unrecognized property of light, probably occurred to the mind of more than one astronomer; but a Danish astronomer, named OLAUS ROEMER (1644—1710), was the first to announce, in 1676, that these differences were owing to the time required for the light to travel, and that the 16½ minutes represented the period it occupied in passing across the earth's orbit. Roemer calculated from this that light travels at the amazing velocity of 185,000 miles in a second.

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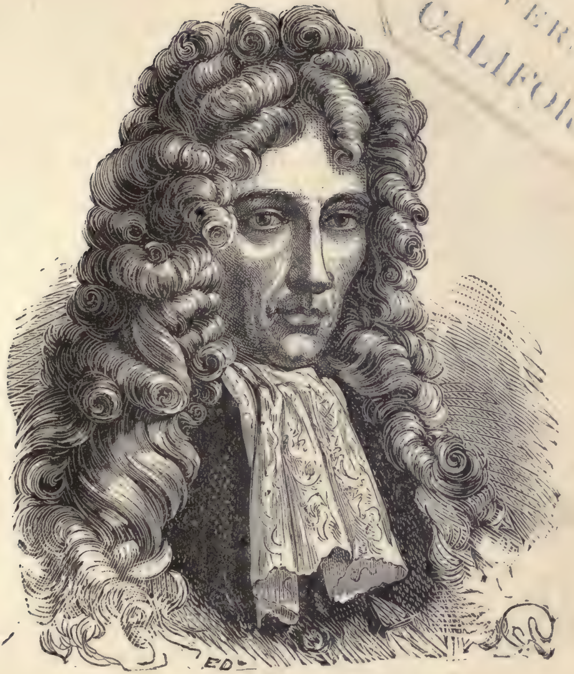

FIG. 112.—ROBERT BOYLE.

# CHAPTER X.

## CHEMISTRY AND NATURAL HISTORY SCIENCE OF THE SEVENTEENTH CENTURY.

AMONG the most successful followers of the new experimental method in science were the chemists Van Helmont and Robert Boyle. VAN HELMONT (1577—1644) was born at Brussels of a noble family and ancient lineage, and, had he been influenced by the ordinary ambitions, he would doubtless have made a considerable figure in his time as a courtier. Instead of following such a career, he applied himself with extraordinary assiduity to the study of science, preferring the toil of the laboratory to the splendour of the Court. His name has some lustre in the history of chemistry, for he was the first to call attention to certain invisible and impalpable, but nevertheless material bodies which up to his time had been overlooked. These bodies were the *gases;* and Van Helmont was also the first who introduced the word *gas* into science. This word is derived from *gahst* or *geist*, which signifies spirit. "Charcoal," he says, "and in general those bodies which are not immediately resolved into water, disengage by combustion *spiritum sylvestrem.* From 62 lbs. of oak charcoal 1 lb. of ash is obtained, therefore the remaining 61 lbs. are this *spiritus sylvestris.* This spirit, unknown hitherto, I call by the new name of *gas.* It cannot be enclosed in vessels, or reduced to a visible condition. There are bodies which contain this spirit and resolve themselves entirely into it: in these it exists in a fixed or solidified form, from which it is expelled by fermentation, as we observe in wine, bread, etc." This passage is remarkable, not only for the explicit mention of carbonic acid gas (as we now call it) as a product of fermentation, and for the introduction of the word *gas* for the first time, but also for the appeal to the *balance*,—the instrument that, as we shall presently see, made a complete revolution in chemical science. Van Helmont points out that the *gas sylvestre* is produced by the action of acids on shells, that it is present in caves and mines, that mineral waters contain it, and that it is engendered in putrefaction, combustion, etc. By "*gas sylvestre*" Van Helmont designated all gas which resisted his efforts to reduce it into a visible form. Thus he describes carbonic acid, sulphuric acid, and other gases under this name.

In Van Helmont's writings it is observed that he sometimes uses the word *element* in the ancient sense; but he admits only air, earth, and water—fire is excluded, because it does not form material combination with other bodies. Sometimes, on the other hand, he sides with the alchemists in recognizing as the elements of all bodies, salt, sulphur, and mercury. He remarks that in the solution of metal by an acid, the essence of the metal is not destroyed, but it exists in the solution as common salt does in water in which it has been dissolved.

Robert Boyle (1627—1691) was one of the original founders of the Royal Society, and his labours greatly contributed to the progress of more than one branch of science. His is a noble example of a life devoted to the study of nature in preference to the pursuit of worldly pleasures. He modestly declined to accept even those distinctions to which his position and attainments gave him a just claim. Though the son of an earl, he would not accept a peerage for himself, and he refused the presidentship of the Royal Society when that honour was offered to him. The advantages which birth and fortune placed in his hand were employed in promoting science by every possible means. The first scientific work he published appeared at Oxford in 1660 under the title of "New Experiments Physico-Mechanical touching the Spring of the Air and its Effects." He describes the successive improvements he effected in the construction of the original air-pump of Guericke. His chief object was to demonstrate the elasticity, or spring, as he calls it, of air, and his name is properly associated with that well-known law which he was the first to announce. This law, which was some years afterwards again discovered independently by the French physicist Mariotte, and is often called by his name also, is that the space occupied by a given mass of air varies inversely as the pressure.

Boyle expressed great doubt about the truth of either the ancient or the alchemical theory of elements. Thus he defies any one to show him the decomposition of gold into sulphur, mercury, and salt, and clearly points out the absurdity of the method of attempting to decompose bodies by heat, the result being in reality the formation of new compounds. Boyle was the first to draw a distinction of fundamental importance in chemistry between *compounds* and *mixtures*. In the former the constituents entirely lose their original properties, and these constituents cannot be mechanically separated from each other, whereas in mixtures the characteristic properties of each ingredient is perceptible, and they can easily be separated from each other by other than chemical means.

Boyle was the first to devise a method of collecting gases. He inverted a vessel filled with water containing sulphuric acid in another, and placed underneath the mouth of the inverted vessel some iron nails. This gas, as we know, was hydrogen; but Boyle did not examine so much its chemical as its physical properties. The experiment was regarded by him as a new instance of the production of *air* by an artificial method. Boyle made a vast number of experiments on the evaporations of various liquids, in the air and in a vacuum; on ebullition; on the congelation of water; on the barometer, etc. He made observations on the effects of placing animals of every kind in the vacuum of the air-pump. He proved in this way that fish require to breathe air dissolved in the water.

The origin of the rust which appears on metals was a question often discussed by the chemists of the seventeenth century. Boyle remarks that verdigris and the rust of iron are produced by corrosive effluvia in the air, and that it is by the study of these products that the composition of the air will be ascertained. After describing a great number of experiments, he comes to the conclusion that there is in the air some vital substance which plays a part in such phenomena as combustion, respiration, and fermentation. When this substance has once been consumed, flame is instantly extinguished, and yet the air thus extinguishing flame has had very little of its substance removed. In a treatise entitled "Fire and Flame weighed in a Balance," Boyle describes a series of experiments on the increase of the weight of metals (lead, copper, tin) by calcination. As he obtained results not differing much whether the crucible in which the metal was heated was opened or covered, he did not trace the effect to the air, but attributed it to the fixation of the particles of fire which passed through the pores of the crucible.

Boyle first proposed several of the methods which the analytical chemist still uses as the most delicate means of recognizing the presence of certain substances. For instance, it was he who introduced the use of vegetable infusions, or papers tinted with them, to find whether a liquid be acid or alkaline. The employment of tincture of galls to discover traces of ferruginous compounds, of nitrate of silver to detect common salt, and of ammonia to recognize the presence of copper, is due to Boyle. He complains of the chemistry of his time for being merely a matter of routine—a collection of experiments without connection or philosophic order, and resting on no solid principle.

Among the noteworthy chemists of the seventeenth century may be named JOHANN KUNCKEL (1612—1702), the discoverer of phosphorus, and he freely communicated his method of preparing that substance to several persons, although he did not publish the method in his works, in order, as he says, that he might not become the indirect cause of serious accidents arising from the very inflammable nature of the substance. It should be mentioned that a Dr. Brand, of Hamburg, was the first who prepared phosphorus, but he kept the process secret, Kunckel in Germany, and Boyle in England, appear, however, to have independently rediscovered the process, guided only by a knowledge that the source of the new substance was urine. Kunckel attacked the alchemical theory, and declared that, after working in chemistry for sixty years, he had neither discovered *sulfur fixum* nor found how it could make part of any metal.

JOHANN JOACHIM BECHER (1635—1682), a German chemist, whose writings show some tendencies towards the alchemical doctrines, is chiefly noticeable by the circumstance of his works containing the germs of a theory which was developed by the Bavarian chemist and physician, GEORGE ERNEST STAHL (1660—1734), who afterwards became very famous. It was the doctrine of Becher that metals contain a combustible principle, which he calls "an inflammable earth." This doctrine was the foundation of Stahl's theory of *Phlogiston*. Such was the name Stahl gave to the "inflammable earth" of Becher, and he regarded it as a subtle principle residing in inflammable bodies and in metals. Fire is—according to Stahl—nothing but rapid escape of the phlogiston contained in the combustible body. When a metal, lead for example, is heated in the air, it gives up its phlogiston, and is converted into a dull powder, or *calx*, as it was then termed. The *calx* of lead, or litharge, is lead deprived of its phlogiston, and it has only to be again united with phlogiston to become again the metal. If, therefore, lead calx be heated with charcoal, which contains much phlogiston, the phlogiston will leave the charcoal, and, uniting with the calx, will reproduce the metallic lead. Thus, according to Stahl, the calcination of a metal is an analytical operation—that is, a separation of the metal into the *calx* and *phlogiston* of which it is compounded;

while he regards the *reduction* of the calx into the metallic form as a synthetical process—that is, a re-combination of calx and phlogiston. He has an answer to the objection that the calx is heavier than the metal — a fact which, as we have seen, was known to Boyle. "This fact," says Stahl, "so far from damaging my theory, supports it. For phlogiston, being lighter than air, tends to lift up the bodies it combines with, so that they lose a part of their weight; hence a body which has parted with its phlogiston weighs more than before." The phlogiston theory reigned for half a century amongst chemists, and its history is an example of the manner in which a scientific theory may be accepted as a simple and adequate explanation of a group of facts, until further discoveries concerning allied facts require the theory to be modified by additional hypotheses and speculative explanations, until it becomes so involved in complications as to be useless as a bond of union for the facts. We shall again refer to the disputes to which the phlogiston theory gave rise towards the end of the eighteenth century, when scarcely two chemists would be found to give the same theoretical explanation of the facts of their science.

The works of Stahl are curious in a literary sense from the strange mixture of German with the Latin. Here is a translation of a passage:

"Besides, by the above-mentioned alteration in metals, it must be observed that they contain three principles or substances. 1°. a substance of almost superficial cohesion, which first goes off, that is to say, the inflammable substance or *phlogiston;* 2°. a colouring substance which is seen in the coloured glasses of these metals; and finally: 3°. a cruder substance, which is found particularly in the thicker metals, iron and copper." The text of this passage runs thus:

"*Sonsten ist aus den angeführten* alterationibus metallorum *zu notiren dass in den* metallis imperfectis *dreyerley* substantia *vorhanden sey:* 1°. *eine* quasi superficialis cohœsionis quæ et ea propter omnia prima abit scilicet substantia inflammabilis seu φλογιστον; 2°. substantia colorans, quæ apparet in coloratis horum metallorum vitris, *und endlich;* 3°. substantia crudior, *und diese sonderlich in den* crassioribus metallis, *Eisen und Kupfer zu finden.*"

The study of gases, the existence of which had been demonstrated by Van Helmont and Boyle, was the foundation of the science of chemistry. The most valuable results were obtained from investigations on the air, respiration, combustion, fermentation, irrespirable airs, gases in mineral water, and similar subjects, which offered a hitherto unworked mine of scientific truths. Most persons having any taste for experimental science in the seventeenth century dabbled in chemistry, and the store of facts began to accumulate rapidly. Sir Christopher Wren, Dr. Hooke, Huyghens, and others, each contributed some experimental truths to the growing science. A young English physician, named JOHN MAYOW (1645—1679), published in 1674 treatises on nitre and on respiration, in which he described experiments that are, perhaps, the most remarkable made during the seventeenth century. He showed that nitre, a substance which had been the theme of endless discussions, was composed of an acid and an alkali, and that it contained a spirit (or gas) capable of actively supporting combustion; that antimony was capable of fixing this gas, and was thereby increased in weight. He proved that flame is maintained at the expense of something in the air, and that the same matter which maintains combustion in air enters into the composition of nitre as its most active ingredient; for a mixture of sulphur and nitre may be inflammable in a vacuum, giving the same products as sulphur inflamed in air. Mayow shows that the "nitro-ærian spirit" is the air which maintains combustion, by inverting a bell-jar over a lighted candle floating on the surface of the water within the jar. The water will rise within the jar as the candle removes the "nitro-ærian" particles. The same result was obtained in similar experiments, in which other inflammable bodies, such as camphor, sulphur, etc., were ignited within the jar by concentrating on them the sun's rays by means of a lens or burning-glass. He observes, that after the flame goes out, it is impossible to rekindle it. He confined a mouse in a vessel covered by a piece of moist bladder, and after a few minutes the bladder bulged inwards, showing that something was being removed from the air; and, in fact, when a mouse or other animal was suspended in a cage within a bell-jar inverted over water, he saw the water rise above the outside level, just as in the case of the burning candle. He found, also, that in each case the bulk of the air was diminished by about one-fourteenth. The air in which a candle had been extinguished was incapable of supporting life in an animal, and air which had been exhausted by respiration extinguished the flame of a candle. The conclusion drawn from these experiments is that *the air loses some of its elastic force by the respiration of animals, as it does also by combustion; and animals remove from it particles of the same nature as flame removes.*

Mayow found that respiration changed the dark colour of venous blood into the bright red colour of arterial blood, and he compares respiration to fermentation in so far as in each there is an absorption of the *igno-ærian* particles. Nor does he hesitate to attribute animal warmth to this absorption.

The experiment of producing and collecting hydrogen, which we have seen had been performed by Boyle, was repeated by Mayow with nitric acid, and, strangely enough, without his observing its inflammability or other properties by which it differs from air. The experiment is described as one to determine whether air can be generated *de novo.* Mayow found that the aëriform body he had collected was permanent; it could not be reduced to a liquid by any cold he could apply. He says, however, that although this substance has the same appearance and elasticity as air, it is hard to believe that it is really such. We see here a little progress, inasmuch as Mayow doubts whether the hydrogen gas (as we now call it) can be real air, whereas Boyle had unhesitatingly accepted it for such.

Mayow was only twenty-nine years of age when he published these researches, in which he describes many new experiments, and incorporates all that his predecessors had advanced on the subject which could be accepted as verified truths. Unfortunately for science, Mayow's life was cut short at the early age of thirty-four, or he might have advanced by a century the knowledge we owe to Lavoisier, as he was on a track which could not have failed to bring him to the goal at which the illustrious Frenchman afterwards arrived. After Mayow's death the path in which he set out was not pursued, but on the subject of combustion philosophers were completely led astray by the false theory of *phlogiston.* Scarcely anything is known of Mayow's life beyond the facts that he was born in Cornwall, graduated as Doctor of Medicine at All Souls' College, Oxford, practised as a physician in Bath, and died in London in 1679.

We place before our readers the records only of such discoveries in chemistry as have contributed more especially to the philosophical structure of the science. It is needless to say that vast stores of useful facts were being accumulated in every branch of applied or industrial chemistry, such as *pharmacy*, *metallurgy*, and *technology*, but a catalogue of these could not possibly interest the general reader. The widespread interest in chemical studies during the seventeenth century is manifested by great numbers of handbooks of chemistry, which were published not only in Latin but in the modern languages. These treatises were in general devoted to some branch of applied chemistry, especially to pharmacy and medicine.

The old alchemical pursuits had not ceased to allure men's imaginations at the period of which we are now speaking. There were founded, even towards the close of this period, small societies having among their objects the discovery of the philosopher's stone, the elixir, and the like. Alchemists there were who actually pretended to exhibit transmutations; but these were no doubt cunning impostors. But in Germany there originated a confraternity which professed to possess the great secrets. This was the Brotherhood of the Rosy Cross, so called because the founder was named Rosenkreuz. He was said to have acquired in Arabia more than the fabled wisdom of the East, and he ordered that a hundred years after his death his tomb should be opened. When this was done, in 1604, a book was found written in letters of gold, in which great secrets were revealed. The brethren of the Rosy Cross declared that they were destined to regenerate the world, and their doctrines were, as usual, mixed up with much religious mysticism. The principal powers of which they claimed the possession were the transmutation of metals, the art of preserving life for several centuries, of knowing all that was passing in different countries, and generally of obtaining by the mysteries of their science a knowledge of even the most occult truths. The pretensions of the Rosicrucians were attacked by some of the more enlightened men of science; but their doctrines had also some defenders.

Zoology is usually understood to include the description of animals, and the methodical arrangement of them. Using this word in a more extended sense, we may include under it certain branches of knowledge which are sometimes treated of apart. Thus it was soon found that no systematic classification of animals was of any value unless it were founded on the structure of the animals; and the knowledge of this structure, which can of course only be obtained by dissection, constitutes *Anatomy*. Again, the knowledge of the structure of the organs of the animal body would give little information about the animal as a whole, unless the actions of the various organs are studied; and this is the object of the science of *Physiology*. The anatomy and physiology of the human body have been studied with great care, because the important arts of curing diseases and of healing injuries could make little progress without that kind of study. The anatomy and physiology of the human body, as the bases of surgery and medicine, being thus of so much practical importance, the words *anatomy* and *physiology*, when used without qualification, are generally taken to imply the anatomy and physiology of the human body, while, to express like knowledge of the bodies of the lower animals, the terms *comparative anatomy* and *comparative physiology* are employed. There is another branch of zoological science upon which the practice of medicine and surgery is largely dependent, and that is *Pathology*, which relates to everything concerning diseases of the human subject especially.

The earliest zoological writer is perhaps Aristotle, whose treatise has already been mentioned (p. 28). Pliny has given a sketch of the classes comprised under quadrupeds, fishes, birds, and insects; and ÆLIAN, who wrote in Greek in the second century, put forth a treatise on animals which was an improvement on the writings of his predecessors. From that time until we meet with ALDROVANDUS (1522—1607), an Italian, who published six large folios on zoology, the science appears to have been neglected. Aldrovandus gave woodcuts of many of the animals he described; but his work is deficient in arrangement, and contains an immense amount of matter introduced without any discrimination.

Among the ancients, the only authors who have treated particularly of plants are Theophrastus (who has been already referred to on page 32), Discorides, and Pliny. DISCORIDES, who flourished during the reign of Nero, was the author of a work on "*Materia Medica*," and he was long regarded as the great authority for everything relating to plants, and at the revival of learning, commentators were at great pains to identify the particular species of plants to which his descriptions refer. Pliny the Elder (23—79), is almost the only naturalist of note who ever appeared among the Romans. His "Natural History" is a compilation from all the authors known in his time, and it includes every branch of natural knowledge recognized by the ancients. The work is divided into thirty-seven books, of which several contain accounts of plants. The number of species mentioned is upwards of six hundred, but the descriptions are by no means accurate. Like his predecessors, he treats particularly of plants employed in medicine. The attempts made in the sixteenth century by commentators to make out the plants described by these ancient authors, proved so unsatisfactory that physicians began themselves to examine and collect plants, and their published catalogues soon showed that so far from the ancients having enumerated all the species of plants, they had in reality described only a comparatively small number. Until the time of KONRAD GESNER (1516—1565), however, no attempt was made to methodically arrange the plants described. Gesner, who was professor of Natural History at Zurich about the year 1555, first suggested the division of plants into classes, orders, genera, and species, and this kind of division has been followed by botanists ever since; although various grounds of division have been proposed, Gesner selected the *flowers* and the *fruit* as the parts of the plant on which the classification was to be based.

Gesner was also the author of a book in which he describes and represents by figures the shapes of fossil shells, and of the crystalline forms of many minerals.

ANDREA CÆSALPINUS (1519—1603), a physician, and professor of botany at the University of Padua, proposed a classification of plants founded on the characters of their fruits or seeds. His descriptions are excellent, and include about fifteen hundred species of plants. His method does not appear to have been generally accepted, and by the end of the sixteenth century a great number of writings on plants had been published; but the whole subject was thrown into confusion by the various names given to the same plants by different authors. This state of things was in a great measure remedied by the labours of two brothers, named BAUHIN (JOHN, 1541—1613; GASPARD, 1560—1624), natives of Switzerland, one of whom published in 1623 a systematical index, in which might be found all the names under which different authors had described each plant.

When the Royal Society of London was founded, one of its declared objects was the advancement of botanical science, and for this purpose persons were engaged to collect plants, etc., in England and in foreign countries. But before this period some botanists had laboured in England. The first British Flora was published by How in 1650, describing one thousand two hundred species, and in 1667 Dr. MERRET issued his work on the "Vegetables, Animals, and Fossils of Britain." In this the number of species of British plants is increased to one thousand four hundred. Many distinguished botanical writers appeared during the seventeenth century in Britain and elsewhere, but we may here pass over their names and labours, in order to devote a little space to the lives and works of two of the most eminent English naturalists of the seventeenth century.

JOHN RAY (1628—1705) was the son of a common blacksmith, but he received his education at a grammar school at Braintree in Essex. He graduated at Cambridge, and was elected to a fellowship there, afterwards holding lectureships in Greek, mathematics, etc. Among his pupils at the university was FRANCIS WILLUGHBY (1635—1672), a young Warwickshire squire, who, though seven years the junior of Ray, became much attached to him, in consequence of the coincidence of their tastes and pursuits as regards the study of natural history. This community of intellectual interests formed the basis of a warm and lifelong friendship between the two men. Ray took orders in the Church of England, but he found himself unable to comply with the conditions which an Act passed in 1662 imposed on those holding a fellowship: he was deprived of his emolument. He had before this made a tour through certain parts of England and Wales in order to study their natural history, and again in 1662 he travelled, in the company of Mr. Willughby, over the northern counties and Scotland, and over the greater part of the southern part of Britain. Ray appears to have taken under his particular care the botany, while Willughby worked at the zoological branch; but both also laboured together in all departments of natural history. The two friends made several journeys between 1663 and 1667 in pursuit of their science, visiting France, Holland, Germany, Italy, Malta, and various parts of England. Willughby married, and Ray now undertook some journeys alone; but much of his time was spent at Willughby's seat in Warwickshire. In 1671 Ray's friend was carried off by a fever in the thirty-seventh year of his age. He left Ray an annuity of £70 a year, confided to him the education of his two sons, and charged him to complete and publish the works on zoology he left unfinished. Ray continued to reside at Middleton Hall until Mr. Willughby's sons were removed from his care, and he then proceeded to discharge his literary trusts, editing his deceased friend's works on birds and on fishes with the most affectionate care, and, it is presumed, with much important additional matter furnished from his own stores. The Ornithology appeared in 1676 in Latin, and two years afterwards an English edition was prepared by Ray. The Ichthology was published in 1686. It had been left in a very incomplete state, so that the first and second books were entirely written by Ray, and he probably added much to the other two. Ray contributed more largely to the progress of zoology than any person of his time. His classification of animals surpasses in scientific merit that of any previous writer. The following table gives the general plan of Ray's division of animals.

| | | | |
|---|---|---|---|
| Animals having red blood. | Breathing by lungs. | Heart with two ventricles. | Viviparous Quadrupeds.<br>Whales.<br>Birds. |
| | | Heart with one ventricle. | Reptiles. |
| | Breathing by gills | | Fishes. |
| Animals destitute of red blood. | Greater | | Mollusca.<br>Crustacea.<br>Shell-Fish. |
| | Less | | Insects. |

Ray's first botanical work appeared in 1660, but his most extensive and laborious production was a "General History of Plants," in a thick folio volume, published in 1704, giving everything known about the subject, and containing notices of no fewer than eighteen thousand six hundred plants. An elaborate attempt to classify plants in natural orders, founded chiefly on the characters of the fruits, was published by Ray in 1668. Plants were first divided into two sets, namely, Herbs in one, Trees and Shrubs in the other. The first set included twenty-five classes, the second eight classes.

Ray died in 1705, at the age of seventy-seven, and with him we may conclude the history of botany and zoology to the end of the seventeenth century,—so far, at least, as those sciences are connected with description and classification. But it is evident that classification, even if that were the only object of these branches of natural history, could have no value if founded merely on external forms. A knowledge of the structure of the various organs possessed by animals and vegetables, and of the several functions of those organs, is as essential for any truly scientific classification of living things as it is necessary for the understanding of the nature of any given animal or vegetable, or the right conception of animal and vegetable life in general. Here we have to note a few of the steps by which the knowledge of anatomy and physiology, both of vegetables and animals, has advanced.

In Aristotle's treatise on animals there are contained many facts of comparative anatomy, and it is probable that such knowledge as the ancients possessed of human anatomy was principally obtained, as far as dissection was necessary, by the dissection of animals. The ideas of the ancient Greeks concerning rites to which the bodies of the dead were entitled, and the guilt which was ascribed to any omission of duty in this respect, must have left to the physicians few opportunities of dissecting human bodies, even if any persons were bold enough to venture. Hippocrates, the father of medicine, has so little to say about anatomy, that it is considered doubtful whether he even had at any time attempted to dissect the body of an animal. Celsus (*c.* A.D. 20), in his system of medicine, has as little knowledge of the structure of the body as his predecessor. Galen (130—200), however, the great medical oracle of the Middle Ages, left an anatomical account of the human body, which remained unsurpassed for a thousand years; for neither the Arabian physicians nor the men who disputed on medical *theses* in the schools of the Middle Ages added anything to the anatomy of Galen. In the fifteenth century, when human bodies came to be dissected in the medical schools, the errors and defects of Galen began to be gradually detected. At length ANDREW VESALIUS (1514—1564), of Brussels, a man of independent fortune, and so enthusiastic an anatomist that he would rob the gibbets and dissect the bodies in his bed-room, boldly affirmed the inaccuracy of Galen's knowledge of anatomy, and showed conclusively that it was wholly derived from the dissection of animals. In 1543 Vesalius published his "System of Human Anatomy," in which the various parts were carefully and minutely described and excellently figured; and the work was speedily acknowledged to be infinitely superior to all previous systems. The authority of Galen thus received a shock from which it never afterwards recovered; but Vesalius had to pay for his triumph in the opposition and abuse of the Galenists. Nearly all the physicians of the period rose up in arms to defend their oracle; but Vesalius, having the facts on his side, was able single-handed to repel all attacks.

At the time he published his celebrated work Vesalius was Professor of Anatomy at the University of Padua; and he was, it is said, the first anatomist who ever received a salary. He laid the foundation of the celebrity of Padua as a medical school, and for nearly two hundred years students resorted to it from all parts of the world. The old prejudices against dissection prevailed in France, Germany, and Britain; so that Italy was indeed the only country to which a student of medicine who wished to acquire a knowledge of the structure of the human frame could resort. Italy produced a series of great anatomists after the time of Vesalius; and even contemporary with him there were some professors in Italy who advanced the science by their discoveries.

Among the students at Padua at the opening of the seventeenth century was a young Englishman named WILLIAM HARVEY (1578—1657). The professor of anatomy at that time was a man of some celebrity, called FABRICIUS AB AQUAPENDENTE, who had, among other things, discovered the valves in the blood-vessels. It was, no doubt, the direction of his master's researches which turned the thoughts of Harvey in a similar course, and prepared the way for one of the greatest physiological discoveries ever made. It need hardly be said that this discovery was that of the Circulation of the Blood. Harvey became himself a demonstrator of anatomy in London, and taught his theory to his pupils as early as 1616; but it was not till 1628 that he published an account of his discovery in a tract entitled "*Exercitatio Anatomica de Motu Cordis et Sanguinis in Animalibus*" ("An Anatomical Dissertation on the Movements of the Heart and of the Blood in Animals"). As usual whenever any new discovery or doctrine is announced, this one was attacked by a host of antagonists, among the most strenuous of whom was Riolanus, then professor of anatomy at Paris. Harvey, however, successfully refuted in subsequent publications all the arguments brought against his doctrine, and many years before his death he had the satisfaction of seeing his discovery everywhere admitted and publicly explained. Harvey was the author of other valuable treatises scarcely less remarkable for solid reasoning and acute observation.

The circulation of the blood, as explained and demonstrated by Harvey, is one of those capital discoveries which completely change or vastly enlarge the conceptions that belong to a whole science. This discovery, which for the first time afforded the means for obtaining just views on the nature of physiological processes in general, may be regarded as the very foundation of modern physiology. What men before had learnt of the circulatory apparatus and its functions was but fragmentary or erroneous. Aristotle knew a little about the structure of the heart, and the Alexandrian physician Erasistratus (third century B.C.) had discovered the valves at the origin of the great vessels. But Erasistratus, finding no blood in the arteries of recently killed animals, concluded that it is the office of these vessels to receive air from the lungs, and distribute it through the body in order to purify the blood. Galen corrected this error by proving experimentally that during life the arteries are filled with blood, and that this blood is, like that which fills the left side of the heart, of a bright red colour. Galen recognized that the function of the lungs is to expose the blood to the action of the air, and he traced the course of the blood from the right side of the heart, through the pulmonary artery, through the lungs themselves, and back to the left side of the heart by the vessel now called the pulmonary vein. He supposed, however, that it was only a small portion of the blood which followed this course, and that the greater part passed immediately from the right to the left side through apertures in the partition between the two ventricles. Another of Galen's errors was to consider that all the veins of the body had their origin in the liver. The fact of the blood in the right side of the heart reaching the left side *only* by passing through the lungs was not established until the year A.D. 1559, and by a professor of Padua named REALDUS COLUMBUS. The pulmonary circulation was not, therefore, the discovery of Harvey, whose investigations, however, corroborated the views of the Paduan professor. The originality of Harvey's doctrine consisted in his maintaining that the blood passes from the left ventricle through the arteries to all parts of the body, and that, following the ramifications of these vessels, it passes in some way (which Harvey's means of demonstration did not enable him to trace) into the roots of the veins, then into the trunk veins, by which it is conveyed into the right side of the heart, whence it makes the circuit through the lungs to the left side, and so on. He thus established a truth previously wholly unsuspected, namely, that the same drop of blood may make the complete course through the pulmonary and the systemic circuits, and arriving at the point from which it set out, continue to *circulate* indefinitely.

The application of the microscope to the examination of the minute structures of animals and vegetables, and the discovery by its means of hitherto unknown forms of minute organisms, caused a rapid advance in the knowledge of living things. The first steps were made by Hooke, Leeuwenhoek, Grew, and Malpighi. The simple microscope or small single lens was probably known to the ancients; and, indeed, the way in which objects are magnified when viewed through a small globular glass vessel filled with water is distinctly referred to by a Greek writer. It is certainly impossible to fix upon any particular person as the inventor of the microscope; but the period at which it came into general use, and the persons who brought it under public notice, are more easy to ascertain. Microscopes were made in Holland at the end of the sixteenth century, and about the middle of the next century many remarkable discoveries were made by the Dutch naturalist LEEUWENHOEK (1632—1723), who first described many of the minute structures of animals and plants. Leeuwenhoek's work was all done with simple lenses of very small dimensions and high magnifying power. The little lens was mounted in an aperture made in a plate of metal, and there was no little inconvenience and difficulty attending the use of the instrument from the object requiring to be placed very near to the lens, which was itself close to the eye. Leeuwenhoek communicated his observations to the Royal Society of London, his name occurring for the first time in the Transactions for 1673. His communications were published in several separate volumes during his lifetime under the title "*Arcana Naturæ Detecta*," and he bequeathed to the Royal Society a collection of his microscopic preparations, each mounted with the lens for viewing it. The merit of Leeuwenhoek's work may be inferred from the remark of Dr. W. B. Carpenter, one of the most accomplished microscopists of the present day: "That with such imperfect instruments at his command, this accurate and painstaking observer should have seen *so much and so well* as to make it dangerous for any one, even now, to announce a discovery without having first consulted his works, in order to see whether some anticipation of it may not be found there, must ever remain a marvel to the microscopist."

In Italy, the professor of medicine at the University of Bologna, MARCELLO MALPIGHI (1628—1694), about the same time was employing the microscope in anatomical and physiological investigations. He confirmed Harvey's great discovery by direct observation of the circulation of the blood in a frog. That is, he was able to view the blood actually passing from the minute branches of the arteries to the minute roots of the veins; so that the theory of this passage which Harvey had advanced as a rational probability became now a demonstrated fact. Many important details of the structure of various organs were first made known by Malpighi. The microscope also enabled him to trace out for the first time something of the anatomy of insects. The silkworm, for example, was carefully observed by Malpighi: he discovered that the small spots which may be seen on each side of the creature are, in reality, carefully-guarded openings into a system of delicate air-tubes, which communicate with every part of the body. These air-tubes, or *tracheæ* as they are called, form a respiratory apparatus corresponding with the lungs of higher animals. Fig. 113 shows a view of the breathing aperture, or *spiracle*, of the silkworm as it appears when magnified under the microscope, and the small circle encloses the object as it appears in actual size.

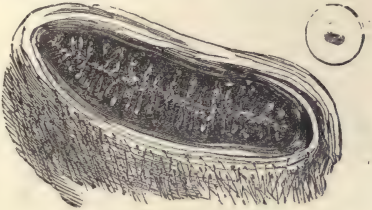

FIG. 113.—SPIRACLE OF SILKWORM.

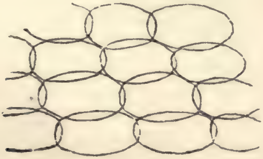

FIG. 114.

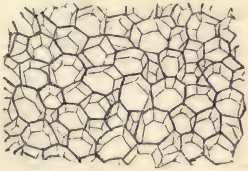

FIG. 115.

Vegetable anatomy next claimed Malpighi's attention for microscopic investigation. Unknown to him, DR. NEHEMIAH GREW (1628—1711) was also occupied in England with the same investigation, and in 1670 a memoir of Grew's on the subject was read before the Royal Society, and shortly afterwards another paper on the same subject was received from Malpighi. The two observers have agreed in most of the particulars they described, and in the physiological interpretations to be put upon the facts. The fact was stated by both of vegetable structures being composed of *cells* of various forms, modified from the simplest form of all, shown in Fig. 114, in which each cell consists of a little spherical or ovoid bladder of transparent membrane. When the cells are crowded together, as in Fig. 115, the intervening spaces are filled up, and the cells assume a polygonal form. In other circumstances the cells may become elongated, as in Fig. 116; and by the absorption of the transverse partitions, they do in many cases pass into the form of tubes, and thus form the well-known fibres and ducts

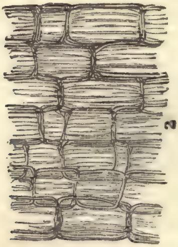

FIG. 116.

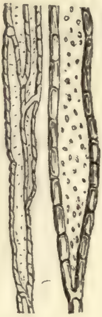

FIG. 117.

of vegetable tissues. In Fig. 117, which represents fibres from the root of the elder-tree, this formation from originally separate cells can be distinctly traced.

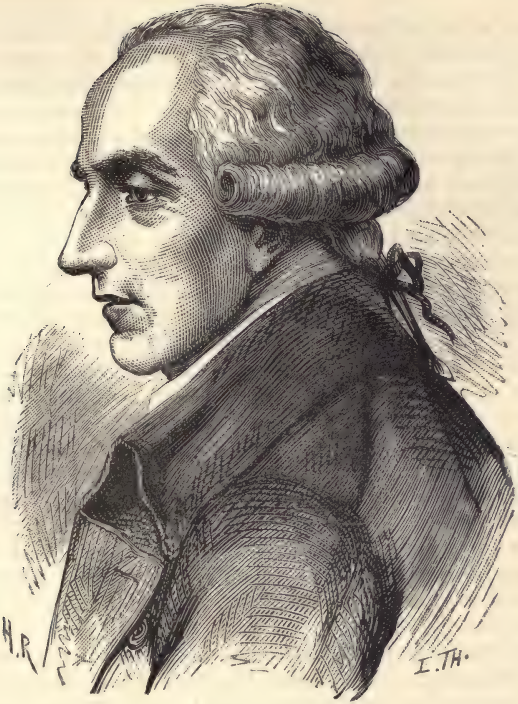

FIG. 118.—LAPLACE.

# CHAPTER XI.

## ASTRONOMY OF THE EIGHTEENTH CENTURY.

ASTRONOMY, by the dignity of its object and the perfection of its theories, is the finest monument of the human understanding, the most noble proof of its intelligence. Led away by the illusions of his senses and by his self-love, man long looked upon himself as the centre of the heavenly motions, and his vain pride was punished by the fears they caused him. At length centuries of investigation removed the veil that hid from his eyes the system of the world. He then found himself on a planet almost imperceptible in the solar system, whose vast extent is itself only an insensible point in the immensity of space. The sublime results to which this discovery has led him are well calculated to console him for the rank it assigns to the earth; for the smallness of the base which he has employed to measure the heavens shows him his own greatness. Let us carefully preserve, and let us increase the treasures of this lofty knowledge, the delight of thinking beings. It has rendered important service to navigation and geography; but its greatest service consists in having dissipated the fears produced by celestial phenomena, and in having destroyed the errors arising from ignorance of our true relation to nature—fears and errors which would soon repeople the gloom were the torch of science extinguished.

In some such words as the foregoing does one of the greatest astronomers of the eighteenth century vindicate the position of his science; and his expressions are fully justified by the grandeur of the results attained by astronomical science in the period upon which we are now to enter. Of all the sciences, astronomy is that which always has soonest and most deeply influenced man's conception of the universe and of his relation to it, and there is, therefore, a certain fitness in giving it the precedence. Its progress, however, has not been and can never be independent of other branches of knowledge. Two causes of the rapidity of this progress in the eighteenth century are obvious in—first, the great increase of the powers of mathematical analysis; and, second, the marked improvement in instruments of observation and measurement. Some of these improvements will be mentioned here as we proceed, and some discoveries which bear upon the construction of astronomical instruments will be named afterwards in connection with general physics.

The first datum for all astronomical calculation is a correct determination of the figure of the earth. We have already seen (p. 197) how Newton's grand idea of gravitation had to wait for its verification and development for a more correct estimate of the earth's magnitude, and had this not been forthcoming, he would probably have laid aside his speculation for ever. No sooner had Picard completed his measurement of a degree than the French Academy of Sciences resolved on extending the operation by measuring a prolongation of the arc in both directions. The measurements were commenced in the latter part of the seventeenth century, but it was not until 1718 that the operations were completed, after Picard, La Hire, Dominic and Jacques Cassini had taken part in them. The results indicated that a degree measures on the earth's surface a shorter distance as it is nearer the pole, and at first Cassini supposed that this was consistent with the assumed flattening of the earth at the poles. It was soon pointed out, however, that if these measures were correct, the earth must be *elongated* at the poles, contrary to the theory of gravitation. That the flattening of the earth's poles would cause the degrees to be *longer* towards the poles may be understood by referring to Fig. 119, where *b a* D A B represents a section of the earth though its axis (with the amount of the ellipticity much exaggerated). The part about the pole A B coincides in curvature with the circle E A B F, which has its centre at C. Similarly the curvature of the equatorial region *a b* coincides with that of the smaller circle *a b g*, which has its centre at *c*. It is obvious that any given angle in the circle E A B F must be subtended by a greater length of line A B at C than on the smaller at *a b* to *c*. Thus the angles A C B and *a c b* being each 10 degrees, the distance A B is longer than *a b* in the same proportion that A C is longer than *a c*. Cassini and his coadjutors would not admit that their determination could be inaccurate; and therefore, to settle the matter, France sent out two expeditions, one to measure a degree near the equator, the other to do the same in Lapland. The base measured in France by Picard was found to contain a considerable error, which vitiated the conclusions of Cassini. The final results fully confirmed the compression of the earth towards the poles, and from that time mathematicians and astronomers have all agreed in assigning to the earth the form of an oblate spheroid. Arcs of the meridian and degrees of parallels of latitude have since been measured in various other countries, as in 1768 by Beccaria in Piedmont, and by Liesganig in Hungary; in 1800 by Mudge in England; in 1801 by Suanberg in Lapland. These various determinations did not exhibit a satisfactory concordance in their results, so that the exact form and dimensions of the earth were ascertained only within certain limits. It may here be added, that, in more recent times, the pendulum has been used for determining the earth's ellipticity, and that arcs have been measured with all possible refinement of instrumental means. In order to complete all that will be said on this subject, the statement of the best recent results may be here laid before the reader. Airey calculates the length of the earth's polar diameter to be 7,899·170 miles, and Bessel makes it 7,899·114 miles. The former assigns for the equatorial diameter 7,925·648 miles, the latter 7,925·604 miles. Thus, if the earth be represented by a globe 3 feet in diameter, we must reduce the polar diameter by one-tenth of an inch to bring it to the true oblately spheroidal shape of the earth.

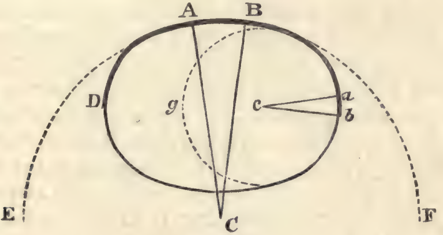

FIG. 119.

The attention of astronomers had been directed from the time of Copernicus to the parallax (page 215) of the fixed stars. It was known that this parallax, if it existed, must be very small; but it seemed reasonable to suppose that a line 180,000,000 miles long (for such is the diameter of the earth's orbit) would subtend some measurable angle to the distance of at least the nearest fixed stars. Though the absence of any annual parallax of the fixed stars was not allowed as a proof against the earth's annual motion, yet it was felt that if even the smallest amount of parallax could be detected, it would furnish an unanswerable argument in favour of the Copernican theory. Galileo had suggested a mode of observing whether some selected star always crossed the meridian at the same altitude, and a modification of his method consisted in permanently fixing a telescope on some immovable support in such a position that the passage of the star across a fixed triangular field may be noted. Should the altitude of the star vary, the interval of time required for it to pass across the triangle will of course vary also. It might be supposed that the effects producible by the annual parallax being perfectly understood, it would be an easy matter to distinguish them. But though the greatest attention has been given to this subject by a multitude of observers, the amount of the parallax is so small, that no satisfactory indications whatever were obtained by the earlier observers. There are, in fact, several other causes which give rise to apparent changes in the position of the fixed stars, and when several of these causes were as yet unknown, it was of course impossible to eliminate their effects from the observations. Hooke, Picard, Römer, Horrebow, and Cassini observed and announced the observation of what they took for the effects of parallax. It was in conducting observations to ascertain the parallax that a discovery very important for astronomy was made. Here it will perhaps be better to anticipate the chronological course of our history by stating what is known of stellar parallax at the present day. Only about a dozen stars are known to possess a sensible parallax. The parallax of the star α *Centauri* is about 0·9″, and this corresponds with a distance from the earth about 224,000 times the distance of the latter from the sun. Sirius, the brightest star in the heavens, has a parallax of fifteen hundredths of a second of arc, and that corresponds with a distance from us of 1,375,000 times our distance from the sun, while the star Capella is at four times the distance of Sirius. It is of course impossible to form any adequate conception of distances so vast, but the velocity of light furnishes a standard of comparison which it may be interesting to apply to this case. Light travels at the rate of 180,000 miles per second, and it takes about eight minutes to traverse the space which separates the earth from the sun. Thus the light from α *Centauri* travels for 3½ years before it reaches the earth, and that from Sirius reaches us 21½ years after it has been emitted, while the time occupied in the journey of the luminiferous impulses from Capella is actually the space of a human life, for the interval required is no less than seventy years. It is curious to reflect that we see the stars not by the light they are now giving off, but by the light they emitted scores of years before, and that were they all suddenly and simultaneously blotted out of existence, whole generations of men might afterwards live their term and pass to their graves without perceiving the difference.

In the year 1725 two English astronomers, MOLYNEUX and BRADLEY (1692—1762), began to observe a star in the constellation of the Dragon (γ *Draconis*), which star passed very nearly vertically over the Observatory at Kew. Observations made on the 3rd, 5th, 11th, and 12th of December, not having indicated any material difference in the place of the star, they were about to be discontinued for the time, when on the 17th Bradley, having from curiosity repeated his observation, perceived that the star passed rather more to the southward than before. This was at first attributed to the uncertainty or errors of observation; but another observation on the 17th showed the star passing still more to the southward. These appearances surprised the two astronomers, for they evidently could not be due to the annual parallax. It was first supposed that some change in the position of the instrument had occurred; but this idea was soon excluded by the observed regularity of the motion. At the beginning of March the star was found 20″ more southerly than at the time of the first observation; by the middle of April it began to return to the north; in September it began to return to the south; and in December, 1726, it occupied the same position as before, allowance being made for the precession of the equinoxes. Bradley at first supposed that a change of the position of the earth's axis would account for these appearances, but observations on other stars were found inconsistent with this supposition. He had a sector (that is, a portion of a graduated circle) erected, having a radius of 12½ feet, and an arc ranging over 6° on each side of the zenith, or vertical point. He devoted another year of obser-

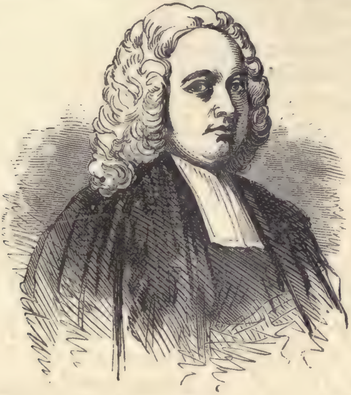

FIG. 120.—BRADLEY.

vation to γ *Draconis* and other stars, and after several conjectures were found inadequate to explain the appearances observed, the happy idea occurred to him that all the phenomena proceeded from the motion of light combined with the orbital motion of the earth. A familiar illustration may convey to the reader's mind a just idea of the nature of the fact discovered by Bradley. Refer to Fig. 121, and let us imagine a shower of rain descending vertically; but out of the multitude of drops we will consider only the course of one, whose line of fall shall be represented by the vertical line C 1 2 3. Now, suppose we have an open tube, it would be quite easy to make the rain-drop fall down the axis of the tube by simply holding it stationary in an *upright* position. But now imagine the tube to be moving from left to right at the same time that the drop is falling within it. It is plain that the drops could not possibly pass along the axis of the tube if the latter be maintained in a vertical position while it is moved. But it remains for the reader to realize the fact that by giving the tube a certain inclination forward, and it would be possible for the rain-drop to be always vertically falling, yet never quit the axis of our moving tube. In the figure the tube and the rain-drop are represented at three instants of time. A very familiar experience will suggest the necessity for the inclination of the tube in the case supposed. When a person is standing still in a shower of rain descending vertically, an umbrella held upright effectually screens him; but if he walks forward he finds it necessary to incline his umbrella in the direction he is moving, and the faster he walks the greater is the inclination that must be given to the umbrella to prevent the rain from reaching his person. Again referring to Fig. 121, it will be seen that in the time during which the rain-drop is falling from 1 to 3 the tube moves forwards (we suppose both motions to be uniform) by the distance A1, A3; hence the velocity of the tube is to the velocity of the drop as the line A1, A3 is to line 1, 3. Now, instead of rain, imagine that we have *light* falling vertically, that the tube is the tube of a telescope, and that it is carried forwards by the movement of the earth in its orbit. It will now be obvious to the reader that the telescope which receives light really streaming vertically downwards must be directed, not to the zenith, but to a point at a certain angular distance in advance; and that a star which was truly in zenith C would appear to an observer to be in the direction D. A little reflection will show that

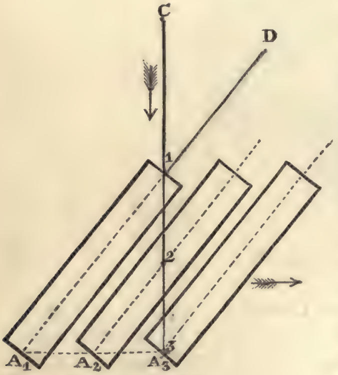

FIG. 121.

this effect is in no way dependent on the use of a telescope, which we have supposed only in order to make plainer the first idea of the aberration of light. It is, in fact, simply the result of the composition of two motions. Looking again at the figure, it is plain that we have $\frac{\textit{Velocity of Earth}}{\textit{Velocity of Light}} = \frac{\textit{Line}\,A_1, A_3}{\textit{Line}\,1, 3}$; but the ratio of these two lines is the tangent of angle C, 1, D (p. 61), for angle C, 1, D = angle $A_1$, 1, 3.

The angle of aberration was estimated by Bradley to be 21·2″, but subsequent astronomers have estimated it at very nearly 20·5″. This discovery is not only one of great importance in practical astronomy, but, in the absence of any observed sensible parallax of the stars, it amounted to a direct proof of the earth's motion, which established the Copernican theory beyond any possibility of doubt, and at the same time confirmed Römer's estimate of the velocity of light. This discovery ranks next in importance only to the laws of Kepler and the precession of the equinoxes. But Bradley, continuing his observations with the sector, discovered an apparent motion of the stars, which he traced to an oscillation of the earth's axis caused by the attraction of the moon on the earth's equatorial protuberance, just as the precession of the equinoxes discovered by Hipparchus is caused by the attraction between the same mass of matter and the sun. Hence there is a certain propriety in the title of the "English Hipparchus" which has been bestowed upon Bradley. This motion of the axis was announced by Bradley in 1745, and is known as the *nutation* of the earth's axis.

The middle of the eighteenth century is an important period in the history of astronomy on account of the great improvements in the arts of observing which were then introduced by Bradley, La Caille, Mayer, and others. It was now that the *transit instrument* began to be extensively employed, and the *mural circle*, now fitted with a suitable telescope and graduated with great precision, was rendered capable of giving very accurate results. The *mural circle* is so called because it is attached very solidly to a wall, and is fixed in the plane of the meridian. From this period dates the formation of trustworthy catalogues of the fixed stars and other celestial objects. Bradley, when he was appointed Astronomer Royal, had a new transit instrument erected, and also a mural circle of 8 feet radius. With these instruments and an excellent clock, Bradley made, from 1750 to 1762, one of the largest and most valuable series of observations which have ever been the work of a single astronomer. La Caille was a French astronomer, who went to the Cape of Good Hope to form a more complete catalogue of the stars of the southern hemisphere than had been made by Halley. La Caille in ten months observed the position of ten thousand stars. On returning to Paris he engaged in researches on atmospheric refraction and other subjects. Tobias Mayer also investigated the laws of refraction, and published tables for the calculation of the positions of the sun and the moon.

The problem of determining the longitude of a ship at sea is one of the greatest importance to navigation. It resolves itself into observing some astronomical occurrence, the time at which this occurrence is observed being counted according to the time of noon at the place; and this is compared with the hour at which the event takes place according to the time at some given place,—as Greenwich, for example. All the practical methods of finding the longitude at sea depend either on carrying the time from the first meridian, or on observations on the position of the moon. If the observer can find the *true place* of the moon's centre, that is, its apparent position when viewed from the earth's centre, he may calculate from tables what is the time at the first meridian at that instant. The method of finding the longitude by observation of the moon's place had occurred to several astronomers as early as the sixteenth century; but without good tables of the moon's distances, and good means of observation, the idea was of no available utility. And these difficulties were not surmounted till the middle of the eighteenth century. At that time astronomy had made such progress by improvements of the theory of the moon's motions, and by the accumulation of correct observations, that Mayer, who was excellent at once as a mathematician and an astronomer, was enabled to draw up tables sufficiently accurate for determinations of the longitude at sea. For this service Mayer's widow received from the British Government the reward of £3,000, a sum which does not appear excessive when compared with the importance of the results the tables were capable of furnishing. These tables, however, removed only one part of the difficulty: there still remained that of observation. Several of the methods proposed were too diffi-

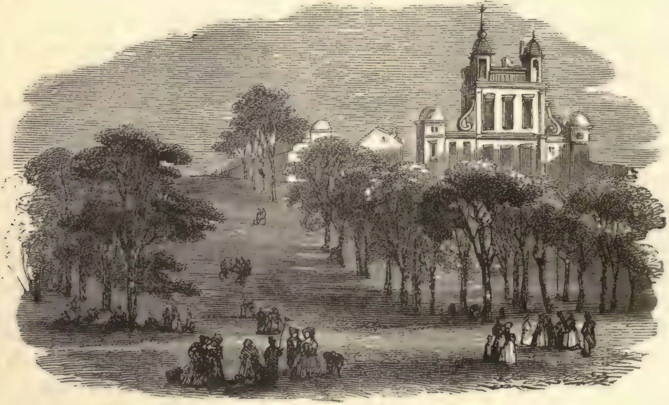

FIG. 122.—GREENWICH OBSERVATORY.

cult on account of the movement of a vessel, and others required the observations to be reduced by too many corrections. Hadley's quadrant was, however, an invention which allowed the observations to be accurately and easily made, and La Caille, Mayer, and Maskelyne advocated the adoption of this method of observation in preference to all others. And to obviate the necessity of laborious calculation by the navigator, the British Board of Longitude undertook to publish annually a Nautical Almanack, in which the distances of the moon from certain stars, calculated for every three hours, were set down for three years in advance.

As it was the invention of reflecting instruments which made this method of finding the longitude possible, it may be interesting to examine the construction of Hadley's instrument. The diagram in Fig. 123 may be first referred to as showing the principle of the instrument without the details. A B is a graduated arc, the centre of which is at C. This arc may be one-quarter of the circumference, when the instrument properly takes the name of *quadrant;* or it may be one-eighth of the circumference, in which form it is called the *octant;* or it may be (and this is the most usual form) one-sixth of the circle, and it is then called the *sextant.* The close parallel lines at C represent a flat mirror attached to and turning with the arm C B, movable about C, the centre. In the figure we see only the edge of the mirror, and the same at L, where the short sloping line represents another mirror also perpendicular to the plane of the arc, and permanently fixed to the frame of the instrument. It is in a peculiarity of this mirror that the special power of the instrument resides. It is made of glass, and only the half of it is silvered, the upper half being left so that it does not interrupt the view of objects to which the telescope *f* is directed. This telescope is fixed to the frame of the instrument, and does not move with the arm C B. The silvered half of the mirror L reflects to the eye of the observer at *f* rays which first fall upon the mirror C. The mirror L is so fixed that it is parallel to mirror C when the index of the movable arm C B points to 0°. This is accomplished by first fixing the movable arm at 0°, and then so adjusting the mirror L, that when the sea-horizon is seen through the telescope, the direct view H L *f*, and the reflected view, due to rays taking the path K C L *f*, shall coincide. Now, suppose it is required to measure the altitude of a star above the horizon: the instrument is held so that

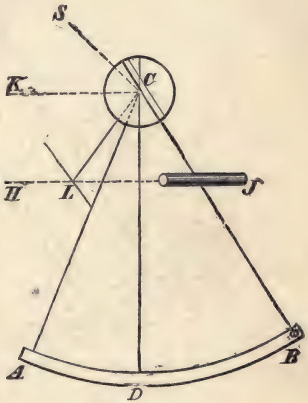

FIG. 123.

the horizon remains in view through the unsilvered part of L, and the movable arm, carrying with it the mirror C, is turned until the image of the star appears to coincide with the horizon. Thus C D may represent the new position; the rays from the star will be reflected along the path S C L *f*, and S will appear to the observer to coincide with H. It may easily be shown that the angle B C D must be exactly half of the angle K C S; and the graduations on the limb A B being arranged accordingly, we have only to read off the angle B C D. The great advantage of the instrument is that both the objects whose angular distance is to be taken are visible at the same instant, and that the instrument requires no levelling, etc., being simply held in the hand. Fig. 124 exhibits the actual construction of the sextant. C and L are the mirrors as before, C E is the movable arm provided with a vernier (page 212), and with a lens G, for reading off the small divisions of the scale. J K is the telescope, O is a handle for holding the instrument, C is a milled head for accurately adjusting the position of the movable arm. M and N are screens of coloured glasses, which can be turned in the path of the rays of the sun when he is the object of observation.

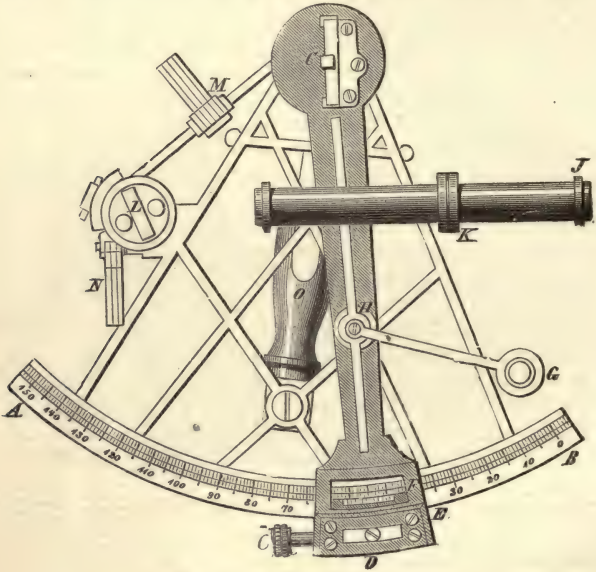

FIG. 124.—HADLEY'S SEXTANT.

The use of the instrument we have just described furnishes a striking illustration of the powers of astronomical calculation, and of the certainty of those principles upon which they rest. "A page of lunar distances from the Nautical Almanack," says Sir John Herschel, "is worth all the eclipses that have ever happened for inspiring confidence in the conclusions of science. That a man by merely measuring the moon's apparent distance from a star with a little portable instrument held in his hand and applied to his eye, even with so unstable a footing as the deck of a ship, shall say positively, within five miles, where he is on a boundless ocean, cannot but appear to persons ignorant of physical astronomy an approach to the miraculous. Yet the alternative of wealth and ruin, of life and death, are constantly staked upon the accuracy of these computations, which might almost seem to have been devised on purpose to show how closely the extremes of speculative refinement and practical utility can be brought to approximate." He then illustrates these remarks by some particulars relating to a voyage made by Captain Basil Hall, R.N. This officer had sailed from San Blas, on the west coast of Mexico, and, after a voyage of 8,000 miles, occupying eighty-nine days, arrived off Rio de Janeiro, having in this interval passed through the Pacific Ocean, rounded Cape Horn, and crossed the South Atlantic without making any land, or even seeing a single ship, except an American whaler off Cape Horn. Arrived within a week's sail of Rio, he set about determining by lunar observations the precise line of the ship's course and its situation at the moment; and having done this, he continued his voyage by the ready methods which are employed in short trips, but which cannot be trusted in long voyages, when the moon is the only sure guide. Captain Hall thus relates the rest: "We steered towards Rio de Janeiro for some days after taking the lunars, and having arrived within fifteen or twenty miles of the coast, I hove-to at four in the morning till the day should break, and then bore up; for, although it was very hazy, we could see before us a couple of miles or so. About eight o'clock it became so foggy that I did not like to stand in farther, and was just bringing the ship to the wind again, before sending the people to breakfast, when it suddenly cleared off, and I had the satisfaction of seeing the great Sugar-Loaf Rock, which stands on one side of the harbour's mouth, so nearly right ahead that we had not to alter our course above a point in order to hit the entrance of Rio. This was the first land we had seen for three months, after crossing so many seas, and being set backwards and forwards by innumerable currents and foul winds."

The method of determining the longitude by the transport of time is one which depends entirely upon the excellence of the timekeepers used. Huyghens had no sooner applied the pendulum to clocks than he endeavoured to make these useful to navigators. The difficulties presented by the violent and irregular movements of ships presented insurmountable difficulties; but his discovery of the isochronism of a steel spring applied to a balance-wheel was, as already mentioned, the first step towards the chronometer.

In the year 1714 the British Parliament passed an Act offering a very large reward to any one who should discover a method of finding the longitude at sea: £10,000 was the reward if the longitude were found within a degree; £15,000 if within forty minutes; and £20,000 if within half a degree. These munificent rewards had the desired effect. JOHN HARRISON, a man of humble origin but great genius, had devoted his lifetime to the construction of clocks and watches for navigators. The improvements he made in the marine chronometer were so numerous and important that we may consider him the inventor of the instrument. For these improvements he obtained a medal from the Royal Society in 1749; and in 1761 the Board of Longitude permitted a trial of his chronometer to be made by a voyage to Jamaica. At the end of sixty-one days Harrison's chronometer gave the longitude of Port Royal within 5″ of time; and, on the return to England, after an absence of a hundred and sixty-one days, the whole variation was only 1′ 5″. The condition of the Act of Parliament had been satisfied, and Harrison received within two or three years the whole reward of £20,000. From this time the use of chronometers spread rapidly among navigators; but the sextant has also always maintained its position as the essential instrument for the seafarer. The advantage is obvious of having two independent methods for the longitude, one of which shall act as a check upon any accidental errors of the other.

In 1736 Bouguer and La Condamine endeavoured to measure the gravitative attraction of the mountain Chimborazo in Peru. In a station north of the mountain its attraction causes the plumb-line of the sector to deviate from the true vertical towards the south, and the converse is true on the south side. Hence the apparent latitudes found at each station differ from the real, and the difference between these latitudes will be the sum of the attractions of the mountain at the two stations. If, therefore, the true latitudes are previously determined by other methods, such as trigonometrical surveys, it is easy to find the attraction of the mountain by its perturbing effect on the plumb-line. Bouguer did not, however, obtain very satisfactory results; but he, indeed, did not operate under the conditions just stated, for both of his stations were to the south of the mountain, and hence it was not the sum, but the difference, of the attractions which was observed. The experiment of observing the actual deflection of a plumb-line produced by a neighbouring mountain was repeated under more favourable conditions by Maskelyne, in 1774, on the mountain of Schehallien in Perthshire. This mountain rises 2,000 feet above the sea-level, and it was easy to select one station for observation to the south of the mountain and another to the north. In this case it was found, after repeated observations, that the sum of the attractions of the mountain at the two stations amounted to 11·7″. The mean density of Schehallien was then determined by carefully finding the density (*i.e.*, specific gravity) of its constituents, and estimating approximately the quantity of each contained in the mountain. From the data thus obtained, the density of the earth was calculated by Dr. Hutton, and in 1775 he published in the "Philosophical Transactions" an account of the methods he had followed, with his conclusion that the mean density of the earth was to that of Schehallien as 9 to 5. And as the density of this mountain had been calculated to be 2·5, the resulting density of the earth was 4·48. That is as much as to say, that the earth would weigh four and a half times as much as a globe of water of the same size. Since that period other surveys of Schehallien have been made, and a higher density has been assigned to the mountain, the result of which is to raise the mean density of the earth to nearly 5, which agrees well with determinations made by other methods, one of which we shall now describe, as the experiments are extremely interesting.

The Rev. John Michell, about 1790, devised a method of directly observing the attraction of gravitation, and of measuring the mean density of the earth. The apparatus consisted of a light wooden rod suspended at its centre by a very fine long wire, so that the rod, bearing a leaden ball at each end, remained horizontal. When the rod had come to rest, the wire would be without torsion, and in this condition of affairs a large spherical mass of lead was brought near each of the balls, so that the attraction might conspire to turn the rod horizontally, thereby to a certain extent twisting the wire. The mechanical force corresponding with each degree of torsion could be easily ascertained, and thence the deviation of the rod became the measure of the attractive action exercised by the masses of metal placed near the leaden balls. This attractive force could therefore be compared with that of the earth, and thus the mass and consequently the density of the latter could be determined. The Hon. Henry Cavendish, in 1798, communicated a paper to the Royal Society, in which he describes the experiments he had made with Michell's apparatus. The mean result of his experiments gives the earth the density 5·45. Cavendish, however, observed certain anomalies in the indications afforded by the apparatus, and he therefore did not put forward his result as entitled to full confidence. He appears to have entertained the design of examining into the cause of the anomalies. Cavendish's experiments have been repeated during the present century with great care and with many ingenious improvements in the details of the apparatus and the modes of observing the deflections. The cause of the anomalies, which were again observed by the more recent experimenters, was found to be connected with the radiation of heat, as indeed Cavendish himself had supposed. The value for the earth's density, as determined in 1837 in this way by Reich, a professor at Freiberg in Saxony, is 5·44; and, as determined by Mr. Baily in 1842, it is 5·66.

Since Galileo there has been no name in the annals of astronomical discovery more illustrious than that of WILLIAM HERSCHEL (1738—1822). He was born in Hanover in 1738, and was the son of a musician in somewhat narrow circumstances. At the age of fourteen he had qualified himself for a position in the band of the Hanoverian Guards, and, as a musician, accompanied the corps to England in 1757 or 1759. He wished to try his fortune in London, and for a long time he had there many difficulties to contend with. Afterwards he passed several years in giving music lessons in different towns in the north of England. In 1765 he obtained a situation as organist at Halifax, and the following year was appointed organist of the Octagon Chapel in Bath. The position was one which afforded opportunities of additional emoluments, and had Herschel been intent only upon deriving a good income from his profession, he had now an assured prospect of doing so. But while yet a teacher of music in a northern English town, Herschel had devoted his leisure not only to making himself perfect in English, but also to the acquisition of Italian, Latin, and Greek. When he applied himself to the study of a learned treatise on Harmony, he found that before he could follow the reasonings he must become a mathematician. With characteristic energy he resolutely turned to this new study, and soon became so absorbed in it, that almost every other pursuit of his leisure hours was laid aside; and although his engagements as a professor of music greatly increased in Bath, the time he there devoted to mathematical studies was rather increased than diminished. After fifteen or sixteen hours of toil with pupils in music, he would on retiring home resort to mathematics for relaxation. In course of time he found himself competent to pursue the study of all those branches of science which are largely composed of mathematical deductions, and of these astronomy and optics seem to have especially arrested his attention. About 1772 Herschel began to make lenses, and soon afterwards proceeded to construct telescopes, and he suc-

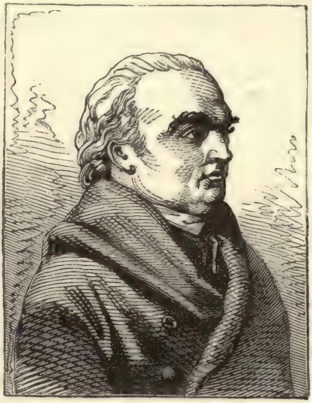

FIG. 125.—WILLIAM HERSCHEL.

ceeded in producing instruments far superior to any that had yet been made. In the year 1774 he finished a reflecting telescope made after Newton's plan (page 217), and this achievement was but the first of a long series of brilliant successes. The focal length of his first telescope was 5 feet, and then succeeded others of 7, 10, 20, and at length his great telescope of 40 feet focal length was erected at Slough, near Windsor. When Herschel had entered upon his career of practical optician and astronomical observer, he became so engrossed with his scientific pursuits that, regardless of the loss of income, he began to limit his musical engagements and the number of his pupils. In forming the mirrors for the successively larger and larger telescopes, his perseverance and labour were almost incredible. He soon found that it was quite impossible to give the true figure to the mirrors by any mechanical means when their focal length was considerable. The exact form for a mirror of upwards of 6 feet focal length can only be finally given by delicate touches with the hand after repeated trials. With all the care and skill given by practice, the chances are very great against the correct form being given at any one operation. It was Herschel's practice to cast three metallic mirrors for a telescope, and, having worked at all three, to place the best one of them in the telescope, and use it for observations while he continued to work at the others successively, again leaving the best in the telescope, and so on. Thus for each telescope very many mirrors were examined before the finally perfect one could be arrived at. In giving the final polish to a mirror, a single rub wrongly applied spoils the shape, and the difference is distinctly perceptible to trained eyes when certain objects are viewed in the telescope. Herschel would work at his mirrors continuously for twelve or fourteen hours without quitting his occupation for a moment. So much was he convinced that "when his hand was in" any interruption would spoil his labour, that he would not quit it to help himself to food, and the little he ate on these occasions was put into his mouth by his sister. It is said that Herschel himself ground 200 mirrors of 7 feet focal distance, 150 of 10 feet, and 80 of 20 feet.

Herschel was still residing at Bath, and dependent for the means of subsistence on his profession as a musician, when he completed his 7-feet reflector. He had brought his mirror (made of an alloy of tin with copper) to a degree of perfection hitherto supposed impossible, and carried the magnifying power of his eye-pieces to a range which had been assumed to be altogether beyond the faculty of the eye to bear. The ingenious fabricator found means of making this and his subsequently still larger instruments so convenient in use, that he had no more difficulty in observing with them than an ordinary person finds in employing a spying-glass two or three feet long. His eagerness to observe the heavens was so great, that for several years he was never in bed while the stars were visible. Winter and summer he passed nearly every clear night in the open air observing the stars, and his robust health enabled him to continue a practice which would have been trying to many constitutions. In this way he began a systematic survey of the heavens, and did not fail to be rewarded by grand discoveries, which shall presently be mentioned.

On the 13th of March, 1781, Herschel observed a small star in the constellation *Gemini*, because this star appeared to have a steady light, and even to have a sensible diameter, in this last particular differing notably from the fixed stars, to which no magnifying power whatever can give the appearance of anything but mere *points* of light. The position of the star among the rest was noted, and after some hours another observation appeared to indicate a slight change in the place. The next night's observations placed this change beyond a doubt, and Herschel announced to Maskelyne, the Astronomer Royal, that he had discovered a comet. Maskelyne confirmed Herschel's conclusion, and sent word to the French astronomers of the position of the supposed comet. After the new star had been observed for some time, attempts were made to calculate its orbit, on the supposition that the orbit was parabolic; but it was soon perceived that the real orbit was nearly circular, and that the object was a planet, and not a comet. It will be an excellent illustration of the utility of catalogues of the stars, such as those made by Flamsteed and by Meyer, to point out that this planet had been observed by the former as a star (No. 34) in *Taurus*, and also by the latter, in whose catalogue it appears as No. 964. As positions of the planet at given moments, included within about a century of observation, were thus accurately known to astronomers, they were at once able to calculate the "elements" of the new planet with great precision. Herschel proposed to give to the new member of the solar system the name of *Georgium Sidus* (the Georgian Star) in honour of George III.; but this terminology did not find much favour in the scientific world, and a French astronomer suggested that the new planet should be known by the name of its discoverer. Both names were superseded by a more appropriate appellation given to the planet in Germany, where the old plan of assigning a name from classical mythology was adhered to, and the name of the most ancient of the gods, *Uranus*, was bestowed on the outermost (as then supposed) body in our system. The distance of Uranus from the sun was found to be 180,000,000 miles, and the period required to complete one orbital revolution eighty-three of our years.

The genius of the Bath musician, who was now reading mighty secrets in the stars, became fully known by this discovery; and as his powers were obviously fettered by the conditions under which he was working, the King (George III.), with commendable liberality set Herschel free to follow the bent of his genius, by bestowing upon him in 1782 a pension of £300 a year, whereupon he took up his residence near Windsor, where he constructed a splendid 20-feet telescope in 1783, and for two years continued to employ it in a series of admirable observations. At the end of 1785 the construction of a telescope of 30 feet focal length was entered upon; but the King afterwards induced Herschel to extend his design to one of 40 feet, which was erected at Slough, and used for the first time in February, 1787; but it was not until the 27th of August, 1789, that Herschel had completely satisfied himself of its perfect completion. On the following day he discovered the sixth satellite of Saturn by its means. The speculum in this telescope was 4 feet in diameter and 3½ inches thick, and weighed 2,118 lbs. The metal in which it was cast was an alloy of one hundred parts of tin to three of copper. Its tube was 4 feet 10 inches in diameter, made of sheet iron, and was supported by a massive framework of wood, the dimensions and machinery of which may be inferred from the fact that the construction employed thirty men for six months. Herschel in his large telescope suppressed the second small mirror used in the Newtonian reflector (p. 217), and received the rays reflected from the speculum at once into the eye-piece placed at one side of the mouth of the tube. The great advantage of this arrangement consisted in the increased distinctness and brilliancy of the image, as a second reflector necessarily entails some loss of light and of definition.

The general method of mounting the great Slough telescope will be understood from Fig. 126, page 262, which represents, however, another gigantic telescope erected by Mr. Ramage, of Aberdeen, in the beginning of the present century.

With the close cluster of stars called the Pleiades the reader is no doubt familiar. By the naked eye few persons can see more than six stars in this group, but when it is viewed under very favourable circumstances even by the naked eye, or still better by an opera-glass or common telescope, it is seen to consist of many more stars. A good astronomical telescope reveals fifty or sixty, and some observers have counted double that number. To persons of weak eyesight the Pleiades present the appearance of a light cloud; and there is in the constellation *Cancer*, an object (*Præsepe*, or the "Beehive") which to the best unassisted eyesight also presents the appearance of a luminous cloud, but which a very ordinary telescope resolves into a cluster of stars. Of these Galileo counted twenty-eight in the *nebula* here referred to. Every person who has seen the skies of the Southern Hemisphere has probably noticed the two luminous patches called the *Magellanic clouds*. Several other similar *nebulæ* were observed and catalogued by Ptolemy and other astronomers, and La Caille and others added to the list of nebulæ belonging to the Southern Hemisphere. But Herschel immensely increased the numbers, for in 1786 and 1789 he published two catalogues of them, giving an account of no fewer than 2,300. Nor was this all, for Herschel's telescopes resolved very

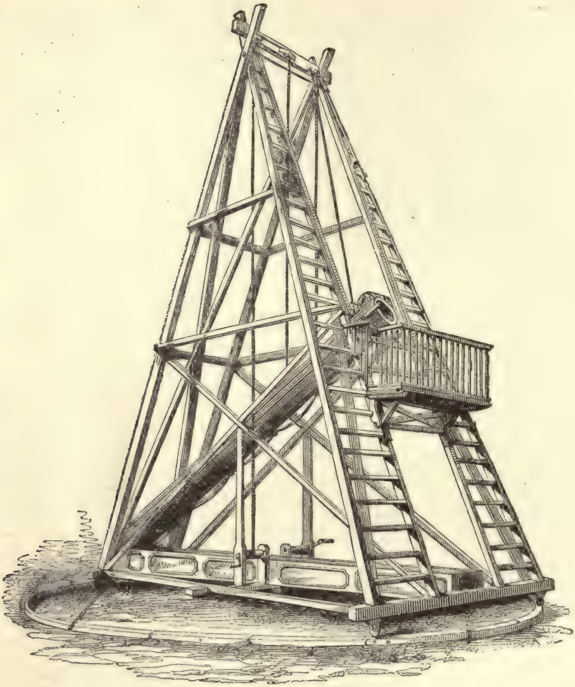

FIG. 126—RAMAGE'S TELESCOPE.

many of these luminous spots into clusters of stars. The forms of these clusters are extremely varied. Some appear more or less globular, others have elongated and very irregular forms. There are, however, a very great number of nebulæ which Herschel's telescopes were unable to resolve into stars; and although a few were subsequently resolved by the gigantic telescope of Lord Rosse, the greater number of the nebulæ retain a nebulous appearance under the highest powers of the telescopes which have yet been brought to bear upon them. Their appearance resembles that of comets in suggesting merely a thin luminous fog.

Of all nebulæ, however, the *Milky Way* is the one which forces itself upon the attention of the most careless spectator, and it has not failed to attract the notice of philosophers from the earliest ages. It has received many names: the Greeks called it the Galaxy, and also the Milky Circle, and by the latter name it was also known to the Romans. Ovid calls it the Milky Way when poetically representing it as the highway of the gods:—

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> Est via sublimis, cœlo manifesta sereno,
> Lactea nomen habet; candore notabilis ipso.
> Hâc iter est superis ad magni tecta Tonantis
> Regalemque domum.

Our English ancestors gave it such fanciful names as *Jacob's Ladder*, *The Way to St. James's*, etc. Aristotle supposed it to be caused by exhalations set on fire, and Theophrastus imagined it to be the course of the conjunction of the two hemispheres. Democritus and Pythagoras anticipated Herschel's discovery when they declared the Milky Way to be nothing but a vast assemblage of stars. Such was indeed the spectacle that the telescope at Slough revealed. It would be impossible to give any adequate idea of the vast numbers of stars in the Milky Way. Herschel calculated that in forty minutes of time 258,000 stars passed across the field of his telescope. Herschel's theory of the Milky Way was that the stars are so placed in space that they form a stratum, so to speak; that is, that vast as may be numbers of stars gauged in one direction (which may be called the thickness of the layer), these are inconsiderable compared with the length and breadth. He considered that our system may be placed towards the centre of the stratum, and not far from a place where the stratum separates into two layers, producing that cleft in the Galaxy which extends from *Cygnus* far into the Southern Hemisphere.

When Herschel was in possession of his first telescope, he began to systematically make observations of all the stars visible in his horizon. This led to a field of discovery almost new—namely, a multitude of stars, which up to that time had been considered single, were found to be really two, three, or four very closely grouped, and usually of different colours. In 1782 he gave a catalogue of 269 such stars, including all particulars as to the size, colour, and position of the components. These compound stars he arranged in classes according to the angular distance between the components, ranging from 1″ or 2″ to 120″. Before Herschel commenced his observations, only four such stars were known, but he observed and described nearly five hundred, and subsequent observers have so increased the number that the catalogues of multiple stars now include nearly six thousand of these objects. Herschel at first supposed that multiple stars were the results of two or more stars lying in the line of vision, and it was chiefly with a hope of determining their parallax that he undertook to form his catalogue. But, instead of finding any annual fluctuation, he observed a regular progressive change, in some cases of the angular distance, in others of the position; so that a real motion of the stars was indicated, or a movement of translation through space of the solar system as a whole. Finally, Herschel announced the existence of several systems of double stars, each component of which revolves in an elliptical orbit about a common point. To such stars he proposed to give the name of *binary*, to distinguish them from stars which appeared double only being nearly in the same line of sight; that is, stars physically double were to be by this term distinguished from those which were merely optically double. After observations extending over twenty-five years, Herschel was able to enumerate fifty instances of binary stars; but more than ten times that number have since been recognized.

The name of "fixed stars" has been found to well express the difference between these bodies on the one hand, and planets and comets on the other; but, besides those movements of *binary* stars just named, it was found by Halley, in 1717, by comparing the anciently recorded positions of the three conspicuous stars, Sirius, Arcturus, and Aldebaran, with his own observations, that after all corrections, there remained certain discrepancies, the apparent changes in latitude amounting to 37′, 42′, and 33′ respectively. W. Herschel's observations placed the existence of proper motions of the fixed stars beyond a doubt, and from a comparison of the various cases, he was led to infer that the solar system was moving towards a point in the constellation *Hercules*, and his conclusion has been since confirmed from results obtained by other observers.

The foregoing paragraphs can give but a feeble idea of the extent of Herschel's labours, and of the striking originality of his speculations. Of the life of this great astronomer we have but few particulars to add. He received the honour of knighthood from the Prince Regent in 1816, and the University of Oxford, by no means hasty in recognizing the merits of the greatest astronomical observer of his age, conferred upon him an honorary degree a few years before his death. Sir W. Herschel lived till 1822, and died full of years and honours on the 23rd of August, when he had nearly completed his eighty-fourth year, leaving a son, Sir John Herschel, whose career, as will be seen, worthily maintained the name of Herschel in the annals of science.

The priority of invention of the differential calculus was, as we have indicated, disputed between Newton and Leibnitz, and the claims of each were hotly maintained by their respective partizans. The bitterness with which this controversy was carried on ultimately caused an almost complete cessation of scientific intercourse between the British and the Continental mathematicians. Each party followed the steps of its own great leader, regardless of the development of the rival system. The British mathematicians, in particular, were completely ignorant of the improvements which their Continental brethren rapidly effected in the *differential calculus*, and adhered rigidly to the very letter of the methods of *fluxions.* They seemed to have been so dazzled by the splendour of Newton's discoveries, that they could not imagine that his methods might be capable of extension and improvement. Adhering to the idea and notation of *fluxions* exactly as Newton had delivered them, they were able to apply the new calculus to very few problems beyond those which obviously admitted of such application. The Continental mathematicians, on the other hand, found the fundamental notion of Leibnitz lend itself more easily to new developments, and his notation also to be a more manageable and effective instrument than that proposed by the great English geometer.

The English mathematicians of the period succeeding Newton were men of conspicuous talents, but their labours were directed to extending the bounds of the ancient geometry by its own rigorous methods, rather than to extensions of the new calculus, which was giving men the means of investigating the universe of things around them. The names of several of these English mathematicians will be remembered, indeed, as contributors of some improvements in algebra and in geometry, and as those to whom are due the best editions of the works of the ancient geometers. The names of GREGORY (1661—1708), WARING (1736—1798), EMERSON (1701—1782), T. SIMPSON (1710—1761), and R. SIMSON (1687—1768), may be mentioned here as illustrating these remarks. ROGER COTES (1683—1716), who was professor of experimental philosophy at Cambridge, gave, however, several improvements to the rules for the inverse or integral calculus, and some ingenious methods for areas of curved figures.

DR. BROOK TAYLOR (1685—1731) published in 1715 his "Method of Increments," a work which added greatly to the resources of the new calculus, and exhibited many ingenious applications of it to physical as well as to mathematical questions. Dr. Taylor's name will always be associated with a particular theorem, in which he shows that the value of any function whatever of a variable can be expressed by a *series* of terms formed according to a certain law. Some writers on the differential calculus have made this theorem the foundation of the whole science.

The validity of the reasoning on which the differential calculus reposed was called in question by Bishop Berkeley, an Irish prelate who is well known for his extreme "idealism" in metaphysics, which he carried so far as to refuse to acknowledge the existence of an "object world" at all. In a tract called "The Minute Philosopher," published in 1734, and again in a later one called "The Analyst," he contends that it is unmeaning and absurd to suppose that any finite ratio can exist between two infinitely small quantities, which he humorously designates as "the ghosts of departed quantities." The Bishop considers that geometry is opposed to religion, and that by a perverse contradiction of mind the geometers are intractable, and, unbelieving in religious matters, they believe in the mysteries of *fluxions*. He declares the new calculus is erroneous and obscure in its principles, and that, if geometrical truths are deducible by its principles it is because one error in these compensates for another; and, finally, he is of opinion that Newton himself did not understand it. Berkeley's attack did good by showing that the *language* of the calculus laid it open to objections of the kind he urged. Replies to these objections were published by two Cambridge professors and by others, notably by MACLAURIN (1698—1746), who published a treatise on *Fluxions*, in which the demonstrations included no supposition of infinitely small quantities. D'Alembert showed the real application of the principle of *limits;* and, finally, Lagrange was able to discard *limits* also, by reducing the whole theory of the differential calculus into an elementary, but protracted, algebraical investigation by developing functions in *series.*

It would be impossible, within our limits, to convey an idea of the great improvements which were effected during the eighteenth century in every branch of mathematical analysis. The theory of algebraical equations received great development by the labours of several eminent mathematicians, more particularly of the indefatigable EULER (1707—1783), whose life presents one of the most remarkable examples of scientific labour. He composed, it is said, more than one-half of the papers on mathematical subjects which occupy forty-six quarto volumes, published by the Academy of Sciences of St. Petersburg, and at his death he left nearly one hundred memoirs ready for the press. He contributed during his lifetime to the journals of various scientific societies, besides publishing twenty-nine quarto and two octavo volumes in Latin, six octavo volumes in German, and five in French. He lost his sight, it is said, by his continually writing and calculating; but after this misfortune, which occurred in his fifty-ninth year, he continued to calculate as actively as before, and dictated his books instead of writing them. His "Elements of Algebra" was produced in this way; and Euler's amanuensis, who was only a tailor's apprentice, ignorant of algebra when he began his task, is said to have acquired a competent knowledge of algebra by merely taking down Euler's words,—such was the clearness and simplicity of the book. These qualities caused it to become widely known, and there is no European language into which it has not been translated.

The application of algebra to geometry led to the study of a great number of different curves, as we have already seen, and this branch of mathematics was largely cultivated in the eighteenth century; almost new fields of research were now opened out by the extension of the principle of co-ordinates to lines of *double curvature*, and to various kinds of *surfaces*. The annals of mathematics would furnish a long series of valuable labours in every branch of the science; but as these could not be made intelligible to a general reader by much less than a complete treatise on mathematics, they must here be passed over. The names of the most distinguished mathematicians of the eighteenth century include Euler, the two brothers John and James Bernouilli, Laplace, Lagrange, Clairaut, Brook Taylor, Maclaurin, Stirling, Cotes, Saurin, Robins, Demoivre, D'Alembert, Daniel and Nicolas Bernouilli, etc. The last-named published some essays on the "Doctrine of Chances or Probabilities," and the theory received most important development from Demoivre. The theory of probabilities is not a mere mathematical curiosity, but one of great utility, not only in the determination of certain scientific problems, but also in its application to political and economic questions. Such matters as *annuities*, *life assurances*, *policies*, etc., are obvious instances. But the attempt has so been made of submitting to mechanical analysis the probabilities of legislative councils, juries, courts, etc., arriving at given conclusions. Many questions affecting commercial speculations are more amenable to treatment by the doctrine of probabilities, and the hazards in a game of chance can of course be accurately estimated.

PIERRE SIMON LAPLACE was born in 1749 near Honfleur. At an early age he distinguished himself for his mathematical ability, and when, by the influence of D'Alembert, he was appointed to the professorship of mathematics in the Military School at Paris, he began to plan the great work to which he devoted the best part of his life and all the powers of his genius. His object was nothing less than to interpret the whole mechanism of the universe. Laplace did not create a new science, like Galileo, nor did he add a new calculus to our mathematics, like Descartes or Leibnitz; but he collected and arranged all that was known on the mechanism of the universe; he traced the doctrine of gravitation to its ultimate consequences; and he reduced a vast range of the truths of physical science to the dominion of mathematical knowledge.

Though Newton pointed out the nature and general results of the forces interacting between the bodies of the solar system, he did not trace out all the effects which those forces produce. The immense complication produced by the actions and reactions of so many bodies, and the changes of their positions and velocities, outstripped the powers of geometry to unravel. So numerous were the varieties of direction and intensity of the different forces, that the maintenance of permanent equilibrium, under conditions so changing, appeared a miracle; and Newton supposed that a superior power must from time to time intervene to restore the order and stability of the system. Euler, who had obtained a more extended knowledge of the perturbing actions of the planets upon each, also refused to admit that the solar system contained within itself the principle of permanent stability. It had been found by comparing ancient observations with modern ones that the motion of the planet Jupiter was being constantly accelerated, and that the motion of Saturn was undergoing a diminution. The inference from the supposed cause of these changes was that the former planet was approaching the sun, and that the latter was receding. The amount of these changes was not great, but their existence was undoubted; and it seemed perfectly just to conclude that the magnificent planet Jupiter would finally plunge into the sun, while the mysterious Saturn, with its ring and seven satellites, would gradually withdraw from our system and pass away into the depth of space. Again, there was the undeniable secular acceleration of the moon, which appeared inevitably destined to end by our satellite being precipitated upon the earth. The dates at which these catastrophes would happen could not be precisely calculated, but it was known that they were very distant. It was probably for this reason that those results, so startling to astronomers, raised no alarm in the popular mind. The scientific world, however, realized more clearly the destruction to which our planetary system was tending, and the most profound mathematicians were invited to study these threatening perturbations. Euler, Lagrange, and others attempted in vain to discover any counteracting influences, and it appeared as if nothing remained but resignation to the inevitable. In this state of the question Laplace drew from certain hitherto overlooked branches of mathematical analysis the means of investigating the variations of the velocities of the moon, Jupiter, and Saturn; and he showed that these were after all nothing but oscillations of long periods, and that, like the rest, they were due to the action of gravitative attraction. The orbit of the earth round the sun is in the main an ellipse; but this ellipse is liable to change its form, on account of the attraction of the earth by the other planets. Now, from the time of the earliest recorded observations the orbit of the earth has been becoming less elliptical, its figure approaching continually nearer to the circular form, and for some indefinite period it will continue to do this, without actually becoming a circle; and at some future time it will be resuming its more elliptical form, according to the same laws by which it is at present becoming less elliptical. Now, Laplace showed that the changes of the motion of the moon depend upon these changes in the form of the earth's orbit, a diminution of the eccentricity necessarily producing an increase of the moon's velocity, and *vice versâ;* and he proved that the observed amount of the moon's acceleration from the earliest ages is accounted for by these causes. The perturbations of Jupiter and Saturn were shown by Laplace to be due to the action of these planets upon each other; and these inequalities run through a period of about nine hundred years, during one-half of which the united action of the planets retard the one and accelerate the other, and during the other half has the reverse effect.

Laplace's theoretical calculations concerning Saturn and its ring are remarkable as predictions, which W. Herschel's observations confirmed. The ring, as every one knows, is entirely detached from the body of the planet. The distance between is 20,000 miles, and the ring is 54,000 miles broad, while its thickness is only 250 miles. With the exception of some concentric streaks (which are now considered to indicate several detached rings), the most skilled observers had failed to detect any mark in the ring which could indicate its period of revolution, if any. From theoretical considerations Laplace concluded that the ring must revolve in its own plane, and that the period of its revolution ought to be 10 hours 33 minutes 36 seconds. By very delicate and careful observations Sir W. Herschel concluded that the ring revolved in 10 hours 32 minutes 15 seconds. In February, 1789, Laplace published a paper proving further that the theory of gravitation required that Saturn itself should revolve about its axis. In November of the same year Herschel announced that he had observed the rotation of the planet.

The name of Laplace must ever be associated with one of the grandest speculations of modern philosophers. The chief grounds upon which is founded the bold conjecture called the *Nebular Hypothesis* may first be briefly stated. All the planets revolve round the sun from west to east, and they revolve in planes that are but slightly inclined to each other. All the satellites (those of Uranus excepted) revolve round their primaries from west to east. The sun, and all the planets and satellites whose rotation has been made out, revolve on axes from west to east. A calculation which has been made by aid of the doctrine of probabilities shows that the chances against this coincidence having been the result of mere accident are many thousands of millions to one. In Laplace's hypothesis these phenomena find their explanation in the circumstances through which, as he conceived, the solar system was formed. He supposes that at some remote epoch our sun was the centre of an immense nebula extending far beyond the region in which the most distant planet of our system now revolves. But, at the period contemplated by Laplace, no planet was yet in existence. The materials of which the sun and the planets were afterwards formed existed, at first, in a state of uniform diffusion. This diffused matter was at so very high a temperature, that all its constituents may be conceived to have existed in the state of gas. As the gaseous matter cooled, some of its particles, condensing into liquids or solids, might collect by gravitative attraction at the centre of the nebulous mass, and thus a centre of attraction would be established, towards which the gradually condensing matter would be drawn, and add its effect to the centripetal tendency. Any circumstance whatever which could cause one part of the nebulous mass to differ from another (such, for example, as a radiation of heat greater on one side than on the other) would cause rotatory movement to be set up, and this movement would increase in rapidity. There would at length arrive a period in which the velocity of rotation of the outermost parts would become great enough to overcome the attraction of the central nucleus, and as they condensed these parts would separate into a ring, which would continue to revolve with the velocity it possessed at the moment of its separation from the rest. In the planet Saturn we witness the results of such a separation, and it is possible by an experiment on a small scale perfectly to imitate the effects here spoken of. Similar separations of revolving rings we may conceive to have successively taken place; and these rings while all revolving in nearly the same plane, would be endowed with different velocities; those nearer to the central mass, corresponding with greater condensation and more rapid rotation, would separate last, and would possess the greater velocities. There would thus be produced a series of rings revolving in nearly the same plane, and possessing greater velocities as they were nearer the central mass; but these rings would not continue to exist as such unless they possessed a perfectly uniform structure throughout. Each ring would therefore break up into several masses, which would assume spheroidal forms in consequence of their fluidity, and would also continue to rotate in the same direction as the original rings. Each of these spheroidal masses would also have a rotation about its own axis, and would, with the vaporous atmosphere around it, repeat on a smaller scale the same series of phenomena as the original nebular mass; hence the origin of rings like that of Saturn and of satellites such as belong to the earth, Jupiter, Saturn, etc. The nebular hypothesis explains the central sun; the spheroidal planets revolving at various distances and with various velocities round it; the satellites which attend the larger planets; the ring of Saturn; the revolutions of planets and satellites about their own axes, and the fact that all these revolutions take place in one direction. Comets are explained in Laplace's cosmogony as nebulous masses not originally any part of the great nebula from which our system was consolidated, but smaller wandering nebulæ which the attraction of the sun has caused to deviate towards him, and in some cases has compelled to travel in very elongated orbits about him. This well accords with the fact of the orbits of comets lying in planes inclined at all angles with that of the sun's equator, in striking contrast with the comparatively small inclination of the planetary orbits to that plane. Such wandering masses must also have sometimes been actually drawn into the body of the sun, and the perturbations produced by their action, when passing through the interplanetary spaces at the period when the condensation was in progress, must have caused the orbits of the planets to deviate from their original plane, which would otherwise have shown an exact coincidence with that of the sun's equator.

Soon after sunset, especially in the months of February and March, a peculiar nebulous light may be observed in the western sky, extending upwards from the place where the sun has set. It has a conical form, or rather the figure which would result from a lens-shaped luminous atmosphere surrounding the sun, as represented in Fig. 127. It is seen extending 50° at least from the sun, and its breadth at the base is between 8° and 30°, but it is so ill defined at the edges that its limits cannot be certainly defined. It is seen to great advantage in the tropics, but in this country it rarely appears equal in brilliance to the Milky Way; occasionally, however, it has been so bright that some persons have mistaken it for a comet. Whatever may be the cause of this remarkable phenomenon, it is assuredly connected with the sun, for its axis is always exactly in the plane of the sun's equator, and as this is not very different from that of the *zodiac*, the luminosity is known

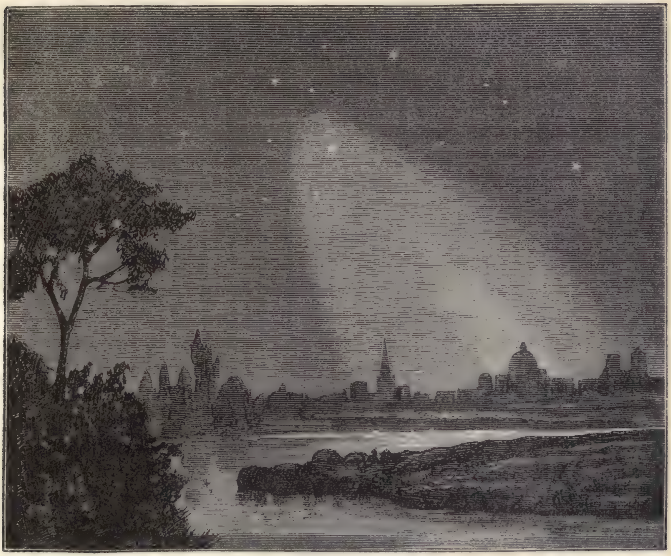

FIG. 127.—THE ZODIACAL LIGHT.

by the name of the Zodiacal Light. It should be mentioned that the zodiacal light is also visible before sunrise. The matter of the zodiacal light, whatever its nature, is sufficiently attenuated to allow the light of the smallest stars to pass through it without perceptible diminution. Laplace supposed the zodiacal light to be a residual portion of the original nebulous mass of the solar system, and if its nebulous nature should be satisfactorily proved by observation, Laplace's hypothesis will receive additional confirmation. The hypothesis, not being founded on observation or calculation, was put forward by Laplace himself with much diffidence. But it unites so many apparently unconnected facts by a few simple and general laws, and those, too, laws known to be in action, that it has every mark of probability to recommend it. This explanation of the origin of the solar system involves no invention of a new agency, and no assumption for the occasion of special laws to suit the case. The hypothetical part consists only in the assumption of a certain condition of things at a remote epoch, namely, the existence of all the matter of the solar system in a vaporous state. The rest is deduced from this as the effects of known laws; and, if the *à priori* evidence in favour of the hypothesis is weak, it is weak only in so far as we are unacquainted with all the conditions under which the known laws operated in distant time. Again, Laplace's hypothesis transcends every other that had been put forward to explain the origin of our system by the circumstance that it applies equally to all the stars, and to the nebulæ and star-clusters. The identity, too, of the materials of the stars with some of the chemical elements of which the earth is composed—one of the most striking discoveries of modern science, which will be discussed in a subsequent chapter—harmonizes with the nebular hypothesis in a remarkable manner, as do many modern observations in celestial physics. It was the work of Newton to show that the great law of gravitation governed all the motions of the planets of our system; but, as he was unable to find in the laws of gravitation and of motion the necessary stability of that system, he conceived that the Deity must occasionally interfere to regulate His work. Laplace showed that the laws of gravitation and of motion assure the eternal stability of the solar system, and he demonstrated that the threatening perturbations which affect the planetary orbits are, in reality, but slight oscillations about a mean position—"immense pendulums of eternity which beat centuries as ours beat seconds." And as he found the law of gravitation to be efficient in the preservation of the system, so he conceived it might also have operated in its formation.

The great project of Laplace was accomplished by the publication of his "*Mécanique Céleste*," a splendid monument of intellectual power, the publication of which forms an epoch of the history of the human mind. This modern *Almagest* uniformly applies to astronomy and general mechanics the highest power of mathematical analysis. The first and second volumes were published in 1799, and treat of the laws of equilibrium and motion, gravitation, the centre of gravity, the figures of the celestial bodies, the tides of the ocean and of the atmosphere, the movements of the celestial bodies round their centres of gravity, the precession of the equinoxes, the moon's libration, the theory of Saturn's ring. The third volume, published in 1802, treats of the theory of the motions of the planets and of the moon. In the fourth volume, which appeared in 1805, the author investigates the theory of the satellites of Jupiter, Saturn, and Uranus; the theory of comets; atmospheric refraction; the barometer; the masses of the planets, etc. The fifth volume, published in 1825, treats of the figure and rotation of the earth, attraction and repulsion, elastic fluids, capillary phenomena, the history of the various researches relative to the system of the world, etc. Laplace published in 1796 a work bearing the title of "*Exposition du Système du Monde*," from which persons unacquainted with mathematics may obtain a just knowledge of the methods to which modern physical astronomy owes its astonishing progress. The production is distinguished by great clearness and simplicity of style, combined with elegance and a scrupulous precision. Another publication of Laplace was on "Probabilities," and it was worthy of the author of the "*Mécanique Céleste*." This illustrious mathematician died in 1827. Like Newton, he possessed the humility of a true philosopher, and his last words were, "It is little that we know, but what we are ignorant of is immense."

The labours of other eminent mathematicians contributed to establish the grand result of the stability of the solar system. CLAIRAUT (1713—1765), in 1758 undertook to calculate the period when Halley's comet, that had appeared in 1682, would again be seen. After a long and intricate calculation of the disturbing actions on the comet of Jupiter and Saturn, he found that the attraction of those planets would so retard the comet that its period of revolution would be lengthened by more than a year and eight months. The constellation in which the comet would first appear was named, and the stars among which it would seem to pursue its course were indicated. These predictions were completely verified, and for the first time the movement of a comet was calculated beforehand with the same precision as the planetary orbits.

Another illustrious mathematician who was deeply engaged in working out the problem of the stability of the solar system was LAGRANGE (1736—1813), a native of Turin, at the Military College of which city he was appointed to the professorship of mathematics before he had completed his twentieth year. But he soon left Turin to enter upon an appointment at Berlin, where he lived twenty years, and where he worked out many of his most remarkable investigations. In 1787 he removed to Paris, where he lived for twenty-six years, and died in 1813 at the age of seventy-seven. An explanation of the mathematical treatment of the problems which Lagrange worked out would be foreign to the design of this work, but the reader has probably already gained some idea of the nature of questions which engaged the attention of the great French mathematicians of the eighteenth century.

The moon has ever attracted the closest attention of astronomers, and its complicated motions have presented some of the most difficult problems, so that it is only within a recent period that these motions have been completely mastered. As nearer the earth than any other celestial body, the physical structure of the moon has admitted of more immediate observation, from the period when Galileo's telescope first revealed the irregular nature of its surface. The earlier observers considered the diversities of the appearance presented by the moon's surface as partly due to the distribution of land and water, and Milton's reference to Galileo's discovery (p. 109) embodies this view. The appearance presented by the moon, when viewed by a common spy-glass, is rudely represented in Fig. 128. But when a good telescope is directed to the face of our satellite, the forms of mountains and plains are distinctly discernible. The most curious objects, however, are the crater-like formations, which are undoubtedly of volcanic origin, and indicate that at some period the moon was in a semi-fluid state. They are precisely such openings as would be formed by the passage of gases from the interior. The passage of bubbles of air through a semi-fluid mass of slaked lime has been observed to produce appearances exactly similar to many of the lunar craters. Fig. 129 represents the appearance of a portion of the moon's crescent as seen by a reflecting telescope, and exhibits clearly the crater-like depressions. Maps of the moon were made by Hevelius, an astronomer of Dantzig, and published in 1647. Cassini (J. D.) and T. Meyer published charts in 1680 and 1749, and others have since appeared, of which the most elaborate is Beer and Mädler's, published in 1837.

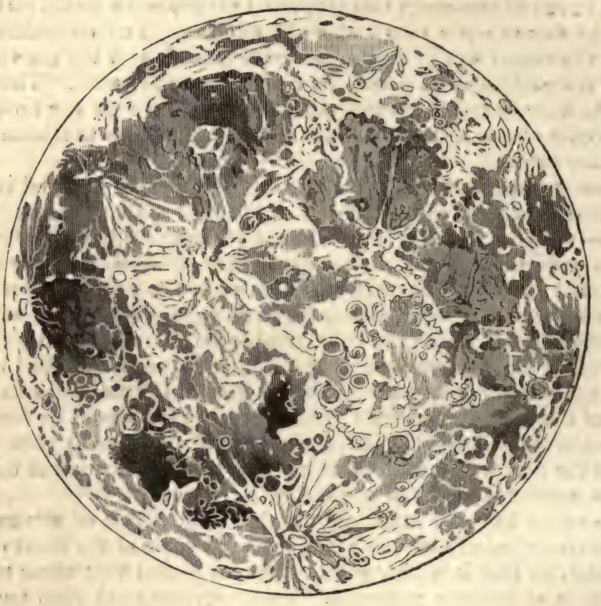

FIG. 128.

A very slight attention bestowed on the appearance presented by the moon, even to the naked eye, reveals the fact that the moon always presents to us the *same face.* Of this face we see more or less as it is illuminated by the sun, and with the telescope the same markings which are distinctly seen on the full moon are visible in the part of the moon's face illuminated only by the "*earth-shine*," when the moon presents the appearance popularly known as "the old moon in the new moon's arms." It must strike every person who considers this phenomenon as a very remarkable circumstance, that the moon, in thus always presenting the same side to the earth, should revolve about her own axis in precisely the same time as she revolves about the earth. That the moon does revolve about her axis is evident, if we reflect that to the *sun* she presents, in succession, every part of her surface. It is necessary to call attention to this, because some persons, considering only

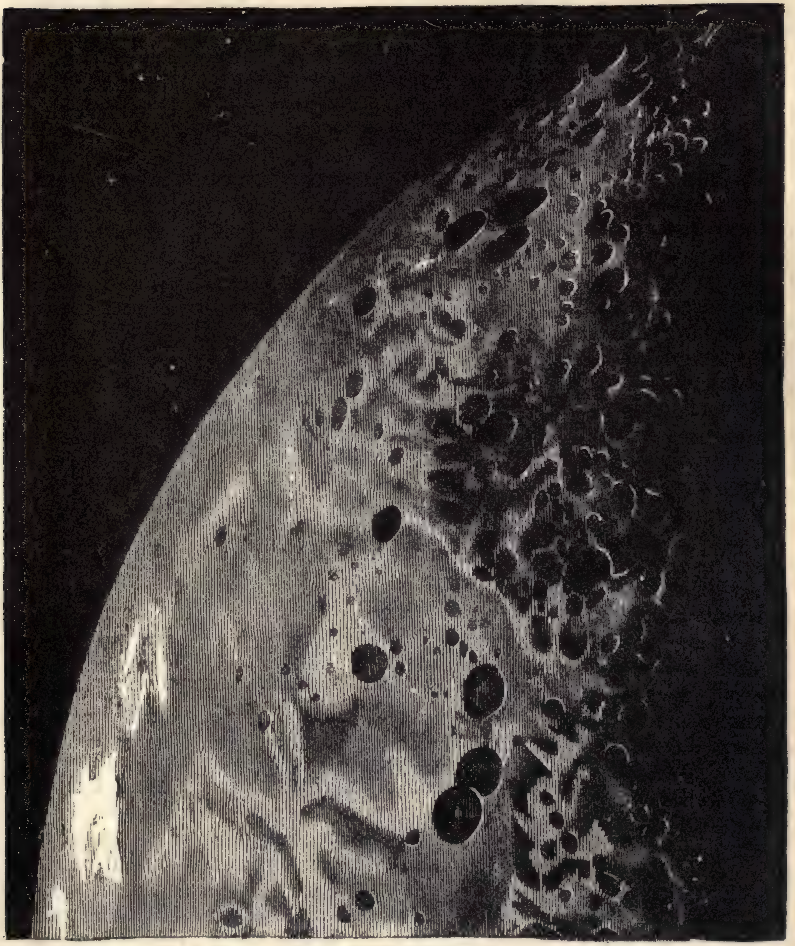

FIG. 129.

the moon's relation to the earth, have been unable to admit that she turns on her axis at all. Now, it is impossible, by all the laws of probability, for us to believe that this perfect equality is a mere accidental coincidence, and not the effect of some connecting cause. Attentive observation of the lunar spots shows that certain parts of the surface near the edge of the moon's disc disappear and reappear periodically. These oscillations seeming to indicate a sort of balancing of the lunar sphere, the movement has received the name of *the libration*, from a Latin word, *libratio*, a balancing. This is due to several distinct causes: the moon's axial velocity being sensibly uniform, while its orbital velocity varies in different parts of its course round the earth, sometimes a little more of the eastern or western edge is visible at one time than another; again, the moon's axis is not quite perpendicular to its orbit, and sometimes we see now a little of one polar region, now a little of the other; lastly, we view the moon from different points, at its rising and at its setting. The combined effect of all these librations is that the part of the lunar surface visible at one time or another, is $\frac{57}{100}$ths of the whole, the remainder, or the portion never seen, is $\frac{43}{100}$ths. The portion of the moon's surface now visible is the same that our most remote ancestors looked upon, and mankind will never behold the opposite hemisphere of our satellite. It might be supposed that the *secular acceleration* (p. 216) would in time prevail over the axial rotation; but the periodic inequalities which affect the orbital movements of the moon equally affect its axial rotation. The law of gravitation explained so much that it would be strange if it failed to explain the isochronism of the moon's rotations. The question was still in an unsettled state when Lagrange's investigation led him to conclude that it depended on the circumstance of our satellite possessing a certain figure, which could not be discovered by observation. He supposed that, the moon being originally a fluid body, it assumed, when it solidified, a less regular figure, in consequence of the earth's attraction, than if that attraction had not existed. The moon's equator, by virtue of this attractive action, assumed the form of an ellipse instead of a circle, with the longer axis of the ellipse directed towards the earth. The lunar equatorial regions may of course have the bulging due to the moon's original axial rotation, but the bulging at the parts *a* and *b* (Fig. 130) would be greater than at *c* and *d*. If the moon were at any time so placed that the line *a b* was not directed towards the earth's centre, the effect of the latter's attraction would be to bring it into this position. Lagrange's theory has the merit of accounting for the otherwise inexplicable additional coincidence of the nodes of the moon's equator with the nodes of its orbit.

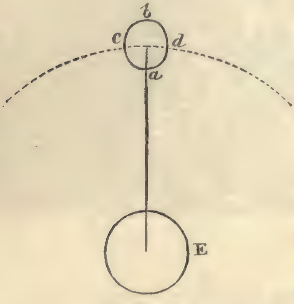

FIG. 130.

In mechanical science the principle of *virtual velocities* was recognized throughout the eighteenth century as the great principle of statics. Lagrange extended this formula in a more general manner than his predecessors, and showed that it is applicable to any system whatever of forces. In dynamics, which became a science only in the hands of Newton, in 1687, the beginning of the eighteenth century witnessed a very singular controversy concerning the measure of the force possessed by moving bodies. Leibnitz gave to the force of a moving body the appellation of *vis viva, living force*, in order to distinguish it from the force of bodies not actually moving, but having only a tendency to move, which he called *dead force.* Mathematicians were divided into two opposite camps on the question. Leibnitz and the German philosophers took one side; the English ranged themselves on the other. France was divided by two scientific leaders in the dispute,—the celebrated female mathematician, the Marquise du Châtelet on one side, and Clairaut, the distinguished member of the Academy of Sciences, on the other. The mode of estimating pressure or dead force was at no time a matter of dispute; but with regard to moving bodies, Leibnitz in 1686 raised the question by a publication bearing the title, "Demonstrations of a Remarkable Error of the Cartesians and others in estimating the Moving Forces of Bodies." He points out that a body, after falling freely from rest through a space of 4 feet, has a velocity only double of that acquired by falling 1 foot; and as these velocities directed upwards would carry the body to the respective heights 4 feet and 1 foot, therefore he said as the forces are proportional to the height to which they carry the bodies, it follows that the *double* velocity corresponds with a *quadruple* force, and so on. That is, the forces are as the *squares of the velocities.* It was objected to this doctrine by the Cartesians that the effect should not be estimated in this way, for the *time* required in each was as the simple velocity. J. Bernouilli entered into correspondence with Leibnitz on this subject, and he came over to the views of the German philosopher. When in 1724 the French Academy of Sciences offered a prize for the best dissertation on the communication of motion, it was carried off by an opponent of the Leibnitzian *vis viva.* The dispute was renewed, and many were the arguments and replies on one side and the other. We need here notice only an experiment brought forward by the partizans of the *vis viva.* This consisted in allowing bodies of the same size and figure to fall from various heights into soft clay, and then measuring the depths to which they penetrated. The results showed that these depths were proportional to the height of the fall and the weight of the bodies jointly, that is, to the *product of the weight into the square of the velocity.* However, the dispute waxed warm, and Clairaut, Herman, Kœnig, Madame du Châtelet, Voltaire, Maupertuis, S'Gravesande, Dr. A. Clarke, and others entered into it. The question continued to be agitated until D'Alembert published his treatise on Dynamics in 1743, in which he shows that it is merely a question of words, and that in some cases it is desirable to measure forces in the one way, in the other cases in the other way. The term *vis viva* is now applied to the product of the square of the velocity of a moving body into its mass, while the word *momentum* designates the product of the mass into the simple velocity.

Many important propositions relating to Hydrostatics, Hydraulics, and Pneumatics were investigated during the eighteenth century, and the importance of such scientific investigations in practical engineering and public works was everywhere acknowledged even in the earlier part of the century. Any detailed mention of such investigations would hardly prove of sufficient interest to the general reader, as their explanation would necessarily assume the form of elementary treatises on these branches of science. As allied to mechanical science in some respects, we here may mention the beginning of some new lines of investigation. LA HIRE (1640—1718) first examined the muscular strength and working power of men. Lambert, D. Bernouilli, Coulomb, and others extended observations and experiments on this subject, and Prony, Desaguliers, and others investigated the muscular power of horses. The subject of friction in machines was studied successively in France by AMONTONS (1663—1705), PARENT (1666—1716), MUSSCHENBROEK, CAMUS, COULOMB, and others contributed experimental researches to this subject.

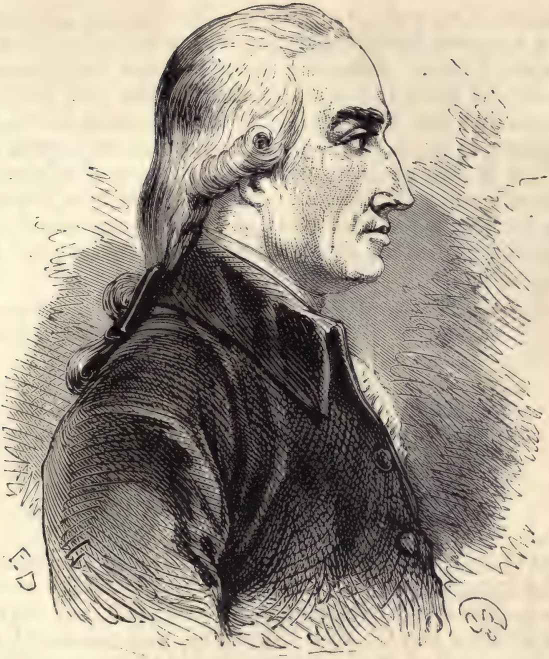

FIG. 131.—JOSEPH BLACK.

# CHAPTER XII.

## PHYSICS OF THE EIGHTEENTH CENTURY.

THE property of a lens to form an image depends upon its power of refracting the rays of light; but as the refrangibility of the rays of light varies according to their colour, differently coloured rays of light will be differently refracted by the same lens, and will be brought to foci at different distances from the lens. Newton's experiment (page 218) showed that ordinary light by the different refrangibilities of its coloured constituents is decomposed in passing through a prism, and it will have already been observed that a lens acts by the same laws as the prism (page 157). Hence, with ordinary light, a common convex lens does not form a single image, but an indefinite series of images. The violet rays, as the most refrangible, are brought to a focus nearest the lens, while the red rays have their focus at a greater distance, and at intermediate positions are images due to the variously coloured rays of the spectrum. The diagram, Fig. 132, shows the effect of a convex lens in separating rays of white light into its constituent coloured rays. According to the order of their refrangibility the rays are bent more or less out of their course, and it will be observed that as they are more refrangible they cross the axis of the lens at points nearer to it; hence the images formed are always more or less indistinct in their outlines, and exhibit fringes of colour. These colours are very noticeable when the image formed by a lens is viewed by another lens, as in the telescope; and this circumstance much impaired the efficiency of the earlier astronomical telescopes. Newton made some experiments from which he was led to the conclusion that it would be impossible by any combination of materials in a lens to obtain the requisite refraction without the separation of the coloured rays; but in 1729 Mr. Charles More Hall was convinced that the optical arrangement of the human eye would afford some clue to the solution of the problem, and it is said that he succeeded in producing lenses that formed images free from colour. It was affirmed towards the end of last century that several telescopes made under Mr. Hall's directions were in existence. It is certain that Mr. Hall's discovery attracted so little notice that it was generally unknown, and the person with whose name the *achromatic lens* is always associated, discovered it for himself in the manner to be presently related. It may be first stated that the celebrated Euler also had, in 1747, arrived at the conclusion that it was possible to make an achromatic lens by imitating the construction of the eye. He proposed to form the lenses of glass enclosing liquids, so that the refraction might be produced as in the eye. All attempts to carry Euler's system into practice proved to be complete failures; and, in fact, Euler himself afterwards admitted that his mathematical analysis overlooked some of the real physical conditions of the problem. It happened curiously enough that the inventor of the now well-known achromatic combination was led to it by a controversy with Euler, in which he endeavoured to combat the conclusions of that eminent mathematician as to the possibility of constructing achromatic lenses.

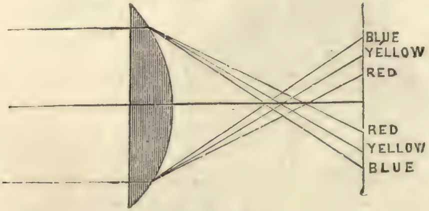

FIG. 132.

JOHN DOLLOND was born in London in the year 1706. His father was a French refugee who had left his country on the revocation of the Edict of Nantes, and had established himself in Spitalfields as a silk-weaver, where John was brought up to the same vocation; but having a studious turn, young Dollond contrived to make himself acquainted with mathematics, astronomy, and optics. John Dollond's eldest son, named Peter, was in his turn trained to the business of silk-weaver; but Peter also had imbibed a taste for scientific pursuits, and resolved to enter upon the business of an optician, counting upon his father's mathematical knowledge. The undertaking proved a success, and John Dollond joined his son in the optical business in 1752, and soon became very proficient in matters pertaining to the practical part of the art. His first achievement was an improvement in telescopes, by using five lenses in the construction of their eyepieces. Improvements in other optical instruments followed, and Dollond soon became known to men of science in London. Several of Dollond's papers had been published in the "Philosophical Transactions" when he entered upon the discussion with Euler to which reference has already been made. Sir Isaac Newton had declared that the dispersion of the coloured rays by refraction had always the same proportion to the mean deviation of the rays, whatever might be the nature of the refracting substance. It was this doctrine which Dollond was maintaining (as against Euler) when he argued that if the fact be as stated by Newton, there could be no refraction without dispersed colour: dispersion and refraction being always proportional, all hope of constructing achromatic lenses was proved to be untenable.

In his reply Euler attempted to show that the assumption of an invariable relation could not be maintained; but Dollond was apparently not convinced by Euler's calculations, for it was not until some years afterwards, when a paper by a Swedish mathematician was read before the French Academy of Sciences, giving a geometrical demonstration of the inadmissibility of the law assumed by Dollond, that the English optician acknowledged that this principle, which Newton's experiments appeared to have established, might, after all, be inaccurate. It was perhaps the great authority of Newton's name which had made it possible for this dispute to be carried on so long without any person verifying experimentally the grounds upon which Newton's conclusions were based. Dollond at length thought that the point would be settled by a resort to the fountain-head of science,—a direct interrogation of Nature, to use the Baconian phrase. Here we may briefly indicate the point in dispute. When a ray of light has traversed a prism, it is, as we have seen, not simply refracted in a single ray, but into a group of coloured rays, spreading out among themselves like the ribs of an extended fan. All the coloured rays are inclined (the red least, the violet most) in their direction to the course of the original ray. If we select a ray, intermediate between the red and violet,—for instance, the yellow ray which forms the brightest part of the spectrum,—we may in any given case determine by what angle its direction, as it emerges from the prism, differs from the direction of the original ray. This angle may represent the mean refraction of the ray, and may be called the *angle of deviation*, or simply the "*deviation*." Again, the angle between the directions of the emergent extreme red and violet rays, which represents the spreading out of the colours, is termed the *angle of dispersion*, and this we may call, for shortness, the "*dispersion*." Now, the conclusion which Dollond and others had drawn from Newton's experiments was that the *dispersion* and *deviation* have one and the same fixed proportion to each other in every case.

The reasoning of Klingenstierna, the Swedish geometer already alluded to, set Dollond on the true experimental track, and in 1757 he began a series of experiments which he prosecuted, as he says, "with a resolute perseverance," devoting nearly a year and a half to this one object, until his investigations led him to a most interesting and important discovery. One of his first experiments was to form a prism with water contained in a vessel with glass sides, so arranged that the angle between these could be adjusted to any desired degree. In this vessel he placed a glass prism, with its refracting angle uppermost. Thus he had virtually two prisms, one of glass and one of water, with their angles in opposite directions, so that their united action tended to destroy each other's effects; and by trial he so adjusted the inclined faces of the water prism, that the refractive effect of the combination was completely destroyed; that is, objects viewed through the prism were seen in their ordinary positions. In this case, then, the "*deviation*" was *nil*; and therefore, if Newton's principle were true, the "*dispersion*" should have been also *nil*. So far was this from being the fact, that the objects were seen as much surrounded by coloured fringes as if they had been viewed through the glass prism alone. Dollond's next step was to place in the water prism a glass prism of a small refracting angle, and then he changed the angle of the water prism until he found one at which the *objects were seen free from colour*, and yet there remained a considerable amount of refraction. Having thus proved by experiment the possibility of constructing achromatic lenses, Dollond proceeded to try experiments with prisms prepared of various substances, and was fortunate enough to find in two different kinds of glass precisely what he required. The common glass, which goes by the name of crown-glass, has a refractive power which is but slightly inferior to that possessed by the denser kind of glass called flint-glass, while the dispersive power of crown-glass is greatly inferior to the dispersive power of flint-glass. The refractive powers of these kinds of glass are as 20 : 21, while their dispersive powers* are as 14 : 21. It is therefore possible to make two prisms, one of flint-glass and the other of crown, with such a relation between their refracting angles that when opposed they shall counteract each other's dispersive effects, and yet leave a refracting effect.

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* The measure of the dispersive power is the ratio of the angle of dispersion to the angle of deviation.

It will not be difficult to understand the principle of the achromatic lens if we suppose, for the moment, that flint-glass has the same refracting power as crown, and that we have a prism of each placed as in Fig. 133; then a ray of light E F would be refracted in the directions F L, L M; and if the refracting angle A B C of the one prism equals the refracting angle B C D of the other, the emergent ray L M would be parallel to the incident ray E F, but for the greater dispersive power of the flint-glass, which would more than counteract that of the other, and consequently L M would really only represent the mean direction of a diverging bundle of coloured rays due to the excess of the dispersive effect of the flint-glass. Now, by lessening the refracting angle B C D of the flint-glass prism, we could make it such that the dispersive effect of the flint-glass would neutralize the oppositely-directed dispersive effect of the crown, and B C N being that angle, the emergent ray H K would be free from colour; but it would plainly now be deviated from the original direction, by reason of the uncompensated refractive action of the crown-glass prism. The result, then, is that we obtain, without colour, a refractive effect, the *direction* of which is due to the crown-glass prism. We have supposed that the refractive powers of the two kinds of glass are equal, as the modification which their slight difference would introduce into the arrangement can be easily understood. But a more important modification of the above explanation is required by the actual facts. The emergent rays have been spoken of as without colour. Now, this is a result which, strictly speaking, cannot be attained. The only thing that the adjustment of the angle B C N can effect is the emergence from the system in parallel directions of two given sets of prismatic rays, *e.g.*, the red and the green rays, the yellow and the violet, and so on. There will still exist a certain small amount of dispersion. But, using a system of three prisms instead of two, three spectra instead of two may be made to overlap, so that an almost perfect achromatism is obtained. Fig. 134 will show the manner in which these principles are applied in the construction of an achromatic lens. L L is a convex lens of crown-glass, and *l l* a concave one of flint-glass. A ray of light S falling on the convex lens at F would be refracted exactly as by a prism A B C, whose

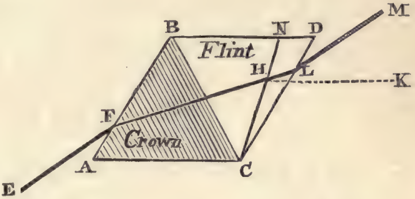

FIG. 133.

faces touch the surfaces of the lens at the points where the ray enters and leaves the lens. The ray thus refracted is also dispersed in the direction F V P, F R T, and would of itself form a spectrum on a screen at P T but for the flint-glass lens *l l*; and the violet ray would cross the axis of the lens at V, while the red would cross at a more distant point

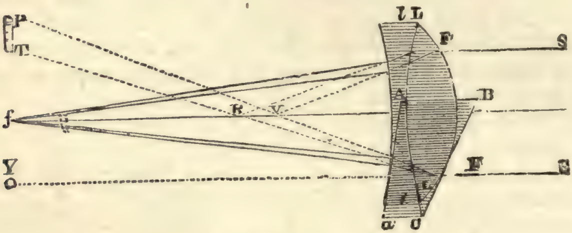

FIG. 134.

R. But the flint-glass lens, or say the prism *a* A *c*, being interposed, another direction is given to these rays, so that they *meet* at *f*; similarly of the ray S F. These coincidences of the rays at one point *f*, instead of at two points R and V, of course give a greatly improved clearness and precision to the image of an object.

Many trials had, of course, to be made before Dollond successfully carried his idea into practice; but such was his perseverance and ingenuity, that it was not long before he produced refracting telescopes giving images retaining scarcely any trace of the coloured borders which formed so great a defect in the former telescopes. When the successful issue of his efforts was announced, many eminent mathematicians were unable to credit the report. The achromatic lens has been of immense service in the advancement of science. The modern astronomical telescope and the compound microscope owe their power to this invention, and the practical advantages obtained by the use of the achromatic lens in sextants and surveying instruments are incalculable. Dollond constructed his first achromatic telescopes in 1758, and although these were more efficient than the old telescopes of three times their length, he extended his principle to the construction of object-glasses composed of three lenses. Such was the optical excellence of these object-glasses, that one of 43 inches focal length would have a magnifying power as great as could be applied in a telescope of the old construction 50 feet in length. The annexed cut, Fig. 135, exhibits sections of various achromatic lenses, including triple lenses. For the discovery of the achromatic lens the

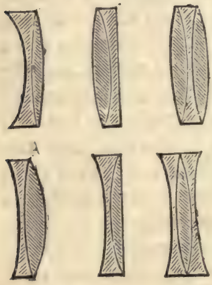

FIG. 135.

Royal Society presented Dollond with the Copley gold medal, and his fame became European. In 1761 he was elected a Fellow of the Royal Society, but he did not live long to enjoy his honours. On the 30th of November, in the same year, while reading a mathematical treatise, he was seized with apoplexy, and died in a few hours. His son, who had been associated with him in the optical business, successfully carried on the establishment for many years.

With the eighteenth century we enter upon a branch of physical science which took form only within that period. Its phenomena unceasingly present themselves, and there is no department of natural science with which they are not more or less connected. Its laws affect our every-day life and govern our industrial arts in a boundless measure. It may be asked how it has come to pass that a science concerned with phenomena so important and so constantly under observation has been so late in its origin. It was not for the want of facts. But facts by themselves, however numerous and however important, do not constitute a science. A science is constituted only when the facts are co-ordinated by laws, and these laws in their highest generality always formulate *quantitive relations* between phenomena. Before any such relations can be formulated, reliable measures of the phenomena must be obtained, and it has been noticed that the development of every branch of science has always kept pace with the means of obtaining exactness in the measurements of its fundamental phenomena. The science we now enter upon had waited for ages for its instrument of measure — a very simple, and now a very familiar one. An ordinary form of it is represented in the annexed cut, Fig. 136. The reader will at once recognize the *Thermometer*, and perceive that the remarks just made apply to the science of Heat. But already we have referred to the thermometer as having been in existence in the preceding century (p. 106). The apparatus, indeed, bore this name, but it was in reality no measurer of heat, or even of *temperature;* for although it would indicate changes of temperature, there existed no standard with which the observation could be compared. The invention of the instrument in this imperfect form has been attributed to the various individuals whose names have been already mentioned in connection with it, and also to others. The invention of the thermometer as a truly scientific instrument may be more properly referred to the individual who first supplied the means of making its observation comparable. This appears to have been no other than the illustrious Newton, who describes in the "Philosophical Transactions" of 1701, a thermometer with fixed points of temperature. Linseed-oil was the expansible liquid employed in this thermometer, and the lowest fixed point of the scale was obtained by plunging the instrument in snow, and marking the point at which the

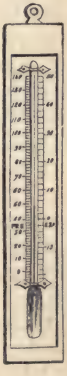
FIG. 136.

oil congealed. The upper fixed point was given by the temperature of the human body, and the interval between was divided into 12 degrees, and, by divisions equal to these, the scale was extended upwards. By the scale of this instrument Newton found that water boiled at 34°, that tin melted at 72°, etc., and, in fact, he measured a number of temperatures. To remedy the inconvenience of the comparatively small dilation of oil, Amontons, a French physicist, returned to the air-thermometer, which he constructed of about 4 feet in length, but which had the disadvantage of requiring its reading to be corrected by a barometrical observation. But Amontons had found that the temperature of boiling water was constant, and he selected this as a fixed point on his scale. But the first practically useful and accurate thermometers were constructed by a scientific instrument maker of Dantzig, named GABRIEL FAHRENHEIT (1686—1736). He at first used alcohol as the liquid with which to fill his tubes; but afterwards adopted quicksilver as in almost every respect preferable. The lower fixed point on his scale was the point at which the mercury stood in the tube when plunged into a mixture of sal-ammoniac and snow, in what proportions are not stated; but he appears to have considered this as the lowest temperature that could be obtained. The higher point was that at which water boils. The interval between these he divided into 212 equal parts, a number which appears arbitrary, but which he was induced to select possibly because the melting-point of ice and the temperature of the human body, two other important fixed temperatures, would fall at the exact number of degrees 32 and 96: most of the thermometers he made were not graduated beyond 96°. Fahrenheit's thermometer was immediately adopted in England and Germany, and is still in common use in those countries. But when REAUMER proposed the temperature of melting ice for the lower point in the scale, and that the division of the interval of temperature up to boiling water should be divided into 80 degrees, his scale was adopted by France and Russia. Finally, in 1741, CELSIUS, a professor at Upsal, divided the same interval into 100 equal parts, and this Centigrade scale superseded Reaumer in France, and is now that preferred all over the world for scientific observations.

The world being now in possession of a scientific instrument for the measures of temperatures, it was not long before it was turned to account in the discovery of the true laws of heat, as a short account of the experiments of Black will show. JOSEPH BLACK (1728—1799) was born at Bordeaux, his father, who there engaged in commerce, being a native of Belfast, but of Scottish descent. At twelve years of age Black was sent to a grammar school at Belfast, and, after four years spent there, to the University of Glasgow. Here he became so fond of the study of chemistry that Dr. Cullen, who then taught that subject, found in him an enthusiastic assistant; but Black, having chosen the profession of medicine, removed to Edinburgh to finish his education. Some academical controversies here directed his attention to certain unsolved problems in chemistry, and the result was the masterly investigations by Black which will be subsequently described. In 1756 Black was appointed the successor of Dr. Cullen in the chair of anatomy and chemistry at Glasgow. The duties of the former, however, he exchanged for the professorship of medicine, and he soon acquired fame in his professorial capacity, and added a lucrative medical practice to his other emoluments. It was between 1759 and 1762 that he completed those important researches concerning heat, which laid the foundation of all our scientific knowledge of this subject, and which enabled Watt soon afterwards to produce the steam engine, surely the greatest present that Science ever made to Industry. Black continued for thirty years to occupy his professorial chair in Glasgow His lectures were so clear, and, from all absence of affectation, so pleasing, that his class-rooms were always crowded. In his seventy-first year Black died as he sat at meat, and so peacefully, without a moment's illness, that his servant supposed he had fallen asleep with the basin of milk in his hand unspilled. The manner of his departure was, indeed, of the kind he had often wished it might be. The investigations which have made Black's name famous were completed before he had reached his thirty-fourth year; yet he left it to others to explore the field he had opened out, and devoted the rest of his life to perfecting his courses of lectures as professor of chemistry in the University of Glasgow. The researches of which we have to treat in the present chapter were not published in a separate form, but were incorporated into Black's professional lectures, and in this way became known to the world.

The thermometer, it should be observed, does not really directly measure heat, but temperature. That which it actually indicates is the change of volume which the mercury undergoes in changing its temperature; or, to be quite accurate, it is the *difference* of the expansion and contraction of the mercury and the glass which contains it. Altogether apart from any theory of the nature of heat, it is obviously true that a certain quantity of it must be required to raise the temperature of 1 lb. of water from, let us say, 40° to 50°, and that to raise 2 lbs. of water from 40° to 50° *twice* as much heat will be required, and to raise 3 lbs. *three* times as much, and so on. We thus begin to get the idea of heat as a *quantity*, and we may proceed to ask such a question as this: Is the same quantity of heat required to raise the temperature of 1 lb. of water from 50° to 60° as to raise it from 40° to 50°? There are many persons who, seeing that 10° is in both cases the range of temperature through which the same weight of water has to be raised, would confidently answer that the same quantity would be required. But it is not the method of science to jump at conclusions in this way. But when 1 lb. of water at 60° is mixed with 1 lb. of water at 40°, it is actually found that the temperature of the resulting mixture is 50°.

And as the quantity of heat which the hotter water parts with raises the other from 40° to 50°, it follows that the same quantity of heat which raises the temperature of 1 lb. of water from 40° to 50° will raise it from 50° to 60°. The like experiment may be tried for other temperatures between 32° and 212°, and the results always show that the quantities of heat required to raise the temperature of a given weight of water are proportional to the number of degrees through which the temperature is raised, the temperature of a mixture of equal weights of water at different temperatures being always the mean. But if we mix 1 lb. of water at 150° with 1 lb. of turpentine at 50°, the resulting temperature of the mixture will be not the mean, but 120°; therefore the heat lost by the water suffices to raise the same weight of turpentine through nearly double the range of temperature, for the experiment shows the same quantity of heat which would raise 1 lb. of water 30°, would raise 1 lb. of turpentine 70°. The difference of the effects produced by the same quantity of heat is still greater in the case of mercury and water. If 1 lb. of mercury at 66° were shaken up with 1 lb. of water at 100°, the resulting temperature would be 99°; hence, the quantity of heat which would raise 1 lb. of water from 99° to 100°, raises 1 lb. of mercury from 66° to 99°. Here, then, are instances of the same quantity of heat producing very different temperatures, and from these it is obvious, also, that to produce the same increase of temperature in different bodies different quantities of heat are required. Thus, the last case shows that, to raise the temperature of a given weight of water 1°, requires thirty-three times as much heat as suffices to raise the temperature of the same weight of mercury 1°. Black expressed the differences of bodies in this respect by saying each substance has its own special *capacity for heat.* We may compare the difference with respect to heat between 1 lb. of water and 1 lb. of mercury, to the difference between two vessels, one of which holds thirty-three times as much as the other. The phrase *specific capacity for heat* does not here necessarily involve any particular views as to the nature of heat; but the phrase was also applied about this period to another class of facts, for the rise of temperature observed in bodies when submitted to pressure was attributed to the *decreased capacity* of the body *for heat.* Heat was thus regarded as some subtle fluid which filled the pores of bodies, and was squeezed out by pressure, just as pressure causes the water to ooze from a saturated sponge. The term *specific capacity for heat* has been superseded by the shorter one "*specific heat*," which is altogether free from any theoretical suggestion.

Black did not himself follow out the path of research he had opened, but for the most part left the determination of the numerical values of the *specific heats* of bodies to his friends and disciples. One of the methods employed in such determinations has been exemplified, namely, the method by mixtures. This method is not here presented as that by which Black originally observed the specific heats, but as giving the simplest idea of the subject. The method employed by Black in his own determinations of specific heats will be better understood after an explanation of another cognate discovery of his, which was announced shortly after the observations on specific heat had been made public. If two precisely similar vessels filled, one with ice-cold water at 32° F., and the other with actual solid ice, be brought into a warm room, the thermometric indications, though originally the same (*i.e.*, 32°), soon exhibit this difference :—the temperature of the cold water begins at once to rise, and in a short time (say an hour) the water may have attained nearly the same temperature as the surrounding atmosphere : while, on the other hand, no rise of temperature will be perceptible in the vessel containing the ice so long as any of the ice remains unmelted, and the thermometer will constantly record 32° for several hours. As the vessels are similar and equal, they must receive equal quantities of heat in the same time ; yet, while the thermometer in the water-vessel has risen perhaps 20°, that in the ice-vessel gives no indication whatever of the heat which has been communicated to that vessel. What, asked Black, becomes of the heat which has been all along given to the vessel of ice ? His reply was that the heat had in some way been expended in converting the ice into water : of this heat the thermometer gives no indication ; but, although it has not shown itself as temperature, it is nevertheless existent in a *latent* form. For if, when the whole has become changed into ice-cold *water* at 32°, the vessel is exposed to cold again, the water at 32° is not immediately reconverted into ice, but must first part with all its *latent* heat. The amount of this heat which becomes *latent* when ice is converted into water may be measured. If 1 lb. of dry pounded ice at 32° be mixed with 1 lb. of water at 175° F., the result will be 2 lbs. of water at 32°; that is, the pound of ice will, in this case, be converted into water without any gain of temperature, but the heat which becomes *latent* in the process is abstracted from the pound of water originally at 175°, the temperature of which is thereby reduced to 32°. The heat, therefore, which is required to convert 1 lb. of ice at 32° into water also at 32°, would, if applied to 1 lb. of water, raise its temperature 143°; and as the quantity of heat required to raise 1 lb. of water 1° is taken as the unit-quantity, *the latent heat of ice* is 143 heat-units. *Not* 143 *degrees*, be it noted, but so much heat as would raise 143 lbs. of water 1° F., or so much as would raise 1 lb. of water 143° F.

Black found that in all general cases, whenever a solid body is converted into a liquid, a certain quantity of heat becomes latent. If a crucible containing a quantity of tin filings be gradually heated, a thermometer placed amidst the tin will gradually and steadily rise until it marks 442° F., and then, although the crucible receives heat constantly, the thermometer will become stationary at 442° until all the tin has melted. The process of melting begins the moment the thermometer marks 442°, and no further rise of the thermometer takes place until the last particle of tin has been melted, when the temperature again begins to rise. The quantity of heat which thus becomes latent in changing a body from the solid to the liquid form is called its *heat of fusion*, and varies from one body to another: thus, while the conversion of 1 lb. of ice into water requires 143 heat-units, 1 lb. of solid tin is changed into 1 lb. of liquid tin by the absorption of about 26 units of heat.

When a pan of cold water is set on a fire, a thermometer plunged in the liquid will continue to rise until it reaches 212°, at which point the water begins to boil, and the thermometer ceases to rise. As heat is, nevertheless, constantly passing into the vessel, it must become *latent* in this case also; and Black, who first studied this subject, found that this latent heat is employed in converting the water at 212° into steam, which possesses no higher a temperature. The steam must have this latent heat extracted from it before it resumes the liquid form. The principle of the methods by which the latent heat of steam may be estimated is very simple. Let A, Fig. 137, represent a vessel containing a known weight of water, which we shall suppose to be 11 lbs. Into this water dips the pipe C, communicating with the vessel B, which contains water that is made to boil by the application of heat. The steam passing along the pipe C comes into contact with the cold water in A, and is there condensed into water, which, of course, mixes with the rest of the liquid. The temperature of the liquid rises gradually until it reaches 212°, after which the steam entering from C ceases to be condensed, but rises up through the liquid in bubbles and escapes, just as common air would do if forced through the pipe. If, at the moment this first occurs, the process be stopped, and the water in A be weighed, the weight will be found increased: thus, if 11 lbs. were the original weight of water at 32°, the weight now would be 13 lbs., or 11 lbs. of water at 32° mixed with 2 lbs. of *steam* at 212°, produce 13 lbs. of *water* at 212°; so that the 2 lbs. of steam will change into water without diminution of temperature; but in the changing of its condition from gas to liquid, it parts with sufficient heat to raise 11 lbs. of water from 32° to 212°. Hence

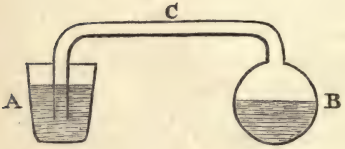

FIG. 137.

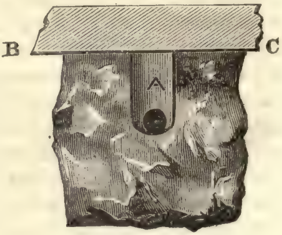

FIG. 138.

1 lb. of steam contains as much *latent heat* as will raise $5\frac{1}{2}$ lbs. of water 180° F.; that 990, or $180 \times 5\frac{1}{2}$, is as much as will raise 1 lb. of water 1°: 990 is, therefore, the *latent heat of steam.*

Black was able to determine *specific heats* with accuracy by a method which has never been surpassed for directness and simplicity. He procured large blocks of pure ice, and after having ground one side of a block perfectly level, he formed a cavity in the mass, as shown at A, Fig. 138. A lid was formed by giving a plane surface to another piece of ice, B C; and it would thus be impossible for any heat to reach the cavity from the outside; for supposing that any air of a temperature above 32° should have penetrated between the block of ice and its cover, the excess of heat of this air would be expended in converting some of the ice into water. When a determination of the *specific heat* of a body was about to be made, the body was raised to some accurately determined temperature, and in a perfectly dry state was quickly introduced into the cavity A, which had been previously wiped with a dry but ice-cold cloth. The lid was then applied, and the apparatus was allowed to remain for several hours, when the warm body having parted with its excess of heat, the cavity contained a quantity of *water* at 32°, the *latent heat* of which represented the amount of heat which the body had parted with in falling from its original temperature to 32°, and therefore its *specific heat* could be found by determining the weight of the cold water. Black accomplished this by wiping up and absorbing the whole of the water by a dry ice-cold piece of linen which had been previously weighed. The increase of its weight was of course the weight of water it had absorbed.

The researches of Black may be said to have created the science of heat. There had before been abundance of speculations on the nature of heat, and observations were not wanting of the changes it produces; but Black first introduced the means of measuring heat as a quantity, and from that period heat measurements have contributed some most important data to many branches of science. The mode of measuring heat which has just been explained is quite independent of all theoretical views as to its nature. We have already seen that Bacon guessed that heat might be an insensible motion of the small particles of bodies (page 136); but at the end of last century the prevalent conception of heat was that of a kind of subtle imponderable fluid, which was capable of combining with substances in greater or less proportion. It was supposed to enter into "the pores" of bodies, which received it in greater or less proportion, and from which it could even be, as it seemed, squeezed out by compression. This suppositious fluid received the name of *caloric*, and the notion and the name survived until a comparatively recent time, and are found in most scientific treatises published during the first half of the nineteenth century.

The method of estimating specific heats by the quantity of ice which a body is able to melt in parting with its excess of temperature above the freezing-point has been much employed. Laplace and the celebrated French chemist, Lavoisier (whose chief scientific work will be discussed in a subsequent chapter), devised an apparatus in which Black's method was used, modified by the circumstance that large blocks of pure ice such as those used by Black could not be easily obtained in many localities. Fig. 139 represents this apparatus, which Lavoisier, with more regard to convenient distinction than to etymological elegance, called the *Calorimeter*. The substance the *specific heat* of which is to be determined is contained in a kind of cylindrical cage formed of iron wire, F F, and surrounded by small pieces of ice contained in a middle cavity, *d d*, from which at the end of the experiment the water is drawn off at *x* into a suitable vessel, and weighed. The ice contained in *d d* is shielded from receiving external heat by means of an outer cylindrical cavity *a a*, which is filled with pounded ice. The water produced by the melting of the ice in *a a* is allowed to drain off at *x*. There is also a lid, B B, containing a stratum of pounded ice. Thus in an apartment where the air is not below but a little above the freezing-point, experiments may be conducted with this apparatus without the internal cavity receiving any heat from without. When a liquid is the subject of experiment it is enclosed in a glass or other vessel, the specific heat of which has been previously ascertained, in order that from the quantity of water collected the amount due to the containing vessel may be deducted. To determine the quantity of heat disengaged during combustion or other chemical action, the bodies are burnt or combined in the interior cavity after their temperature has been reduced to 32°. The apparatus also admits of estimates being made of animal heat produced by respiration. For this last kind of experiment a guinea-pig, an animal which does not suffer from the cold, is introduced into the interior cavity, and supplied with air cooled to 32° by means of one pipe, while another pipe carried spirally through

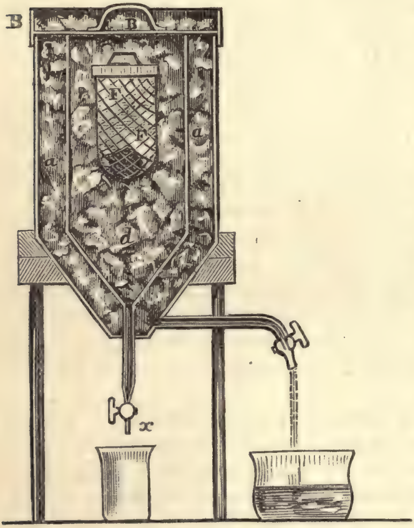

FIG. 139.

the ice in the interior cavity carries off the vitiated air, thus deprived of all its heat.

The two *savants* whose names occur in the foregoing paragraph were the first who succeeded in making accurate measurements of the amount of expansion which is produced in solids by a given rise of temperature. The increase of bulk which heat occasions in solids is too small to be recognized by the eye; but by such apparatus as that shown in Fig. 140 it is customary to demonstrate the fact in lectures on heat. A B is a cylinder of copper, which at ordinary temperatures exactly fits into the space A B, and passes through the circular opening E in the gauge C D; but when the cylinder is hot, it proves too large to enter these openings. The method of measuring the expansion which Lavoisier and Laplace adopted is an excellent illustration of one of the artifices often employed in the measurement of very small quantities. The bar of metal or other substance, C D, Fig. 141, was placed in a long metallic trough, A B, in which the bar rested on rollers, so that it might experience no resistance to longitudinal movement. One end of the bar abutted at C against an independent immovable stop, and the other end, D, was pressed by the end of a lever which turned on a horizontal axis E, that also carried a telescope. The telescope was provided with a cross-wire, and when the bar was at the temperature of the melting ice filling the trough, a vertical scale placed at a distance of 200 yards was viewed through the telescope, and the division of the scale cut by the line of sight E F was noted. By the application of heat the ice in the trough was melted, and the water raised to the boiling-point (212° F.). The bar by increase of temperature expanded and pushed out the end of the lever, thus turning the axis carrying the telescope through a small angle, so that the cross-

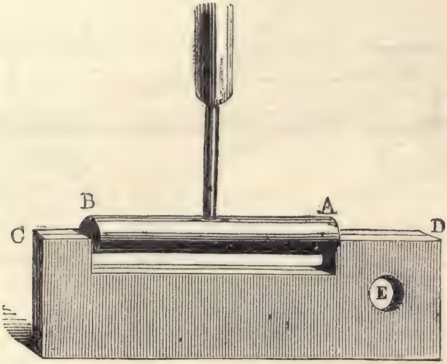

FIG. 140.

wire would then be seen intersecting some other division of the vertical scale, as at G. The angles DEH and FEG being of course equal, the distance DH would have the same proportion to FG that ED has to EF; and therefore DH, the amount of expansion, could easily be calculated from the known distances FG, EF, ED. In the apparatus actually employed by Lavoisier and Laplace, by the movement of the end of the bar at D the optical axis of the telescope was displaced on the scale FG through 744 times the distance DH. The expedient here adopted consists, it will be noted, in magnifying in a known proportion the quantity to be measured. Of course the proportion must be very accurately known; and these expedients often prove illusory from the difficulties of accurately determining this proportion. Thus, in the case before us one of the most essential points is to find the exact length of the lever ED. This is a difficult matter, and may introduce serious errors. A more direct mode of measuring the elongation of bars was practised by Ramsden. Three metallic troughs are fixed parallel to each other, as at A, B, C, Fig. 142. A and C contain bars of iron about 6 feet in length, and these bars are immersed in ice at 32° during both phases of the experiments. The middle trough contains a bar of the substance of which the expansion is to be ascertained, and the ends of the three bars are provided with certain optical and mechanical arrangements, which are partly displayed in Fig. 143. *f* and *i* are attached tubes, containing a lens which acts like the object-glasses of microscopes; while *g* and *k* contain the corresponding eye-pieces, each provided with cross-wires. At *e* and *h* rings are mounted containing cross-wires to serve as fixed marks to be viewed through

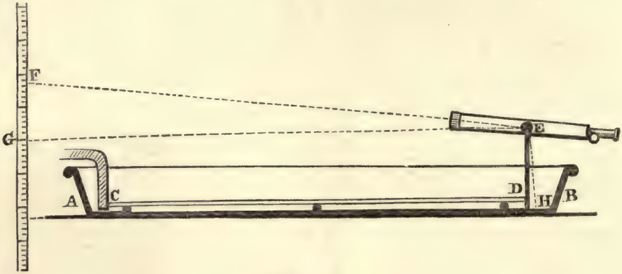

FIG. 141.

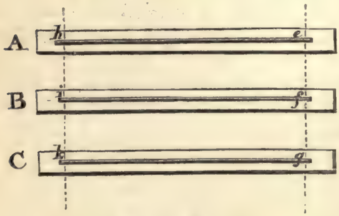

FIG. 142.

the microscopes. Let *e, h, g, k* be supposed to have been permanently adjusted, and tne object-glass *f* to have been brought into the position in which the image of *e* coincides with the-cross wire of the eye-piece. This is accomplished by means of a screw, which gives a longitudinal motion to the whole bar in the middle trough. The object-glass at *i* can be moved longitudinally along the bar by means of a screw of known thread, that is, the amount of longitudinal motion due to a turn of the screw is known, and parts of a turn are indicated on a graduated circle. When all the three bars are in melting ice, this screw is so adjusted that the optical axes of both microscopes coincide with *e* and *h*. The middle trough is next heated to 212°, the middle bar is, if necessary, adjusted to the original coincidence at *f*, and the number of turns which must now be given to the screw at *i* to bring *h* into coincidence again, indicates the elongation of the bar.

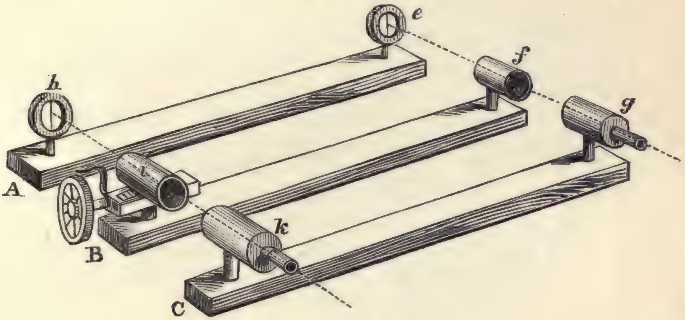

FIG. 143.

The principles of latent heat had no sooner been expounded by Black than they found a famous practical application in the hands of the illustrious WATT (1736—1819), whose steam-engine has changed the whole world of arts and manufactures more than any other invention that has ever been made. It is, of course, quite possible that the steam-engine would have been hit upon, even if Black had never enunciated a single proposition concerning latent heat. But we can hardly doubt that Black's doctrine must have guided Watt in his invention. Watt was certainly acquainted with the course of physical and chemical instruction given at the University of Glasgow, for he was the officially appointed instrument maker to the university, and in frequent communication with Black, and with Anderson the professor of physics. The latter had sent him a working model of Newcomen's pumping engine, with a view of having its defective action remedied, if possible. Watt soon rectified the defects, and the apparatus was regularly made to work in the class lecture-room before the students. But Watt was not content with his success, for his mind had been directed to many points concerning the theory of the machine, and he had ascertained many facts relating to that theory. He made experiments to discover the proportion in which water is expanded by conversion into steam; the quantity of water that is vaporized by a given weight of coal; the quantity of steam expended at each oscillation of Newcomen's engine; the quantity of cold water that must be thrown into the cylinder to cause a certain effective force to operate on the descending piston; and, lastly, he determined the elastic force of steam at various temperatures. It appears to have been about the year 1763 or 1764 when Watt's attention was first directed to the improvement of the steam-engine, but it was not until 1774 that he succeeded in making a model to work satisfactorily. To describe all the steps by which Watt brought the steam-engine to perfection, and the beautiful mechanical combinations he devised, would require a separate treatise, while but a short notice of the main improvements is all that can here be given. The several machines previously projected or constructed, in which steam appeared as the agent, do not in general require any special description here, as with two exceptions they did not prove successful in practice. We may pass over Hero's machine and the controversies of Solomon de Caus, of the Marquis of Worcester, and of Sir Samuel Morland. DENIS PAPIN, a Frenchman, who however was living in England at the time he was engaged in his experiments, conceived the idea of creating a vacuum by means of steam beneath a piston moving in a cylinder. His plan was to place a small quantity of water in the bottom of his cylinder, which, when heated, converted some of the water into steam, and this, by its expansive force, raised the piston against the pressure of the atmosphere. This done, the fire was removed and the cylinder allowed to cool, when the steam which filled it condensed again into water, and in this state, taking up only a small portion of the space of the steam, left a vacuum which, opposing nothing to the weight of the atmosphere, allowed the piston to be pressed down with a force which would be nearer to about 15 lbs. on the square inch in proportion to the perfection of the vacuum within the cylinder. Not only was the notion of using a piston new in the application of steam, but the still more remarkable novelty was the condensation of the steam by cold as the means of obtaining the effective force from the pressure of the atmosphere.

There is another contrivance, for which we are indebted to Papin. As already stated (p. 290), when heat is applied to an *open* vessel of water the temperature will rise to 212° F., at which point it will remain stationary until all the water has been converted into steam at 212°, the steam carrying off in the form of latent heat, the heat passing into the vessel. If, however, the escape of the steam be prevented by completely closing the vessel, the temperature of the

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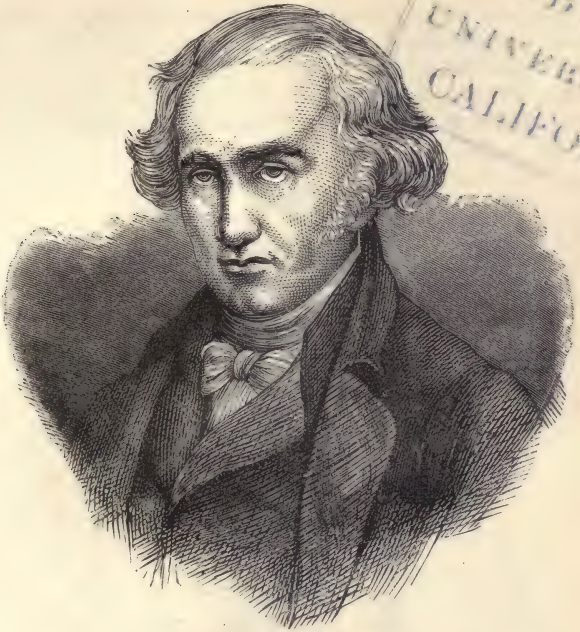

FIG. 144.—JAMES WATT.

water will rise; but at the same time the pressure, tension, or elastic force of the steam within the vessel will increase. At 212° this tension is able to balance the pressure of the atmosphere, that is, it is equal to about 15 lbs. on every square inch of surface. Hence if a closed vessel containing water is heated to 212°, every square inch of its internal surface is pressed outward with this force. But if the temperature be raised to 250 F., the pressure becomes 30 lbs. per square inch. At 300° F. the pressure is about 67 lbs. per square inch; at 358° it is 150 lbs.; and so on. Papin contrived the apparatus represented in Fig. 145 for the purpose of heating water to a higher temperature than 212° F. It consists of a strong iron vessel O, provided with a lid, which can be forcibly fastened down by the screw shown in the figure. The object of raising the temperature of the water is to increase its solvent powers for culinary or other purposes. To prevent the vessel from exploding by undue tension of the steam within, Papin contrived the *Safety-Valve*. This is simply a metallic plug, *s*, which closes an opening in the lid of the vessel, and could be readily lifted up from within, were it not pressed down by the lever L, upon which the weight P acts. When the tension of the steam upon the plug exceeds the pressure of the lever the plug is raised, and permits the excess of steam to escape. It is curious that the safety-valve was employed by Papin for this apparatus, which is still called "Papin's Digester;" and yet he did not propose its use for steam-boilers, nor was it in fact used for this purpose until many years afterwards.

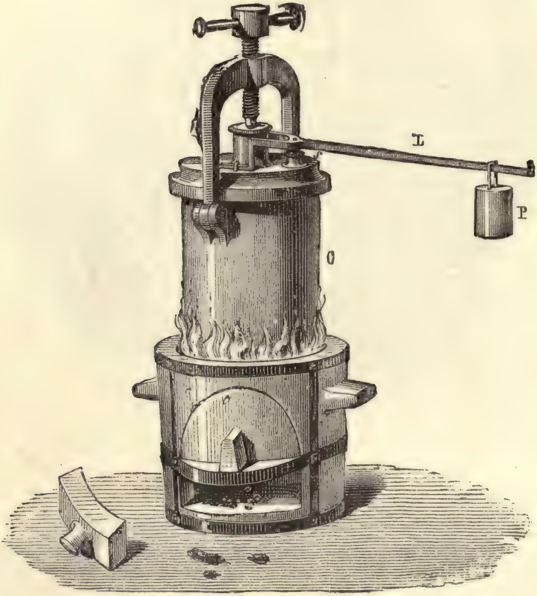

FIG. 145.

We come now to the engine invented by CAPTAIN THOMAS SAVERY in 1698; and his apparatus was perhaps the first practical one of steam power in any form, for Papin did not get beyond the construction of small models. Savery's arrangement was actually successfully and extensively employed in raising water from mines, etc. His method was simply to create a vacuum within a capacious vessel by first filling it with steam, and then condensing that steam by cold. A pipe connected the vessel with the water to be raised, which was forced up by the atmospheric pressure exactly as in the common pump. He also employed the tension of steam acting directly upon the water to force it up to a higher level.

In 1711 THOMAS NEWCOMEN, an ironmonger, and JOHN CAWLEY, a glazier, both of Dartmouth, perceiving the defects of Savery's arrangement, constructed an engine mainly upon the principle formerly proposed by Papin. This engine, as being the immediate forerunner of Watt's engine, requires a word of explanation. Its mode of action may be gathered from Fig. 146, in which *a* represents the boiler, from which steam passed into the cylinder *c* through the opening *e* when

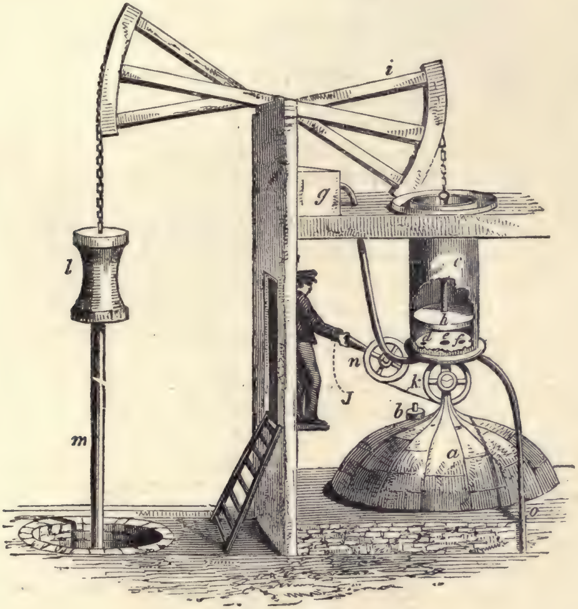

FIG. 146.—NEWCOMEN'S ENGINE.

the cock *k* was opened, which was originally done by hand, as the piston *h* was drawn up by the weight of the pump-rod *m* and counterpoise *l*, attached to the other end of the beam. In the lower part of the cylinder, which in the figure is shown as if partly broken away, in order to exhibit its interior, were two other apertures, one of which, *d*, communicated with a cistern, *g*, of cold water, which being admitted into the cylinder when the latter was full of steam and the communication with the boiler closed, caused the condensation of the steam. It will be seen that here again the steam is simply the means for readily obtaining a vacuum, in consequence of its property of condensing into water by the application of cold. Any desired power could be obtained by this machine, not by increasing the tension of the steam, but by enlarging the diameter of the cylinder. The great defect of this machine was, that after the piston had descended and the water had been forced out of the cylinder by the exit-pipe *o*, the cylinder itself had necessarily been cooled, partly by the injected water, and partly by its interior becoming exposed to the contact of the atmosphere; for the cylinder was quite open at the end. Hence on the readmission of the steam a large quantity was condensed by the cold cylinder, until the latter had acquired the temperature of 212°. When Watt began the study of Newcomen's engine, with a view of employing steam in a more effective manner, he proceeded, as already stated, in a truly scientific method, by first ascertaining all the properties of steam. Having by a long series of experiments learnt the qualities of the agent with which he had to work, he considered the mode of its application; and he found that of the total quantity of steam required to work Newcomen's engine, three-fourths was expended in re-heating the cylinder. Therefore if the cylinder, instead of being cooled at each stroke, could be kept constantly hot, a fourth part of the steam, and consequently a fourth part of the fuel hitherto used, would suffice. Besides all this, the heated cylinder always converted some of the injected cold water into steam, which diminished the force effecting the descent of the piston by about one-fourth, even in the best of Newcomen's engines.

Watt thought over the means of overcoming these defects, and a simple but beautifully effective plan presented itself to his mind. He proposed to connect another vessel with the cylinder by means of an open pipe, and when the cylinder and the vessel were full of steam, to inject cold water into the vessel, when, a vacuum being produced there, more steam would rush in from the cylinder, and would be condensed in its turn, and so on so long as any remained. Thus the required vacuum would be produced without a single drop of cold water coming into contact with the cylinder. The invention of the separate *Condenser* was the grand improvement which Watt effected in the steam-engine; but there was another of first-rate importance, and that consisted in preventing all access of the atmosphere to the interior of the cylinder. The cold air, if allowed to urge the piston, would of course cool the interior walls of the cylinder, and loss of much steam would result. Watt substituted the tension of the steam from the boiler for the atmospheric pressure, and there was always steam on one side of the piston, a vacuum on the other. The top of the cylinder was no longer open as in Newcomen's engine, but was closed by a steam-tight cover, a hole in the centre of which admitted free passage for the piston-rod, while the passage of air or steam was prevented by packing very tightly round the opening with greased tow. The general arrangement of the double-action engine as perfected by Watt is shown in the diagram, Fig. 147, where A is the cylinder, I the condenser, J a pump worked by the engine for drawing the injection water and condensation water from the condenser into the *hot well*, *j*, where it is supplied to the force-pump M to feed the boiler. The comparison of Fig. 146 with Fig. 147, in conjunction with the remarks made above, will, it is hoped, enable the intelligent reader to clearly perceive the principles by which Watt converted the mere draining-pump of Newcomen into the steam-engine.

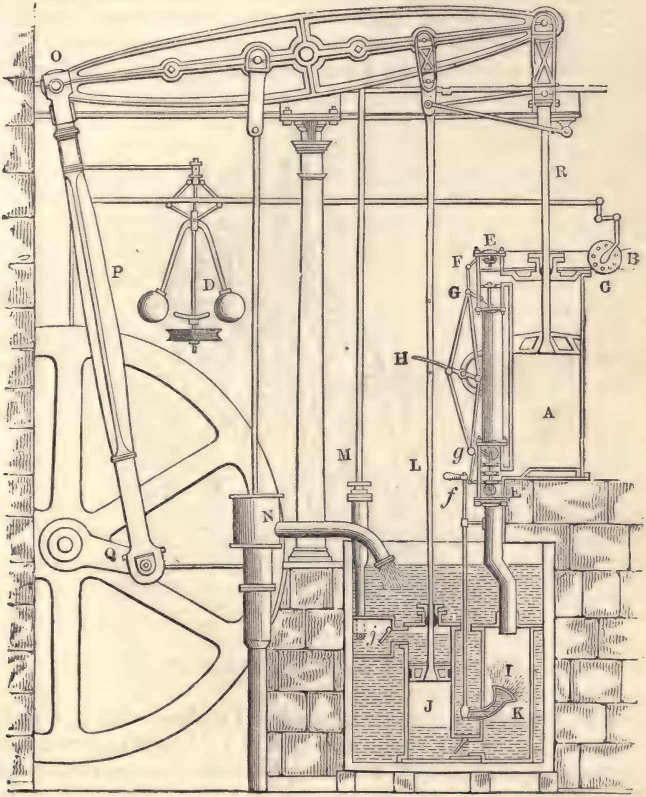

FIG. 147.—WATT'S ENGINE.

The story of successful inventions and continued prosperity which marked Watt's career from the period (1774) when he became connected with Boulton in the gigantic manufacturing establishments at Soho near Birmingham, has been told too often to require recapitulation here. Watt retained his intellectual faculties to the last. In the last months of his life he was occupied in devising a machine for copying statuary with mathematical precision. Some examples of the work of this machine were facetiously called, by the illustrious inventor himself, "the first attempts of a young artist entering the eighty-third year of his age." Watt died peacefully at his house at Heathfield near Birmingham on the 19th of August, 1819, and was buried in the parish church at Handsworth. In this church the statue represented in Fig. 148 was shortly afterwards erected to his memory, and at other places also statues of the great inventor were erected. A seated portrait of Watt in marble by Chantrey may be seen in Westminster Abbey, supported on a pedestal bearing the following inscription:—

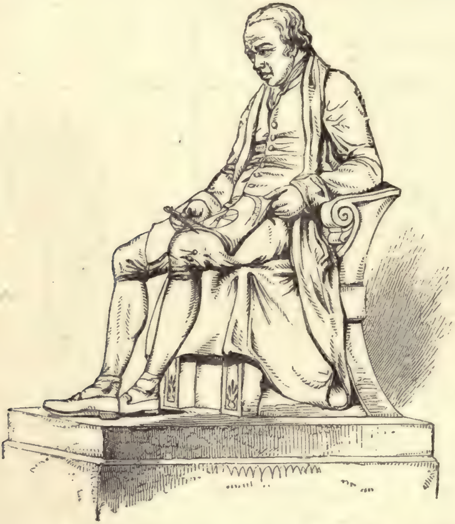

FIG. 148.—STATUE OF WATT IN HANDSWORTH CHURCH.

"Not to perpetuate a name which must endure while the peaceful arts flourish, but to show that mankind have learnt to honour those who deserve their gratitude, the king, his ministers, and many of the nobles and commoners of the realm raised this monument to JAMES WATT; who, directing the force of an original genius, early exercised in philosophic research, to the improvement of the steam-engine, enlarged the resources of his country, increased the power of man, and rose to an eminent place among the most illustrious followers of science and the real benefactors of the world. Born at Greenock, MDCCXXXVI.; died at Heathfield, MDCCCXIX."

The character of Watt and the powerful influence of his great invention are indicated in the few following sentences, which have the more interest as coming from the pen of a man not less famous in literature than his countryman was in science:—"It was only once my fortune to meet him, whether in body or in spirit it matters not. There were assembled about half a score of our Northern Lights. . . . . Amidst the company stood Mr. Watt, the man whose genius discovered the means of multiplying our natural resources to a degree perhaps even beyond his own stupendous power of calculation; bringing the treasures of the abyss to the summit of the earth; giving the feeble arm of man the momentum of an Afrite; commanding manufactures to rise as the rod of the prophet produced water in the desert; affording the means of dispensing with that time and tide which wait for no man, and of sailing without that wind which defied the commands and threats of Xerxes himself. This potent commander of the elements, this abridger of time and space, this magician whose cloudy machinery has produced a change on the world the effects of which, extraordinary as they are, are perhaps only now beginning to be felt, was not only the profound man of science, the most successful combiner of powers and calculator of numbers as adapted to practical purposes, was not only one of the most generally well informed, but one of the best and kindest of human beings. There he stood, surrounded by the little band I have mentioned of northern *literati*, men not less tenacious, generally speaking, of their own fame and their own opinions than the national regiments are supposed to be jealous of the high character which they have won upon service. Methinks I yet see and hear what I shall never see or hear again. In his eighty-third year the alert, kind, benevolent old man had his attention at every one's question—his information at every one's command. His talents and fancy overflowed on every subject. One gentleman was a deep philologist—he talked with him on the origin of the alphabet as if he had been coeval with Cadmus; another, a celebrated critic—you would have said the old man had studied political economy and *belles-lettres* all his life; of science it is unnecessary to speak—it was his own distinguished walk."

The vast revolution which the steam-engine has wrought in commerce, in industry, in arts, and in social relations, is not a theme that need be dwelt upon here. Everybody knows how it propels our ships, draws our carriages, and labours incessantly in countless mines and workshops and mills, in carrying, lifting, pumping, sawing, hammering, weaving, printing. As the greatest of mechanical inventions, it has had an immense influence on the progress of science; for in no other way than by the perfection of mechanical appliances could the investigators of the present century have been provided with those refined and accurate instruments upon which the accelerated progress of recent times has largely depended. Perhaps the difference between the mechanical workmanship of last century and that of our day cannot be better illustrated than by a circumstance belonging to the history of the steam-engine itself. It is recorded that one of Watt's great difficulties in the construction of his earlier engines was to obtain cylinders bored with sufficient accuracy to prevent the steam from escaping at the edge of the pistons. The amount of mechanical precision which is met with, even in the most ordinary machines and scientific instruments of the present day, is more striking when considered in connection with the circumstance just mentioned.

Not greater was the revolution in industry effected by the steam-engine than that which has been brought about in the theoretic aspect of science by the *dynamical theory of heat*, which about the same period first took a definite form. Its inception must be associated with the name of a man otherwise remarkable in several ways, and deserving of mention in these pages if he had done nothing beyond founding the Royal Institution of Great Britain. BENJAMIN THOMPSON was born in 1753, in a village of Massachusetts, U.S., called North Woburn. His birthplace is but twelve miles distant from that of another Benjamin, born half a century earlier, whose discoveries will claim our attention in the next chapter. From the age of thirteen to that of eighteen Thompson acted as assistant in what our transatlantic cousins call "dry goods stores." He then became successively a student of medicine and a schoolmaster, and before he had reached twenty years of age he had married, at a village then called Rumford but afterwards named Concord, a widow with ample means. It is doubtful whether the marriage would have been a felicitous one, as his wife was by many years his senior. A separation, however, soon took place, on account of the disturbed condition of affairs brought about by the struggle then impending between England and her great colony. Thompson had been appointed major in a militia regiment, and he fell under the suspicion of "being unfriendly to the cause of liberty." In November, 1774, a mob assembled round his dwelling, and with hootings and hisses demanded him to appear. He had, however, secretly left Concord, and taken refuge at Boston. He was arrested, and examined by the Revolutionary Committee on the charge already named. Of this charge, however, he was fully acquitted, and set at liberty. He now devoted himself to the study of military tactics for a few months; but in the autumn of 1775 he was sent to England with despatches from General Howe to the British Government. In London he speedily ingratiated himself with the Secretary of State for the Colonies, and in May, 1776, he was appointed to an under-secretaryship. He had now finally quitted his native country, leaving behind his wife, whom he never met again, and an infant daughter, whom he saw only after twenty-two years had elapsed. In London Thompson carried on many experiments on gunpowder, on one occasion sailing with the fleet, to make himself acquainted with the firing of heavy guns. In 1779 he was admitted Fellow of the Royal Society, and having still retained his military commission, he was promoted to the rank of colonel in the British army. In 1783 Thompson travelled on the Continent, visiting among other places Vienna and Munich.

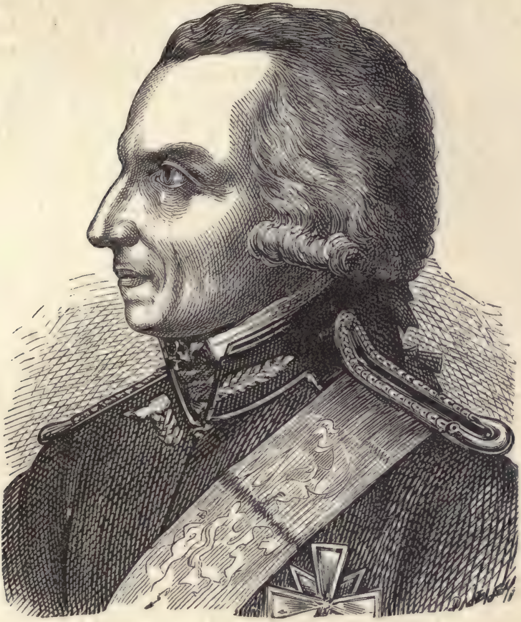

FIG. 149.—COUNT RUMFORD.

At the latter capital the Duke of Bavaria recognized the special aptitude and ability of the ex-"dry goods" dealer, and invited him to enter his service. George III. accorded his permission, and sent Thompson away with the honour of knighthood. Thompson's career at Munich is perhaps the most singular part of his life, for the story of his administration of public affairs there reads more like the recital in an Arabian tale of a course of beneficent legislation enacted and carried out by some wise grand vizier, than the records of ordinary statesmanship. Not less remarkable also are his proceedings from the fact that in the midst of his labours and enterprises he should have carried on the continuous study of science, philosophy, and economics. During his eleven years' sojourn in Munich Thompson published papers in the "Transactions of the Royal Society" on heat, dephlogisticated air, moisture in the atmosphere, photometry, etc., etc., and contributed also to the journals of other scientific bodies.

The Duke of Bavaria at first appointed Thompson colonel of a cavalry regiment and to be aide-de-camp to himself, and a palatial residence was assigned for him. He rapidly acquired the German and French languages, and was made a privy councillor of state. He was advanced to the rank of general, and placed at the head of the War Department. In 1791 he was raised to the dignity of Count of the Holy Roman Empire, and on this occasion he selected as his title the name of the village where fortune had first smiled upon him. The offices held by Sir Benjamin Thompson included nominally the chief administration of the War and Police Departments of the Electorate of Bavaria, and he was also the Chamberlain of the Elector. In fact, he virtually exercised control over every branch of the public service. Let us now see some of the uses he made of this power. The Munich of those days was a very different place from the handsome city whose artistic treasures and embellishments now attract tourists to the banks of the Iser. The industry of the country was saddled with the maintenance of a disproportionate standing army, and in the enforced idleness, oppressive and useless discipline, and harsh treatment of the soldiery, Thompson recognized the source of great social evils. He made the position of the soldier as pleasant as possible, increased his pay, improved his clothing, allowed him all liberty consistent with military discipline, gave him quarters neat, clean, and attractive. Schools were established in all regiments. As the best of all means of mitigating the evils attending a standing army, he aimed at "making soldiers citizens, and citizens soldiers." The men had raw material furnished them, and were at liberty to dispose of the produce of their labours when off duty. They were also employed on public works, and they were permitted to engage in agricultural labours to a certain extent. It was found that the soldiers could by their organized civil labour earn three times the pay the State could afford to give them, and that discontent and disorder no longer prevailed.

More striking was Thompson's suppression at one vigorous and decisive blow of the enormous system of mendicity which had long heavily taxed the industrious part of the community. Beggars and vagabonds of all ages, sexes, and nationalities swarmed in all parts of the country, levying contributions by importunities, threats, and thefts. The roads seemed to a traveller to be lined with extended palms. Mendicity became a profession, and the beggars formed in the cities an organized caste with assigned beats and districts. The account of the effective, wise, and benevolent measures which Rumford planned for at once putting an end to these evils, forms perhaps the most interesting pages in the memoir of the Count, which was not long ago published in connection with a complete edition of his works, prepared under the auspices of the American Academy of Arts and Sciences. To relate the manner in which these measures were carried out would lead us too far from our subject, but of their success the reader may judge from the inscriptions on the monumental memorial to Rumford which is set up in the park or "English Garden" at Munich. This park was formed by Rumford himself from a dreary and neglected spot on the north-east of the city. The inscription on one side of the monument commemorates this circumstance by inviting the passer-by to heighten his enjoyment by gratitude. "A suggestive hint," it continues, "of Charles Theodore's, seized on with genius, taste, and love by Rumford, the friend of mankind, has transformed this once waste spot into what thou now seest around thee." On the opposite side may be read the following: "To him who rooted out the most disgraceful public evils—Idleness and Mendicity; who gave to the poor relief, occupation, and good morals, and to the youth of the Fatherland so many schools. Go, passer-by (*lustwandler*), and strive to imitate him in mind and action, and us in gratitude."

In 1795, after an absence of eleven years, Rumford returned to London to publish his essays. He endeavoured during his visit to interest the people of England in plans of public and domestic economy like those he had put into operation in Germany. Some of the best known of his schemes related to economy in the domestic consumption of coal, improvements in the construction of fireplaces, heating buildings by steam. He personally superintended the fitting-up of the kitchen at the Foundling Hospital in accordance with the plans he had advocated. No detail of household economy appeared to be too small for Rumford's advocacy, if it could be referred to some scientific principle. This circumstance imparts to his schemes of domestic reform, as it does to the great state and social reforms he effected, a peculiar interest. We have here not only an instance of science bearing Baconian fruit, but we see that the relations of men in societies had been admitted to form a science, and we witness the application in practice of its deductions.

In 1799 Rumford issued in London a prospectus entitled "Proposals for forming, by subscription in the metropolis of the British empire, a public institution for diffusing the knowledge and facilitating the general introduction of useful mechanical inventions and improvements, and for teaching, by courses of philosophical lectures and experiments, the applications of science to the common purposes of life." In the course of the pamphlet he remarks, "It will not escape observation that I have placed the *management of fire* among the very first subjects of useful improvement, and it is possible that I may be accused of partiality in placing the object of my own favourite pursuits in that conspicuous situation. But how could I have done otherwise? I have always considered it as being a subject very interesting to mankind, and it was on that account principally that at a very early period of my life I engaged in its investigation." He then shows that all arts and manufactures depend upon operations in which fire is employed; that fuel costs the country ten millions annually;* and that much of this may be saved. The result was the establishment of the Royal Institution of Great Britain, which was formally opened on the 11th of March, 1800, Rumford being its superintendent. Three months afterwards Dr. Thomas Young was engaged as Professor of Natural Philosophy. The Institution has been fortunate in the selection of its professors, for after Young there came Davy, and after Davy came Faraday, names which have shed a lustre on the Institution that is worthily maintained by the present occupier of the chair of Natural Philosophy—Professor Tyndall.

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* This has now increased to more than four times that amount.

In 1805 Count Rumford married Madame Lavoisier, the widow of the celebrated and unfortunate French chemist. This union proved an unhappy one, by reason of incompatibility of tastes and habits, and four years afterwards the parties separated. Count Rumford died in 1814 at Auteuil near Paris.

When Rumford was engaged in superintending the boring of cannon in the Government arsenal at Munich, his attention was arrested by the great amount of heat acquired by brass guns during the operation of boring. He was led to institute a number of experiments, the details of which are described in his essay entitled, "An Inquiry Concerning the Source of the Heat which is excited by Friction." The substance of this essay was contained in a paper read before the Royal Society in 1798. One case will suffice to acquaint the reader with the general nature of the experiments. A brass cylinder was made to revolve against a steel borer, the cylinder being placed inside of a wooden vessel, which also contained a determined quantity of water. The quantity of water used in one experiment described by Rumford was 18¾ lbs., and the temperature of the water, which was 60° F. at the beginning of the experiment, rose continually when the cylinder was set in motion, until at the end of 2½ hours the water actually boiled.

"It would be difficult," says Rumford, "to describe the surprise and astonishment expressed in the countenances of the bystanders on seeing so large a quantity of water heated, and actually made to boil, without any fire." Yet, of the ways in which heat can be artificially produced, that in which it is developed by friction is probably the most ancient. For the mode in which some uncivilized races still obtain fire may be taken as a true indication of the usages of prehistoric man in this respect. This method is by friction between two pieces of wood. The *modus operandi*, however, is not simply the rubbing together of two sticks held in the hand, but that shown in our illustration, and used, with slight differences, in very different parts of the world—for example, in Northern Asia, North America, Brazil, Australia, and Polynesia. This plan consists essentially in causing the end of a wooden rod to revolve very rapidly in a small cavity made in a piece of dry timber. The differences observed consist only in the ways in which the rapid rotatory motion is impressed upon the rod, which is, in all cases, pressed strongly against the piece of timber. The simplest

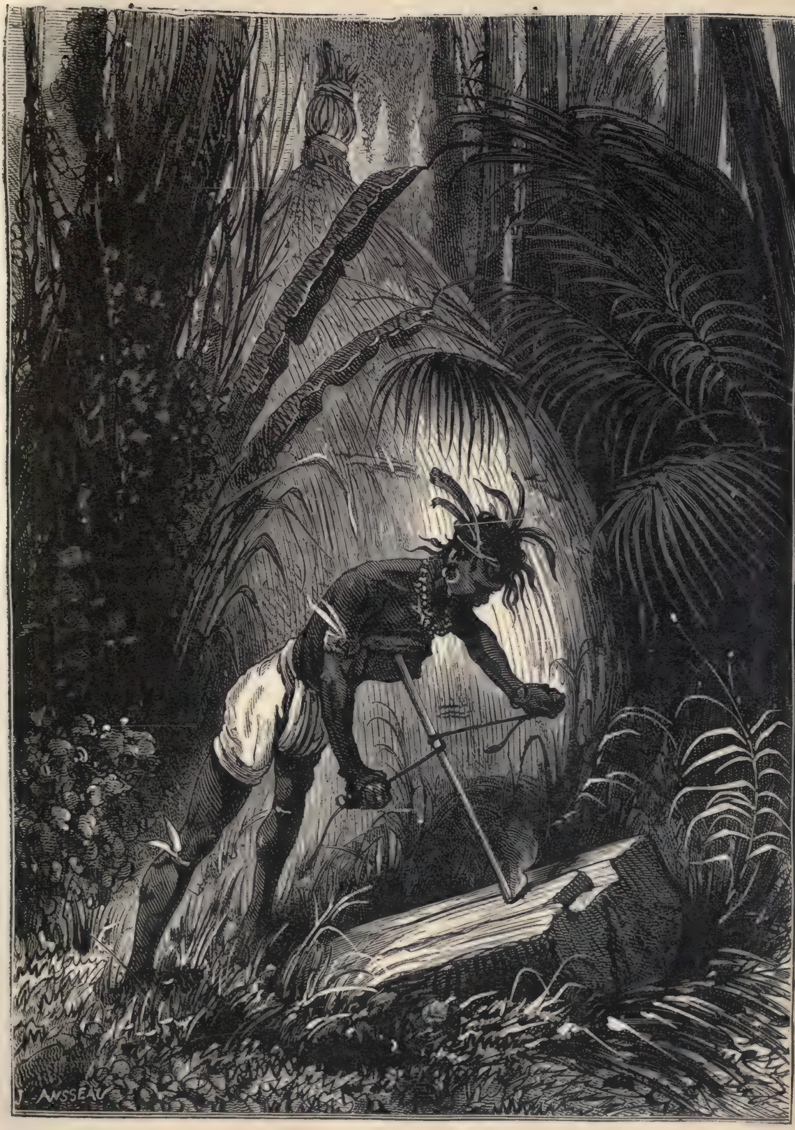

PLATE VII.—AN INDIAN OBTAINING FIRE BY FRICTION.

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method consists in rolling the rod between the fingers; but in many of the Polynesian islands the rod is longer and somewhat flexible, so that it bends under the pressure into a bow-like form, and the hand applied to its centre imparts to it a circular motion, exactly as a carpenter turns an augur. The method represented in our illustration is that used by the North American Indians: they pass two or three turns of a cord round the rod, and draw the cord alternately in one direction and the other. The reader will hardly fail to remark the identity between the modes of producing heat in Count Rumford's experiment, and in the very ancient and primitive operation just described.

Rumford estimated the quantity of heat developed in his experiment, taking into account the *specific heats* of the brass cylinder and of the steel borer, but neglecting that taken up by the wood and lost by radiation. He states the result to be a quantity of heat sufficient to raise 26½ lbs. of water from the freezing-point to the boiling-point, and this heat, he says, "is produced equably or in a continual stream (if I may use the expression) by the friction of the blunt steel borer against the bottom of the hollow metallic cylinder, and is greater in quantity than that produced equably in the combustion of nine wax candles each three-quarters of an inch in diameter." The next passages are remarkable as showing that Rumford understood that the source of this heat was the mechanical energy exercised by the horses employed in turning his machine, and that he seems even to have had some idea that the horses' food might in its turn be the source of their muscular power. His words are: "As the machinery used in this experiment could easily be carried round by the force of one horse, though to render the work lighter two horses actually were employed in doing it, these computations show further how large a quantity of heat might be produced, by proper mechanical contrivance, merely by the strength of a horse, without fire, light, combustion, or chemical decomposition; and, in case of necessity, the heat thus produced might be used in cooking victuals. But no circumstances can be imagined in which this method of procuring heat would be advantageous; for more heat might be obtained by using as fuel the fodder necessary for the support of a horse." He then goes on to show that the heat evolved in these experiments could not have been due to a diminished capacity for heat of the fragments which the borer detached from the mass of the brass; for the explanation which was then commonly accepted was that the metal thus forcibly detached in small pieces had its capacity for heat diminished, and then the "*caloric*" oozed out, as it were, in the form of sensible heat. No one before Rumford thought of testing this plausible explanation by experiment: he collected the fragments of brass, and found that their *capacity for heat* was unchanged. He proved also that the heat produced in his experiment could in no way have been derived from the air, or the water from other parts of the apparatus. Thus he arrives at the following remarkable conclusion: "It appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated in the manner the heat was excited and communicated in these experiments, EXCEPT IT BE MOTION."

These experiments and the author's conclusions from them did not attract the attention they deserved, for men's minds were prepossessed with the notion of *caloric*. It was not, in fact, until more than forty years after Rumford published his inquiry that the conception of heat as a mode of motion began to supersede the notion of heat as the subtle fluid *caloric*. The *caloric* theory had indeed been found useful as a means of expressing many facts, and of binding them together by means of a common principle. *Caloric*, as we shall have occasion more fully to discuss in another chapter, displaced *phlogiston* in the interpretation of chemical phenomena, and easily explained a considerable range of physical phenomena; but in explanation of those adduced by Rumford it utterly failed, and other experiments were soon after devised to which the *caloric* theory was, if possible, still more obviously inapplicable. For instance, Sir Humphry Davy (whose great chemical discoveries will be mentioned in another chapter) devised a very ingenious experiment in 1799. He arranged an apparatus by which two pieces of ice could be made to rub against each other in the exhausted receiver of an air-pump. In a few minutes the pieces of ice were entirely converted into water, and this water was collected and found to have a temperature of 35° F., although the temperature of the atmosphere was lower than this. According to the notions then prevalent, the capacity for heat had diminished; but, says Davy, the capacity of water for heat is *much greater* than that of ice, and ice must have an absolute quantity of heat added to it before it can be converted into water. Friction consequently does not diminish the capacity of bodies for heat. The heat developed in this experiment could not be derived from the air, for none was present, nor from the parts of the apparatus, for the liquefaction took place only at the surfaces of the ice which rubbed against each other. Davy came to the conclusion that the immediate cause of heat is the motion of the invisible particles of bodies, and that it is by the ordinary laws of the communications of motion that friction produces heat. He conceives that the motion may consist of vibratory or undulatory movements of the particles of bodies, or that these particles may revolve on their axes or round each other. The particles of the hottest bodies, he supposed, would move with the greatest velocity, and through a space *relatively* greater. Temperature may be conceived to depend upon the velocity of the vibrations; increase of capacity upon the motions being performed through greater spaces; disappearance of sensible heat in the conversion of solids into liquids, and these into gases, may be explained by these states consisting in greater amplitude of the vibrations of the particles.

A reader unaccustomed to consider atoms and their motions of greater or less amplitude, etc., may not improbably be at first unable to reconcile the obvious and complete apparent absence of motion in a body as a whole, with the possibility of complex and rapid motion of its minute particles. He must remember that these particles or atoms, being themselves by supposition altogether invisible, their motions must be invisible also. His conceptions may be aided by the beautiful illustration which is given of this very subject by Lucretius, who points out that a flock of sheep browsing on a very distant hill will appear as a motionless white patch on the green slope, although the individual animals are, in fact, freely moving in various ways.

> Nam sæpe in colli tondentes pabula læta
> Lanigeræ reptant pecudes, quo quamque vocantes
> Invitant herbæ gemmantes rore recenti ;
> Et satiati agni ludunt, blandeque coruscant,
> Omnia quæ nobis longe confusa videntur
> Et veluti in viridi candor consistere colli.—*De Rer. Nat.* II., 317.

The idea of atoms and their invisible motions is, as indeed we have already seen, by no means new to philosophy. The views enunciated by Rumford and Davy as to the nature of heat may, however, be considered the first distinct expression of the *Dynamical Theory of Heat.*

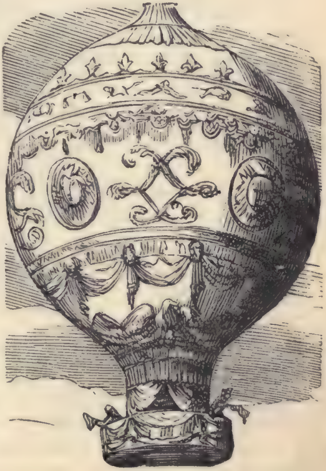

FIG. 150.

From the latter part of the eighteenth century dates a scientific discovery—or, perhaps more properly, an invention—which excited everywhere the greatest surprise and admiration in the public mind. It appeared also to open up to the scientific explorer regions which the most audacious of his predecessors would have considered for ever inaccessible. Though the great results which it seemed to promise to science and to humanity have not been realized, and the utility of the invention has hitherto proved as limited as the enthusiasm it first excited was unbounded, it is never even now seen in action without lively interest. In 1782 the brothers STEPHEN and JOSEPH MONTGOLFIER made their first experiments on balloons; and in June, 1783, the ascent of a balloon was for the first time witnessed by the public at Annonay. (*See* Fig. 151.) The balloon was made of linen covered with paper, and was of the kind called a fire-balloon. That is, it was provided with the means of burning substances at the lower part, so that the smoke should fill the balloon. The Montgolfiers at this time erroneously imagined that the ascensive power was due to the *smoke*, whereas in reality it was the expansion of the air within the balloon occasioned by the *heat* that produced the effect. The first aërial voyage was made by Pilatre de Rozier and the Marquis d'Arlandes on the 21st of November, 1783, in the *Montgolfier* (or fire) balloon represented in Fig. 150. Ten days afterwards two daring experimenters made at Paris the first voyage in which a balloon filled with hydrogen gas was used. Balloon ascents after that became comparatively frequent.

The most ingenious and persevering attempts have been made to find some method of propelling or of guiding balloons through the air. None have succeeded; chiefly because a balloon does not, like a bird, move *through* the air, it merely moves *with* the air. There are, however, many persons who still believe in the practicability of directing the course of balloons.

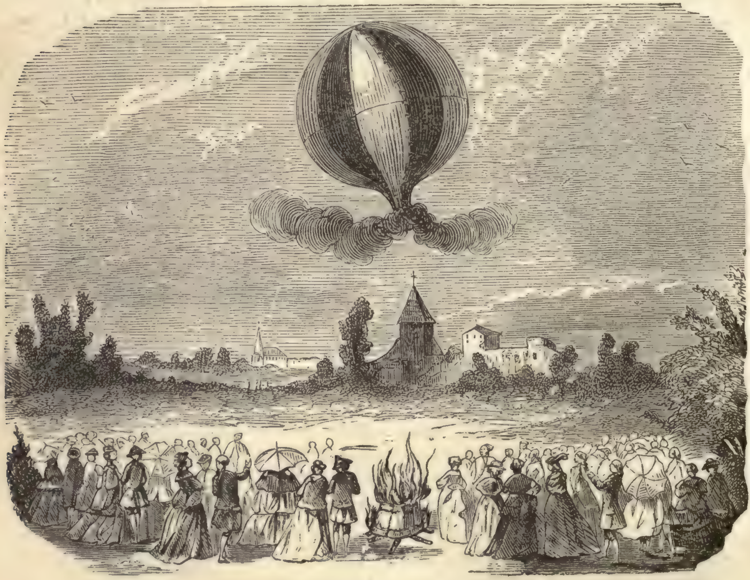

FIG. 151.

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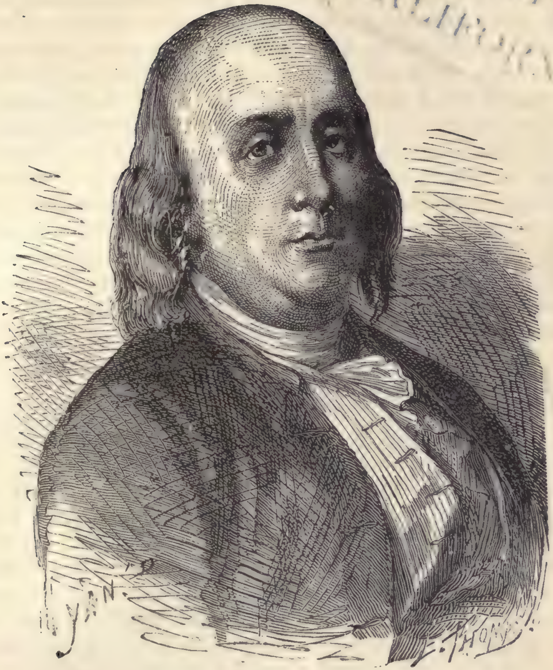

FIG. 152.—BENJAMIN FRANKLIN.

# CHAPTER XIII.

## PHYSICS OF THE EIGHTEENTH CENTURY (*continued*).—ELECTRICITY.

IN the history of each science there are periods which have a special lustre from the number and brilliancy of their discoveries. In the present chapter we have to treat of a science which scarcely had an existence at all before the eighteenth century; yet its history from that time to the present day is a voluminous record of the continuous revelation of previously unsuspected and very wonderful truths. The facts which it investigates are in some respects stranger than all the tales of fairyland or the wildest dreams of magic. A very few of the phenomena of Electricity were known before the period we are now considering. The only experimental data were observations of Gilbert of Colchester (page 94), and those of Otto von Guericke, the inventor of the air-pump (page 172), to whom also science is indebted for the first electrical machine. Guericke simply made a globe of one of the substances recognized by Gilbert as possessing the same property as amber, and this globe the Magdeburg physicist caused to turn on an axle, so that it could readily be rubbed. In a book published in 1672 Guericke describes the mode of preparing the machine thus:—"Take a spherical glass bottle as big as a child's head, fill it with small pieces of sulphur, and bring it near the fire so as to melt the sulphur. Then let it become cold, and break the glass in order to get out the sphere of sulphur, which you will keep in a dry place. If you like you may make a hole through it, so that it can be turned round on an iron axis, and the globe will then be ready for use." Guericke first noticed the *electric spark*, which this arrangement, so much larger than the small pieces of material used by Gilbert, permitted him to observe. He also remarked that light substances, which had been attracted by the excited sulphur, were afterwards repelled until they had touched some other body. When a novel observation of this kind is made by a person of a scientific turn of mind, he almost always seeks to find in the phenomena some analogy with other phenomena with which he is familiar. Sometimes the similarities between apparently different cases are so disguised as to require a very rare order of intellect to perceive them; in other cases they may be sufficiently obvious. The discoverer is, however, liable to be placed on the wrong track by misleading analogies. And this is what happened in the present case to Guericke, who saw in these attractions a perfect imitation, as he thought, of the attraction which the earth exercises on bodies near it.

Guericke's machine, if such it can be called, possessed very feeble power. The spark could only been seen in the dark, and the sound accompanying it was audible only to an ear placed very near the sulphur globe.

Much more powerful effects were produced by a machine arranged by HAWKSBEE (16..—17..), who also made considerable additions to our knowledge of electrical phenomena, though he did not at first suspect that electricity was the agent concerned in the phenomena he observed. In the year 1705 Hawksbee found that when mercury is shaken in glass vessels, light is produced. This fact, indeed, was known before; but Hawksbee tried the effect of shaking the mercury in a bottle from which the air was exhausted, in order to discover if the air was in any way the cause of the luminous appearance. He was somewhat surprised to find that when the bottle was partly exhausted the light was more vivid than before; but though he concluded that friction was the cause of the phenomenon, he was unable to determine whether or not the presence of some air was necessary. By many experiments he discovered that the rubbing together of various substances was capable of producing light, as, for instance, glass or amber rubbed with flannel. Continuing his experiments, he found that when an exhausted glass globe is rubbed by the hand, a vivid light is produced; also when two pieces of lump sugar are rubbed together, and in many other cases. He still did not suspect that electricity was the cause of the light, for in the case of the exhausted globe he attributes the light to some peculiar quality of the glass. At last it occurred to him that the excitation of electricity explained the luminous appearance; and he found that a large glass tube attracted light bodies when rubbed, and also gave off sparks. When the interior of the tube was exhausted, the luminous appearances were increased, but the attractive power was less. The arrangement represented in Fig. 153 was then fitted up by Hawksbee, for the purpose of better studying these luminous appearances, and the reader will hardly fail to recognize in it the prototype of the cylinder electric machines that are seen in cabinets of scientific apparatus at the present day. But the apparatus has some special adaptation for the purpose Hawksbee first intended it. There are two glass cylinders, an inner and an outer one, and these could be exhausted so that the luminous appearances in a vacuum might be studied; or they might be filled with various media. The glass cylinders could also be made to rotate separately or together. Hawksbee relates many experiments with this apparatus, noticing sometimes differences in the appearances observed when only one cylinder revolved, when both revolved in one direction or in contrary directions, when one of the cylinders was rubbed, when

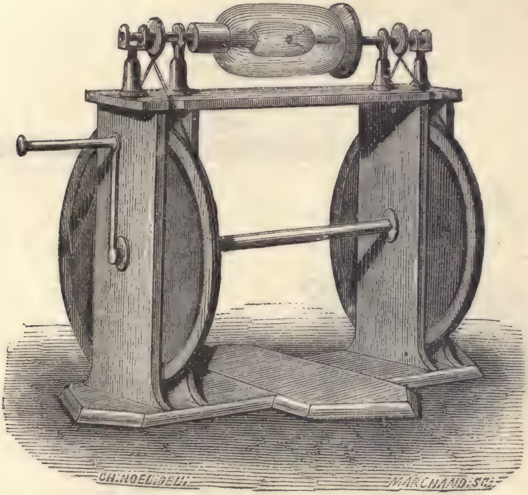
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FIG. 153.—HAWKSBEE'S ELECTRICAL MACHINE.

both, etc. Books published a few years later represent Hawksbee's machine under the simpler form of a single glass globe traversed by an axis, as shown in Fig. 158. But for some time after devising the globe apparatus, this experimenter continued to use the still more elementary glass tube held in the hand.

In 1708 a paper published by Dr. Wall in the "Philosophic Transactions" states, that the author found that amber, sealing-wax, gum-lac, diamond, and, in fact, all electrical bodies, are rendered luminous by friction. Thus a general property of electrical bodies was established; that is to say, it was proved that when a body was by friction made to attract light substances, that body at the same time became luminous.

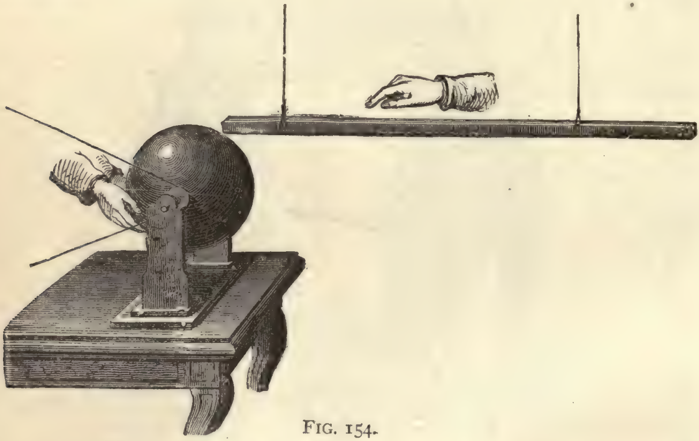

FIG. 154.

It may be convenient at this stage to point out that we may call *electrical* all substances which, when rubbed, acquire the property of attracting light bodies. It is usual also to say that bodies are *electrified*, and to call the cause, whatever it may be, of these phenomena *electricity* (from *electron*, the Greek name for amber). It may also not be wholly superfluous to remark that *these words do not in the slightest degree explain the phenomena in question.* They are merely convenient words for expressing certain states of matter and the absolutely unknown cause of those states. The differences observed in the behaviour of bodies when rubbed caused all substances to be divided into two classes: the one contained all those which could be electrified by friction, to be called *electrics* or *ideo-electrics*, such as glass, amber, sealing-wax, etc.; the other class contained those which showed no signs of electricity when rubbed, such as *metals*, etc., and these were termed *non-electrics* or *anelectrics*. This was a classification established by the next experimenter who appears on the scene—that is, STEPHEN GRAY

(1695—1736). Gray used as the source of electricity in his experiments a glass tube about 3½ feet long and 1¼ inches diameter; and, in order to prevent dust settling in the interior of the tube, he closed its open ends with corks. The tube was simply held in the hand and rubbed with a piece of cloth. One of Gray's first experiments with this tube was to ascertain if the closing of the tubes with corks influenced the development of electricity. He was unable to find any difference, but in the course of his experiments he observed a circumstance which put him on the track of further investigation: he noticed that a feather was attracted and repelled by one of the corks exactly as it would have been by the excited glass tube itself. He surmised that the electric virtue had passed by contact from the tube into the cork, and generalizing upon this, he conceived that perhaps electricity might have the power of passing through bodies in contact, and in order to test this he tried the following experiments in succession. He fixed an ivory ball on the end of a piece of wood 4 inches long, and thrust the other end into the cork. On rubbing the tube, he found that the ivory ball attracted and repelled a feather with more vigour than the cork. He then fixed the ball on longer wooden rods, to see whether the effect was immediately conveyed to greater distances; first used a rod 8 inches, then one 24 inches long, and found the same effect as before. He then varied the substance by taking a piece of iron wire, one end inserted into the cork and the other bearing the ivory ball. He found that the ivory ball attracted the feather as before, and he noticed also that the ball had a stronger effect on the feather than any part of the wire connecting the ball and the excited tube. Brass wire acted in the same way as iron. When the piece of wire was 2 or 3 feet long, the vibrations produced in it by rubbing the tube making the experiment more difficult to manage, Gray bethought himself of hanging the ivory ball by a piece of packthread attached to the tube. He found that the ball attracted and repelled light substances as before. For his ivory ball he now substituted other things in succession: a ball of cork, one of lead; a coin; fire-shovel; poker; copper tea-kettle, first empty, then filled with cold water, then with hot water; etc., and in each case the experiment succeeded as before. His experiments, again, had for their object to determine how far the electric virtue might be carried. Having by him a hollow walking-cane, 31 inches long, he fitted this into the end of his glass tube instead of the cork, and found that this also conveyed the electricity to the ivory ball. A solid cane had just the same effect. He then inserted part of a fishing-rod, in all 14 feet long, and afterwards greater lengths of tapering rods of cane and reed, until he at last had a ball of cork 32 feet from the end of this glass, and the same result as before with regard to the attractions. But the bending of this great length of rod, and the vibrations which were occasioned by rubbing the tube, made this form of experiment an extremely inconvenient one, and Gray therefore again resorted to the expedient of hanging a ball by a piece of packthread to the end of a short length of rod.

He now tried to carry the electric virtue horizontally by supporting his line of packthread by passing it through a loop of the same material which hung down from a beam. In this case not the least sign of attraction was perceptible, and Gray concluded that the electric virtue must have passed along the line until it arrived at the point where it was suspended, when it must have gone up to the beam. Gray went the next day to exhibit these experiments to his friend Wheler, and from the parapet of his house to the ground, a distance of 30 feet, they found that the line conveyed the electricity. Wheler then wished to see how far the line would conduct it horizontally, and Gray described his non-success of the preceding day. Wheler suggested that the packthread might be supported on silk threads, as this material would have sufficient strength with the smallest thickness, and be on that account less liable to carry off the electrical virtue. The first experiment of this kind was made on the 2nd July, 1729, in a long matted gallery, the line of packthread being supported on cross lines of silk stretched transversely on nails driven into the opposite walls. The line of packthread was 80 feet long, yet the ivory ball at its extremity at once attracted some light substances when the tube was rubbed. In order still more to increase the length of line through which the electricity might pass, the friends arranged the packthread so that, after passing from end to end of the gallery, it should return, and the tube and the ball being both at the same end of the gallery, care was taken that they were yet sufficiently far apart to prevent any influence reaching the ball except through the line, which was now 147 feet in length. The result was as decided as before. The illustration opposite (Fig. 155) represents Gray conducting these experiments, in which he is virtually constituting for the first time an electric telegraph. In some subsequent experiments of the like kind, the silk supports broke under the weight of the packthread. On this they attempted to hang their line by some fine iron wire, because its thinness would, as they still thought, prevent the electricity passing off. But the iron wire being too fine to sustain the weight, some rather thicker brass wire was substituted. And now, although the tube was well rubbed, not the slightest manifestation of electricity appeared at the ivory ball, and Gray concluded that the wire carried off the electricity, recognizing now the fact that it was not the smallness of the silk lines, but their *nature*, which caused the former experiments to succeed. Gray found, soon afterwards, that hair and some other substances have the same property as silk—that is to say, they do not allow the electricity to pass off through them. He proceeded to institute a long series of experiments on various substances to ascertain their power of receiving electricity. For example, he found that a soap-bubble may be electrified so as to attract thin leaves of Dutch gold. Another experiment, which was thought to be

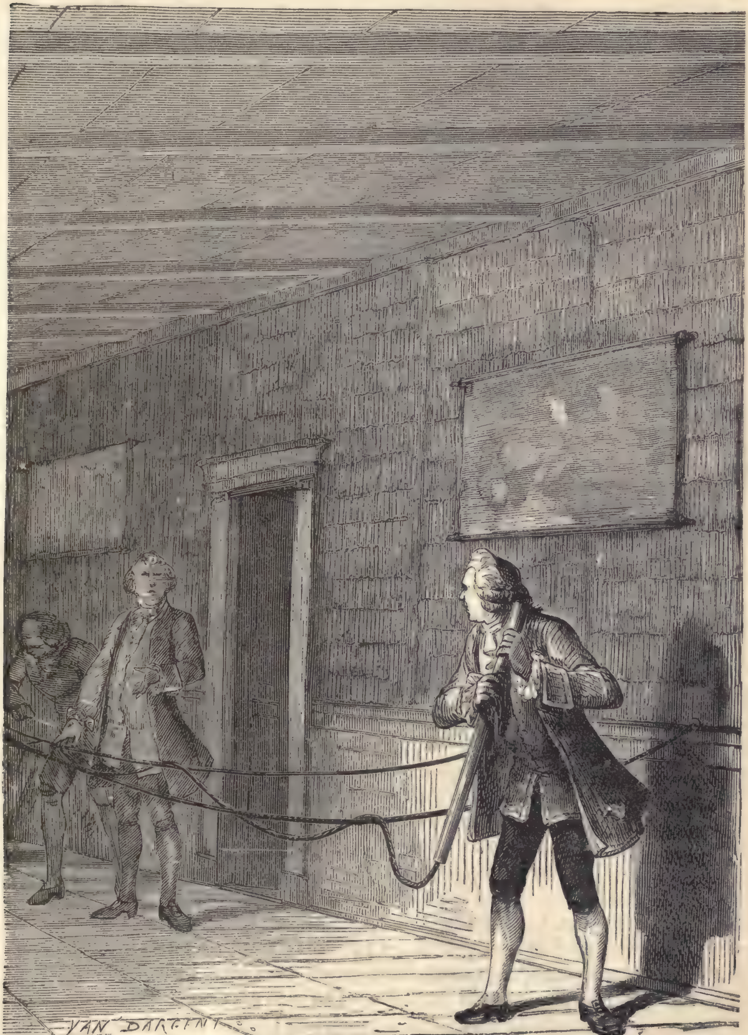

FIG. 155.—GRAY'S EXPERIMENT.

extremely curious, consisted in suspending a boy by several hair ropes, and by merely bringing the excited glass tube near his feet, causing electrical attraction to be manifested by his face.

The last paper which Gray communicated to the Royal Society was dictated by him on his death-bed to Dr. Cromwell Mortimer. He died in 1736; and had he lived to revise his experiments, he would doubtless have come to different conclusions. He, like Guericke, attempted to assimilate the motions of the planets to the movements of electrified bodies. He was persuaded that an electrified body could cause another to revolve round in an ellipse, and from west to east. "Place," he said, "a *feebly* electrified small iron ball, an inch or an inch and a half in diameter, on the centre of a circular plate of resin seven or eight inches in diameter, and then a light body suspended by a very fine thread five or six inches long, held *in the hand* above the centre of the plate, will begin to move in a circle round the iron ball, and always from west to east. If the globe is placed at a little distance from the centre of the circular plate, the small body will describe an ellipse, the eccentricity of which will be the distance of the ball from the centre of the plate." If the plate of resin is of an elliptical shape and the iron ball be placed in its centre, the light body will describe an elliptical orbit of the same form as the plate." These experiments were quite fallacious, although Dr. Mortimer, repeating them after Gray's death, tried and, as he thought, confirmed their accuracy. The error of these observers is attributed to the now well-known psychological action by which the movements are determined in the familiar oracle of the "Bible and key," and similar cases. The idea present in the mind of the experimenter, that the suspended body would move from west to east, and that it would describe such and such an orbit, was sufficient to cause an unconscious impulse to be communicated from the finger holding the thread. In fact, it was soon found that when the thread was attached to a support, instead of being held by the finger, these phenomena were no longer observed.

The foundation of electrical science may be said to have been laid by Gray's experiments; but some important discoveries were soon announced by a French man of science, and he besides supplied the infant science with its first theory. Perhaps this is but another illustration of the tendency to generalization which distinguishes the French intellect. In this case the theoretical views have been singularly appropriate to the facts, for Du Fay's theory has not been superseded even at the present day. DU FAY (1698—1739) was originally a military officer, but after some years' active service he quitted the army, and devoted himself wholly to scientific pursuits. He was an active member of the Academy of Sciences, and was appointed to the superintendence of the Royal Botanical Gardens at Paris, in which capacity he was the predecessor of the celebrated Buffon. Du Fay was first induced to study electricity by the writings of Gray and Hawksbee, and by these he was put in the track of the following discoveries. He found that all bodies (except, he thought, metallic, fluid, or soft ones) could be made electric by friction with a cloth, provided they were previously made thoroughly dry and warm. All bodies without exception could be rendered electrical by merely bringing an excited glass tube near them. Gray had discovered this to be the case with some bodies; but Du Fay showed that the fact was general, provided the bodies were supported upon a perfectly dry glass stand. Gray had supposed that differently coloured bodies were attracted in different degrees. Du Fay made an experiment which seemed to confirm this; but with much sagacity he traced the effect to the substance of the dyes, and not to the colours themselves. Du Fay also describes the electric spark as drawn from a person suspended by non-conducting lines. But the most important of all Du Fay's writings are those wherein he establishes the fact that there are two kinds of electricity, one of which he calls the *vitreous* and the other the *resinous* electricity, and that when two bodies are charged with electricity of the same kind, they repel each other; but bodies charged with different electricities attract each other. The facts and the theory will be perfectly clear to any person who will try the following very simple experiments. Take a fibre of white silk, attach it to the middle of a straw, B, Fig. 156, and hang from some fixed point of support. At each end of the straw fix a little disc of thin paper a quarter of an inch in diameter. The only other apparatus required are: 1st, a piece of sealing wax or of amber, or an ebonite paper-knife, penholder, or similar article; 2nd, a piece of glass tube, or a common glass tumbler, very dry and warm; 3rd, a dry or warm silk handkerchief. The experiments and their explanation by Du Fay's theory are simply these: Rub the glass briskly with the handkerchief, and immediately present the glass to one of the paper discs: it will first be attracted into contact with the glass, from which it will receive a charge of *vitreous* electricity, and will then be strongly repelled. If, however, the stick of sealing-wax be rubbed and presented to the same disc which is repelled by the glass, it will be strongly attracted, because the friction has charged the sealing-wax with *resinous* electricity. On contact with the sealing-wax the disc becomes charged with this kind of electricity, and is repelled; but in this condition it is strongly attracted by the excited glass.

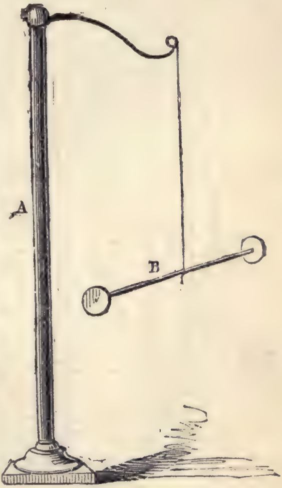

FIG. 156.

The principle propounded by Du Fay opened a wide field for the progress of electrical knowledge, and it threw a light upon all the facts which were known at his time, permitting those facts which had befor appeared hopelessly intricate and inexplicable to be expressed and arranged in a simple and systematic manner. Gray repeated, in his turn, many of Du Fay's experiments, and he especially describes the luminous appearances and the electrification of water. In connection with the subject of the "electric fire," he makes the remark (1735) that several of the experiments appear to indicate that it may be of the same nature as lightning.

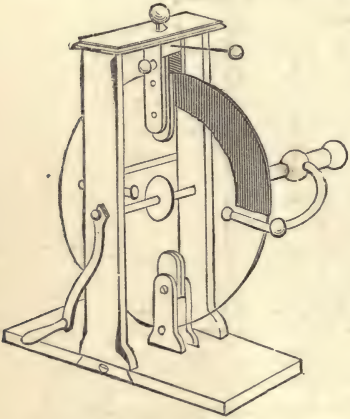
FIG. 157.—THE PLATE ELECTRICAL MACHINE.

Germany took a part in the development of the new science by devising improvements on the electrical machine. The progress of any natural science is, we shall constantly see, greatly dependent upon the perfection of the appliances by which its phenomena are studied. Conductors were provided by which the electricity might be gathered from the revolving globe of glass, and a cushion was substituted for the hand. About 1750, cylinder machines were constructed in England and in Holland; but soon after this, machines upon an entirely new plan were made by RAMSDEN, who substituted a circular plate of glass for the globe or cylinder; and the most commonly met with electrical machine at the present day is that of Ramsden. The form of this, represented in Fig. 157, is too well known to require description. In the earlier globe machines the "conductor" was simply a metallic bar or tube suspended by silk cords, with one of the ends nearly touching the revolving globe. This arrangement is shown in Fig. 154. WILLIAM WATSON, who wrote on electricity about the middle of the eighteenth century, gives in his work the view of the machine shown in Fig. 158, which represents, he says, an electrical machine much used in Holland, and especially in Amsterdam. This may be compared with the machine constructed many years afterwards by NAIRNE, a view of which is given in Fig. 159. C is a cylinder of glass, D and D′ are hollow metallic cylinders with spherical ends, and each is supported on a glass pillar. To D is attached a cushion pressing against the cylinder, and D receives the electricity from the glass by a number of projecting metallic points placed on the side next the cylinder. This machine permits both resinous and vitreous electricity to be studied, for while the conductor D supplies *vitreous* electricity, the conductor D′ yields an equally ready supply of *resinous* electricity. This form of the electrical machine illustrates another

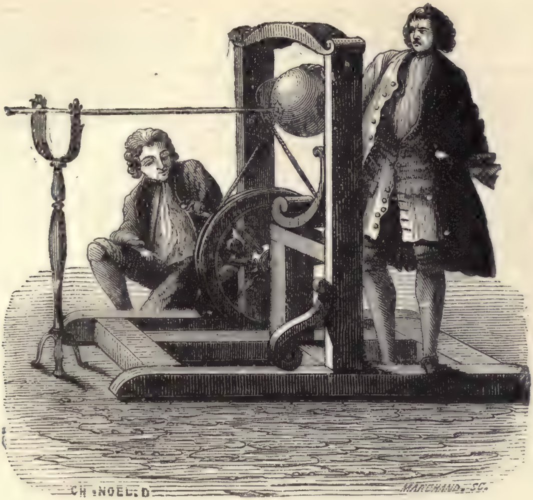

FIG. 158.—DUTCH ELECTRICAL MACHINE.

fundamental fact which has not hitherto been mentioned, and that is, that one kind of electricity is never produced without the other manifesting itself in an equal degree.

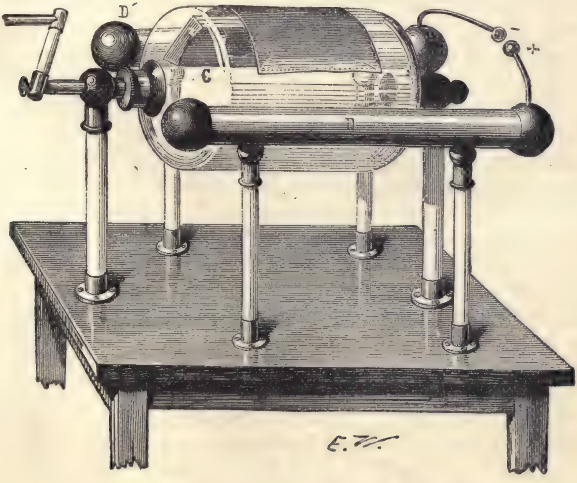

FIG. 159.—NAIRNE'S ELECTRICAL MACHINE.

On the death of Stephen Gray, in 1736, a series of researches in electricity was entered upon by a very active member of the Royal Society, Dr. J. T. DESAGULIERS (1683—1744), who exhibited many experiments to illustrate the fresh facts he continued to discover. And, indeed, new observations of electrical phenomena accumulated very rapidly in all quarters, for the singular nature of these phenomena excited great interest in learned and simple alike. But there were not wanting severely practical men who regarded all the interest which electricity was exciting as so much idle curiosity and childish amusement, and these persons raised the cry of *cui bono?* It is modestly, piously, and philosophically replied to by Watson, who admitted that the electricians of his day had not advanced far enough in their discoveries to make them useful to mankind, but observed that in every department of physical science perfection is attained only by very slow degrees. "It is our duty to continually advance in our knowledge, and to leave the rest to that Providence who has created nothing in vain."

Towards the middle of the eighteenth century another electrical phenomenon was discovered, and, familiar as it is now, it is difficult to picture the sensation it produced. It was an experiment which caused a new word to be added to every language in Europe; or, rather, it was the occasion of a term, originally purely scientific, being adopted into the common language, of course with a derived signification, as when it is said that a speech "*electrified* the audience," etc. The discovery to which we here allude was that of the *Leyden Jar.* It was made by MUSSCHENBROECK (1692—1761), at Leyden, in 1746. He was trying to find a means of preserving electricity, when it occurred to him that if the water in a glass bottle were electrified, and the bottle closed by a stopper, the electricity might remain in the water for an indefinite time, inasmuch as it would be surrounded by a non-conducting substance. It was while endeavouring to realize this idea that Musschenbroeck became the first man to experience the *electric shock.* There is every reason to believe that the shock in question was very much weaker than those that are now taken for amusement even by children. But the novelty of the sensation made the first experimenters unconsciously exaggerate to themselves the impressions they received. Let us see how Musschenbroeck himself describes his own sensations in the letter in which he, on the 20th of April, 1746, announces his discovery to Reaumer:—

"I wish to describe to you a new but dangerous experiment, which I advise you not to attempt yourself. I was engaged in some researches on the power of electricity, and for that purpose I had suspended (*see engraving*), by two blue silk lines, a gun-barrel, which received the electricity of a glass globe rapidly turned on its axis, and rubbed by applying the hands to it. At the end of the gun-barrel, away from the globe, there hung a brass wire, the end of which plunged into a round glass bottle, partly filled with water. I was holding the bottle with

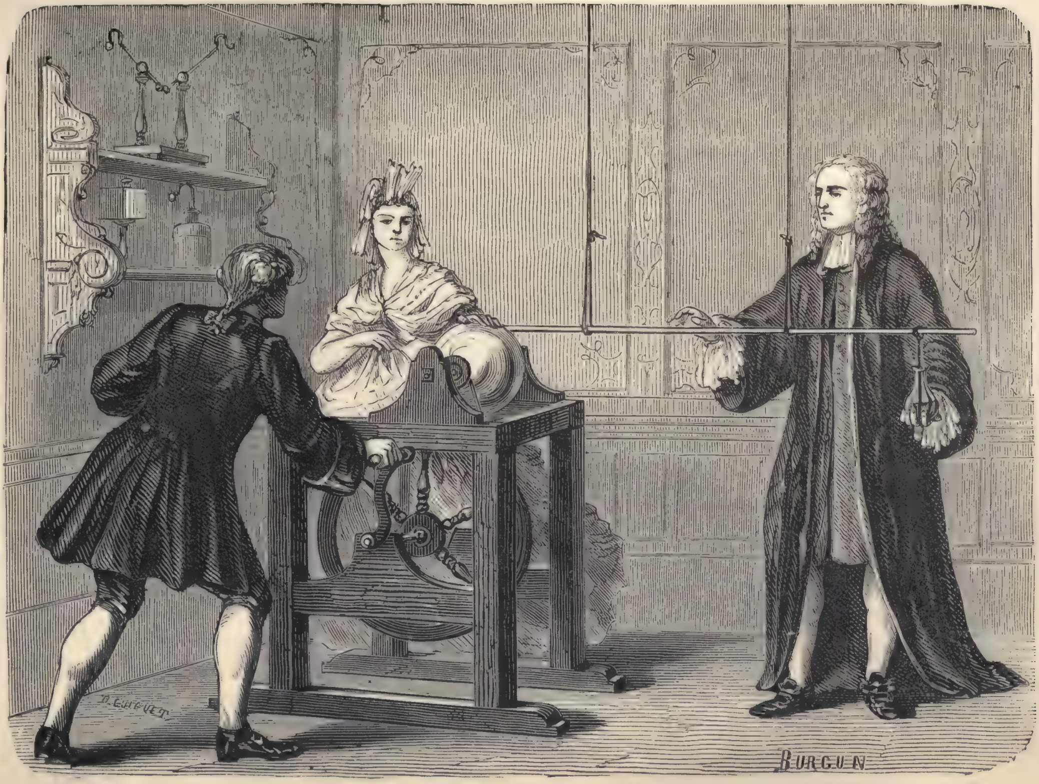
D. CHIGUET
BURGUN

PLATE VIII.—THE DISCOVERY OF THE LEYDEN JAR.

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one hand, and with the other I was trying to draw sparks from the gun-barrel, when suddenly the hand holding the bottle was struck with so much violence that my frame was shaken as if by a lightning-stroke. I thought that all was over with me, for my arm and my whole body were affected in a dreadful way, which I cannot describe. The glass vessel is not generally broken in this experiment, nor is the hand moved; but it is singular that the experiment succeeds only when the vessel is made of Bohemian glass: the shape and thickness are immaterial. The bottle which made me think I was killed was of thin white glass, and five inches in diameter. The person who tries the experiment may simply stand on the floor, but the glass vessel must be held by the same person who with the other hand receives the spark." Musschenbroeck says also that he would not go through such an experience again for the crown of France.

There were two persons who co-operated with Musschenbroeck in these experiments. To CUNEUS, one of these, the honour of being the actual first discoverer of the Leyden jar is sometimes ascribed. The other, named ALLAMAN, says that the first time he experienced the shock he was so stupefied as to be unable to breathe for some minutes. But perhaps the alarming element in the accounts of the early experimenters reaches its height in the description given by Winckler, a professor at Leipzig. He says that his whole body was convulsed, that his head felt as heavy as if it had a stone inside, and that thinking he was going to be seized with a burning fever, he was obliged to have recourse to cooling medicines. The professor's wife was another victim to the power of imagination, for we are told that after she had experienced the shock, she remained for a week scarcely able to move. The Abbé Nollet, attempting the experiment at Paris with a flask, not of Bohemian but of French glass, and hardly expecting to succeed after Musschenbroeck's statement about the kind of glass required, he received a tolerably smart shock, on which he was obliged to let go the glass vessel he was holding. The error of Musschenbroeck in supposing that a certain kind of glass was necessary has been since explained by the mere accident of his Bohemian glass vessel having been *dry* in the upper part, whereas the others were not so.

After the novelty of the experiment had passed, and familiarity had deprived it of all imaginary terrors, persons of every age, sex, and rank were desirous of experiencing the new sensation. The *electric shock* became, in fact, a fashionable amusement, and for several years was, to use a colloquialism, all the rage.

Among the men of science of the eighteenth century were many whose careers also as statesmen, ecclesiastics, military or civil officials, and men of ordinary business, were honourable and distinguished. The fact of so many men achieving success at once in the ordinary affairs of the world and in science, appears less extraordinary if the methods of thought required for science be after all nothing more, as some one has said, than the refinement of common sense. The illustrious American, whose name is prominent in the history of electricity in the eighteenth century, had apparently no training which specially qualified him for scientific researches. BENJAMIN FRANKLIN was born at Boston, in North America, on the 17th of January, 1706, he being almost the youngest of a family of seventeen children. Franklin's father had emigrated from England about twenty-four years before, and he followed in Boston the occupation of soap-boiler and tallow-chandler, and apparently with only very moderate success. When only ten years of age, Benjamin had to assist in his father's business; but as this proved very distasteful to him, he was two years afterwards apprenticed to an elder brother as a printer, one of the considerations which caused this calling to be fixed upon being Benjamin's fondness for books. He became clever at his business, but continued to follow various literary studies with great assiduity. At seventeen years of age Franklin arrived at New York in search of employment, and passed from thence to Philadelphia; but soon afterwards made his way to London. Here he learnt the newest processes in his craft, and after about eighteen months' stay he returned to Philadelphia in his twenty-first year. There he eventually established himself as a printer and stationer, and there he was elected to fill important public offices.

Franklin was a man of original and independent mind, untrammelled with any of those prepossessions which a training in the science of the schools would in some degree have entailed. In 1746 and 1747 Franklin was informed by a correspondent of the electrical discoveries which had produced so great an impression in Europe, and he had sent out to him some apparatus for the experiments. Franklin's discoveries in electricity were communicated to a friend in England named Peter Collinson, who was a member of the Royal Society, in a series of letters containing short and simple accounts of detached experiments. From these we shall collect the general outline of the important researches by which Franklin contributed to the progress of the science of his day. There are two points on which Franklin's electrical labours were concentrated,—the one theoretical, the other experimental. The former precedes in order of time. The importance of some theoretical views as a guide through the intricate labyrinth of innumerable facts has been referred to already in these pages. The theory by which Franklin proposed to explain the facts of electricity proved of very great service in the development of the science. What this theory was, and by what facts he supported it, we may now proceed to consider.

The great question that presented itself to Franklin's mind was this: Is electricity created *de novo* by the friction of the glass tube, or is it merely communicated to the glass from other bodies? He resorted to a simple experiment to get some solution of this question. Standing on a cake of resin, so as to cut off all electrical communication with the ground, or, as the phrase goes in electrical matters, "*insulating*" himself, Franklin excited his glass tube by rubbing it with his hand, and then drew off its electricity into his own body. It was then well known that, if under these circumstances he had drawn off the electricity from an excited tube presented by another person, he would have been electrified; but Franklin found that in his experiment his body showed no signs whatever of electrification. Now, he reasoned, the circumstance of having rubbed the tube had effected precisely such a change in him, that the electricity he afterwards drew from the tube failed to produce any effect whatever, or, in other words, its operation was exactly neutralized by the state induced by the rubbing. He tested these views by separately insulating two persons, one of whom rubbed the tube, and the other drew electricity from it. In this case both became electrical, exhibiting the usual signs, such as attractions of light bodies and sparks. But when these persons, still insulated and electrified, touched each other, all signs of electricity vanished; and it was noticed that in touching, the spark produced was brighter than if they had touched a third person. Upon such facts as these the main features of the Franklinian theory were founded. Franklin considered that every body in nature contained in its ordinary state some definite quantity of a subtle fluid which constitutes electricity. In this condition, all bodies being equally supplied with the fluid, no signs of its existence are manifested. But when a glass tube is rubbed, the ordinary or natural distribution of the fluid is disturbed, the glass receives more than its natural share, and the excess is derived from the hand that rubs the tube. Thus, from the body of the operator electricity is drawn off on the glass tube; but if he simply stands on the ground, the deficiency is immediately supplied from the earth, and he manifests no electrical signs whatever. If he is insulated he does become electrified, provided he does not allow the electricity accumulated in the tube to pass back again into his body, thus restoring to him his normal quantity of the fluid. Franklin introduced the terms *positive* and *negative* electrification, to express the respective conditions of bodies containing more or less than their normal amount of electric fluid. Thus, as we have seen, he regards an excited glass tube as *positively* electrified, while the hand which rubs it becomes *negatively* electrified. Similarly he recognizes *negative* electrification in a piece of rubbed sealing-wax. It will be observed that Franklin accounts for the facts of two different electrical conditions by the hypothesis of a *single* fluid in excess or defect, while Du Fay's theory (page 320) requires two *different fluids* to explain the phenomena. Franklin's theory appeared at first to have the advantage on the score of simplicity. But some difficulties were afterwards observed. It was a necessary part of Franklin's theory that the particles of the electric fluid repel each other, but attract the particles of matter; and he explains the movement of electrified bodies by this reciprocal attraction.

It was afterwards pointed out that to make this hypothesis agree with the fact, we must assume that there exists also a *repulsive* action between particles of matter at considerable distances; or if this be held inadmissible, as indeed it is, there are certain other purely hypothetical assumptions required to reconcile the theory with facts, and such additional assumptions deprive it of the advantage of its original simplicity. Franklin's theory had the *prestige* of accounting in an intelligent way for the phenomena of the Leyden Phial, which were at this time astonishing everybody and perplexing the ablest philosophers in Europe. "The electric fluid," said he, "spreading over the interior surface of the bottle, repels the fluid which is spread over the external covering; so that while an excess of fluid is accumulated in the interior, a corresponding deficiency is maintained on the exterior." He showed that when a Leyden phial is placed on an insulated stand, it cannot be charged unless the outer coating has some conducting body sufficiently near it, when, for every spark passing between the machine and the inside, a spark will pass between the outside and the conducting body near it. A series of Leyden jars may be simultaneously charged by connecting the inside of each with the outside of the preceding one, all being kept insulated except the last in the series. It need scarcely be said that Franklin regarded the effects of the Leyden jar shock, spark, etc., as due to the passage outward of the excess of electricity accumulated in the inside when that was *positively electrified;* and to the rush inwards in the opposite case.

Some points may here be mentioned with regard to the theories of electricity, upon which great confusion exists in the minds of unscientific readers. We have seen that Du Fay recognized two *electricities*, or, if you will, two different *electric fluids*. He called one *vitreous*, because it is excited on glass by friction; the other *resinous*, because friction develops it in resins. These names at first appeared sufficiently correct as a description. But at a later period it was found that by friction between two bodies of the same kind, both electricities were developed, and that under some circumstances glass gives the *resinous* electricity. Then some other terms of distinction appeared desirable, and the words *positive* and *negative* were substituted for *vitreous* and *resinous* respectively. The reader must here carefully guard against a misconception: *positive* must not be taken to mean something actually present, and *negative* the absence of that thing. The words were applied to electricity in the first instance as the mathematical distinction of quantities. An engineer speaks of the *up* and the *down* line on a railway, without meaning thereby that the sets of rails are inclined in different directions; so that it is, in fact, merely a common agreement by which one of these terms shall be appropriated to each line of rails. So likewise the application of the terms *positive* and *negative*, as applied to the two kinds of electricities, might be reversed without any change in their meaning, provided everybody agreed to so reverse them. There is the same confusion in the meaning of these terms on account of Franklin's excess-and-defect-of-single-fluid theory. Be it observed that Franklin does not admit a positive electricity *and* a negative electricity, although he speaks of bodies being *positively* or *negatively electrified.* While these Franklinian phrases may be used when we are expressing the facts in terms of the *two-fluid* theory, we cannot, consistently with Franklin's theory, speak of the passage of *negative electricity*, etc.

One other observation with regard to electrical theories. If there is anything known at all in the present day concerning the nature of electricity, it is the certainty that *electricity is* NOT *a fluid.* This is a mere fiction which is *consciously* adopted, at least by scientific men, in order to assist the conception or description of the phenomena. The reason why electricity should be pictured as a *fluid*, while in the case of gravitation no such notion is required, will appear hereafter. The popular fallacies on the subject arise from the general tendency to assume the *expression* of a fact as its *explanation*, and to confound a *phrase* with a *principle.* As in the subsequent description of electrical discoveries, the expressions "*electric fluid*," "*flow of electricity*," etc., etc., may often occur, the reader will do well to constantly bear in mind that these expressions are nothing more than expedients to avoid prolixity. The cause or real nature of electricity is absolutely unknown. We are completely ignorant of *what* it is that acts in these phenomena, but we know something of *how* it acts.

The second claim to a distinguished place for Franklin in the history of electricity rests on his demonstration of the identity of electricity and lightning. Not that Franklin was by any means the first person to whose mind the idea occurred. The resemblances between lightning and the electric spark were too obvious to escape notice, and even in the seventeenth century these resemblances were explicitly mentioned by more than one author. The ABBÉ NOLLET in France had stated several reasons for believing that thunder and lightning were the electricity of nature. Franklin puts forward the same view hypothetically in a letter dated November 7th, 1749, and he enumerates all the points in which electricity and lightning were known to resemble each other. He notices the irregular and angular course of the lightning-flash, and observes that long electrical sparks sometimes exhibit the same appearance. Lightning strikes elevated and pointed objects, such as trees, towers, steeples, masts of vessels, etc.; and he declares that pointed conductors are more affected by electricity than smooth surfaces. Lightning inflames combustible substances; electricity can do the same. Lightning sometimes fuses metals; electricity can be made to do the same. The similarity of lightning and the discharge of the Leyden jar is seen in such effects as killing of animals, rending of certain substances, reversal of the poles of magnetic needles, etc. That the effects of lightning should be so much more powerful than those of the electric spark is simply because of the larger scale of the phenomena: if a spark of two inches long will pass with sharp report from an electrified gun-barrel, at how great a distance will the spark pass from an electrified cloud thousands of acres in extent, and how loud must be the report!

Franklin noticed with great interest the power of pointed bodies to attract electricity. He described the glow or light which is seen in the dark when a pointed body is brought near a conductor, remarking too that the sharper the point the greater the distance at which the glow showed itself. He concludes his paper by saying: "The electric fluid is attracted by points. We do not know whether this property be in lightning; but since they agree in all the particulars on which we can already compare them, it is not improbable that they agree likewise in this. Let the experiment be made." He describes the manner in which he conceived the knowledge of the power of points might be useful in preserving houses, churches, ships, etc., from lightning by fixing on the highest parts of these erections upright iron rods terminating in points. The iron rods he proposes should be gilded, in order to preserve them from rust, and should be connected with the ground (or, in the case of ships, with the water) by a wire rope. "Would not these rods," he asks, "probably draw the electric fire silently from the clouds before they come near enough to strike? and by this means might not we be preserved from many sudden and dreadful disasters?" Franklin had intended that whenever a lofty spire was erected in Philadelphia he would cause a pointed rod to be placed on the top and insulated, so that he might ascertain whether it would have the power, as he anticipated, of drawing off electricity from thunder-clouds. But before he had an opportunity of putting this idea into practice, the sight of a boy flying a kite suggested to him a method of readily and easily raising a metallic point towards the clouds. He immediately constructed a kite by stretching a piece of silk over two sticks placed crosswise, and to one of these was attached a pointed wire connected with the string, which was merely a piece of common hemp cord. At the next approach of a thunder-storm he took his kite into a field in which there was a shed, and with the assistance of his son, to whom alone he had revealed his intended experiment, he raised the kite (Fig. 160). The lower end of the string was held by a piece of silk cord, and to the hempen string was attached a small key. The experimenters placed themselves under the shed, and waited in vain some time for any manifestations of electricity, although a dark thunder-cloud had passed overhead. Just as Franklin was beginning to despair of the success of his attempt, he observed that the loose fibres of the hempen cord were diverging from each other. He brought his knuckle near the key, when a strong spark passed. Other sparks followed: a Leyden jar was charged, a shock taken from it, and, in fact, all the well-known electrical experiments were performed.

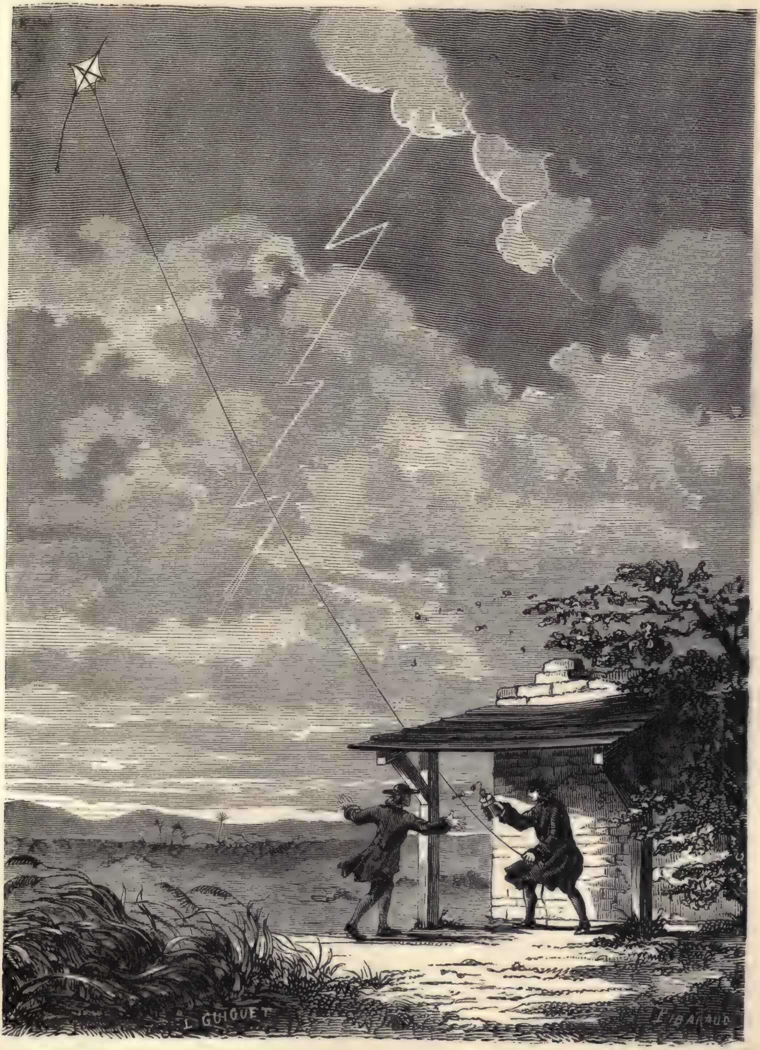


FIG. 160.—FRANKLIN'S EXPERIMENT.

Franklin was greatly moved by this now perfectly successful demonstration of the identity of the lightning and electricity, for he knew that the discovery would make him famous. This was in June, 1752.

The reason that Franklin did not at first obtain any electrical effects was that the string was dry, and in this state was a very imperfect conductor. When it became damp with the rain it conducted more freely; and perhaps, had it been thoroughly saturated with moisture, the experiment might have cost the bold philosopher his life. Franklin's practical genius perceived the advantages to be gained from this experiment. The identity of lightning and electricity was no longer an hypothesis, and some houses in Philadelphia had actually lightning conductors fixed to them a few years afterwards.

When Franklin's paper on the identity of lightning and electricity was read before the Royal Society of London, it was received with derision. In France, however, Buffon had a translation of the paper published, and thus it quickly became known, versions of it being issued in various languages. After a time Franklin's experiment began to be spoken of in England, and the Royal Society thought it would be as well to reconsider the papers which they had thought unworthy of a place in their "Transactions." One of the members of the Society verified the fact of the possibility of bringing electricity from the clouds. Thereupon the Society soon made Franklin amends for their former treatment. Without any application on his part he was elected a member, the customary subscription being dispensed with. To him was awarded the Copley medal for 1753, and the presentation was made the occasion of a very laudatory speech by the President of the Society, Lord Macclesfield. Some years afterwards, when Franklin visited England, the honorary degree of Doctor of Laws was conferred on him by the Universities of Oxford, of Edinburgh, and of St. Andrew's, and he was elected a member of all the chief scientific societies of Europe. The whole career of Benjamin Franklin has biographical interest of the highest kind. It gives an example of a man raising himself from an obscure position by the aid of such education only as he gave himself, and, though actively engaged in ordinary business, achieving a scientific fame which any of the most learned professors of his time might have envied. Beyond all this, we find Franklin taking his part in statesmanship amongst the most eminent men of that craft. At the beginning of the War of Independence he was sent by the United States as ambassador to France to seek an alliance with that country. In 1783, on the conclusion of the war, he signed the treaty of peace in which England recognized the independence of the United States. It was in allusion to Franklin's twofold fame—as a man of science and as a patriot—that Turgot composed the well-known line about him:

Eripuit cœlo fulmen sceptrumque tyrannis.

On his return to America two years afterwards he was chosen to be President of the Supreme Executive Council of the United States.

His active and honourable life ended on the 17th of April, 1790, when he was in the eighty-fifth year of his age.

Franklin at once utilized his discoveries and experiments by his plan of protecting buildings and ships from thunderstrokes. But his views were not announced without provoking doubts and raising controversies. A very warm dispute arose among the members of the Royal Society of London concerning the best form of lightning-conductors. Some supported Franklin's views as to the advantage of using pointed conductors; others, and notably Wilson, obstinately advocated the superiority of conductors furnished with a ball instead of a point. The contention was carried on for some time with no little warmth, and papers relating to it occupy a very large space of several successive volumes of the "Transactions."

In 1772 the Government requested the opinion of the Royal Society as to the best means of protecting powder-magazines from lightning. A committee of the Society, consisting of Franklin, Cavendish, Watson, Robertson, and Wilson, recommended *pointed* lightning-conductors. Wilson appended a protest, giving it as his opinion that points solicit the lightning, and that blunted conductors were the proper thing. Pointed conductors were fixed, yet it so happened that a magazine at Purfleet was struck by lightning and slightly injured, though the powder did not explode. The Government again asked the advice of the Society, and a second committee again pronounced in favour of the pointed conductors, much to Wilson's annoyance. He made experiments, drew up a long paper, which caused a lengthy discussion, with the same result as before. The Government were made to believe that the decision was not concurred in by the Society at large, and finally the question of pointed *versus* "knobbed" conductors became almost a political one. It is said that George III. even privately asked Sir John Pringle, the President of the Society, to support Wilson's views; and when the President expressed his desire always to act in accordance with his Majesty's wishes, adding, "But, Sire, I cannot reverse the laws and operations of nature," the story goes that the King replied, "Then, Sir John, you had perhaps better resign." It appears that the monarch actually had knobs put up on the conductors in his palace, and the occasion gave rise to the following epigram:—

While you, great George, for knowledge hunt,
And sharp conductors change for blunt,
The nation 's out of joint;
Franklin a wiser course pursues,
And all your thunder useless views,
By keeping to the point.

It would be tedious for the reader to peruse a mere list of the numerous discoveries in electricity which now rapidly succeeded each other, and to describe them in detail would be impossible within our limits, however interesting it might be for the student of this branch of science in particular. To the subject of atmospheric electricity the attention of everybody was drawn in 1753 by the tragical death of Professor Richman at St. Petersburg. RICHMAN (1711—1753), who was a native of Sweden, had been appointed to the professorship of experimental physics in the Imperial Academy of Sciences at St. Petersburg. He was engaged upon a work on electricity, and was very desirous of obtaining some data as to the intensity of the electricity of the air during thunder-storms. He had an iron rod fixed on the roof of his house, and this rod was connected by a chain with some apparatus which he had arranged to indicate the degree of electric action. On the day of his death, Richman had attended an ordinary meeting of the Academy, and hearing the rumbling of distant thunder, he hastened home in order to make observations with his electrometer. He took with him an engraver named Sokolow, who had undertaken to prepare the figures for the projected work, in order that the artist, seeing the actual working of the electrometer, might the better represent the apparatus in his plates. Richman was describing his apparatus, when a terrific clap of thunder alarmed the whole city, and from the rod a ball of fire leapt to the head of the unfortunate professor, who was standing at the distance of about a foot. He instantly fell backwards, dead. Sokolow was stupefied for a few minutes, but was not otherwise injured.

When the great body of facts connected with electricity had been comprised under some comparatively simple general theory or principle, such as those suggested by Du Fay, Franklin, Cavendish, and others, little progress could be made in the science as such until its chief phenomena could be under the control of mathematical laws by means of exact measurement. The person who did this was COULOMB (1736—1806), a French military engineer, who, after having at a very early age shown extraordinary capacity for the mathematical sciences, engaged in several original scientific investigations involving elaborate measurements and calculations. He was a member of the commission for the determination of those new weights and measures decreed by the Revolutionary Government, which gave the *metric system* to France and to science. Coulomb himself has, by his invention of the *torsion balance*, furnished the experimental investigator with one of the most delicate and accurate instruments of measurement that can be placed in his hands.

The phenomena to be measured were the forces which displayed themselves in the attractions and repulsions, that are among the most obvious effects of electricity. The questions to be answered would relate to the amounts of those forces under given circumstances, and to the laws of their variation by distance. The way in which Coulomb solved the problem presents one of the most elegant examples of such determinations to be met with in physical science. The research was a difficult one, because the forces to be measured were extremely small, and were at the same time liable to gradually diminish even during the progress of an experiment. Coulomb nevertheless succeeded in making accurate determinations by availing himself of a principle with which the reader should here become acquainted. Let A C, Fig. 161, be a wire or very slender metallic rod suspended from A, and hanging vertically under the influence of the small weight D. A B and C E are index fingers solidly attached to the wire, and their points move round fixed graduated circles. When such an arrangement is left to itself, the wire will come to rest in a position in which it will have no twist (or *torsion*). Suppose that its upper end A is then fixed by all movement of A B being prevented; if we turn C E round so that E shall point to one division to the right or left, a certain amount of force must be applied, because in so doing we twist the wire. Now, the thing to be here clearly understood is that to keep it *two* divisions away from its normal position, twice as much force will be required; to maintain it ten divisions distant ten times the force must be applied, and so on,—*the force always being exactly proportional to the angle through which the wire is twisted.* To put the case in a slightly different manner. Suppose in the arrangement represented in the figure we placed a peg at E, which would prevent C E from turning round; then if we turned A B, the wire between A and C would be twisted, and the pressure of E against the peg would always be proportional to the angles through which A B was turned; that is, the force is proportional to the *angle of torsion.* Now, Coulomb balanced the forces of electrical attractions and repulsions against the force of torsion, and the former could therefore be compared with each other by simply reading off the *angle of torsion.* The apparatus he employed, which is aptly enough termed a *Torsion Balance*, is represented in Fig. 162, where a cubical case of plate glass, A B C, is seen,

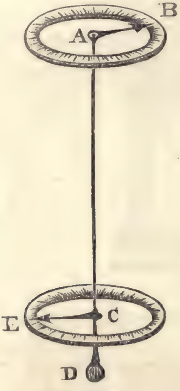


FIG. 161.

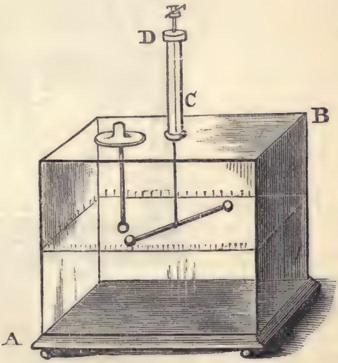


FIG. 162.

the upper plate of which is pierced with two openings: from the central one rises a glass tube, C D, which is firmly fixed. At the top of this is an arrangement to which the head of the torsion wire is fixed, and by which it may be turned round, while the number of degrees are indicated on a graduated scale. The wires used by Coulomb were sometimes extremely fine. Thus in some of his experiments a degree of torsion corresponded to a force much less than the millionth part of a grain.

The investigations of Gilbert on the natural magnet, which have been mentioned in a former chapter (page 94), furnished almost all the information on the subject that existed during the seventeenth century. Something was, however, added to the knowledge of the compass during that period. The declination of the magnetic needle, that is, the angle between its direction and that of the geographical meridian, had, during the sixteenth and seventeenth centuries, been observed and recorded for different places; and it was known that the declination was liable to a slow change, and that its amount at any given time was different at different places. The discovery of the *dip* or *inclination* of the needle was made in 1576 by Robert Norman. He was in the habit of balancing needles on their points before touching them with the magnet, and he found that when they were touched the north-pointing end of the needle always dipped below the horizon,

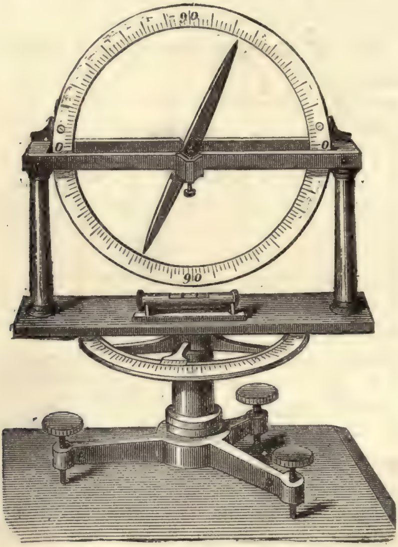


FIG. 163.—THE DIPPING-NEEDLE.

so that he had to place a counterpoise on the south end. He was advised to make an instrument for the express purpose of finding at what angle the needle would dip below the horizon when its movement in that direction was unrestrained. He thus ascertained the dip to be at that time 70° 50′. The dipping-needle, as it is called, is now constructed with great improvements, so as to obtain the greatest possible accuracy in its indication. Fig. 163 shows the dipping-needle in one of its simpler forms. The needle turns in a vertical plane on a horizontal axis which passes through its centre of gravity, so that only the magnetic forces affect its position. A graduated circle is so fixed that the amount of the dip may be read off by the position of the points of the needle. The instrument is adjusted by means of a spirit-level, which is shown on a larger scale in Fig. 164. When the instrument is used it is *first* placed so that the needle stands vertically, and in this case the plane of the graduated vertical circle will be perpendicular to the magnetic meridian. The degrees which the index on the horizontal graduated circle marks are observed, and the upper part of the apparatus is then turned through an angle of 90°, when the plane of the dipping-needle will coincide with that of the magnetic meridian, and the dip may then be observed.

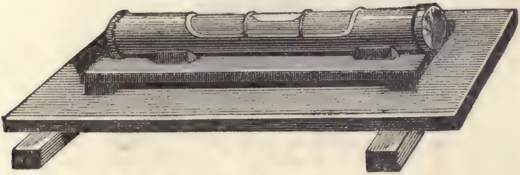


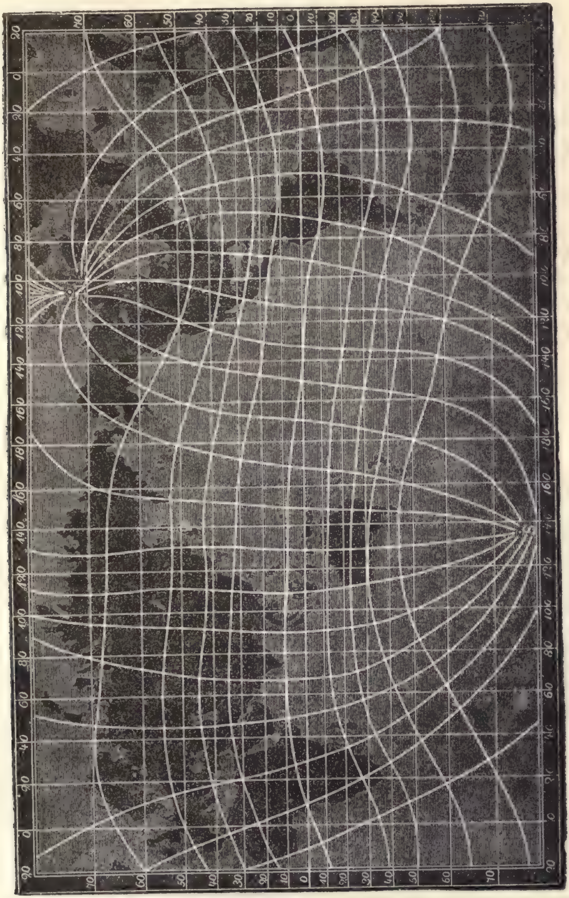
FIG. 164.—THE SPIRIT-LEVEL.

Newton, Hooke, Huyghens, Halley, and others devoted some attention to magnetism. Halley in 1698 and 1699 made two voyages expressly to investigate the variation of the compass. He traversed various parts of the Pacific and Atlantic Oceans, and made so large a number of observations, that from these and some results which others had obtained he was able to form a chart of the *variation* of the compass. Many such charts have subsequently been made; and in order to bring before the reader a general view of the variation, Fig. 166 is given as the sketch of such a chart. It represents, as will be seen, first a chart of the earth on Mercator's Projection, in which the meridians and parallels of latitude are shown by parallel straight lines. These are the fainter white lines in the diagram; the more distinct white curved lines are the magnetic curves. It will be noticed that these form two sets; one set which is in a direction in general parallel to the equator, and another set which cross the former at right angles. These last lines indicate the *variation*, for a compass at any place would

FIG. 165.—DR. HALLEY.

point, not north and south, but in the direction of the curve which passes through that place. It will be noticed that these lines converge to two points, one in the extreme north of America, the other in the circum-polar seas of the Southern Ocean. These are the *magnetic poles*, and at them the horizontally-poised needle would not be turned magnetically at all. The other set of curves are in this chart drawn to form right angles with the magnetic meridians, to which they have the same geometrical relation in this respect as the geographical parallels of latitude have to the geographical meridians. But the so-called "magnetic parallels" have no physical significance, and both the sets of lines are on modern charts replaced by two other sets, which are respectively lines of equal variation, and lines of equal dip. These are formed by drawing lines through the places where the declination is the same; and similarly for places where the inclination is 0°, 5°, 10°, etc. Thus, for example, all the places where the needle does not dip at all are situated near the earth's equator, and the line including them all would be not unlike the "magnetic meridian" shown in the figure. As we leave this line to go northwards, the north-pointing end of the needle dips; and as we leave it to go towards the South Pole, it is the south end of the needle which dips. Graham, the celebrated instrument-maker, discovered in 1722 that the direction of the magnetic needle was subject to a small daily variation of about half a degree.

The law of the decrease of magnetic force by distance occupied the attention of Newton and others, who made many experiments to discover it. Some experimenters drew erroneous conclusions from their results, through neglecting to consider the joint action of the *four*


70
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120
140
160
180
160
140
120
100
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60
40
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0
20

FIG. 166.—CHART OF MAGNETIC CURVES.

*poles.* This was the case with Musschenbroeck, Hawksbee, and Taylor, and the real laws remained unknown until the classical investigations of Coulomb, which were made by means of the torsion balance, the general principle and construction of which have been already explained (page 335). Coulomb thus demonstrated that the magnetic forces vary inversely as the squares of the distances. He also investigated the distribution of the magnetism in a bar, the effect of heat on magnets, and many other subjects relating to the magnetic forces. The torsion balance was used by Coulomb in all such inquiries with singular advantage. Canton, Duhamel, Ritterhouse, Scoresby, Æpinus, Poisson, and Barlow are perhaps the most eminent and successful of the other investigators who contributed to the knowledge of magnetism. The study of the variations, changes of intensity, dip, etc., of the compass, besides the interest of the subject in itself, showed that the magnetic condition of the earth is connected with other phenomena which present themselves on the large scale. It has been perceived that the study of terrestrial magnetism may lead to the discovery of laws and relations between the cosmical forces—laws and relations of which but an imperfect notion has yet been formed. Many are the observations that have been already made in various parts of the world, and every scientific expedition adds to their number, for arctic and other observers are always sent out with instruments for magnetic experiments. Fig. 167 represents the use of the magnetometer in very high latitudes.

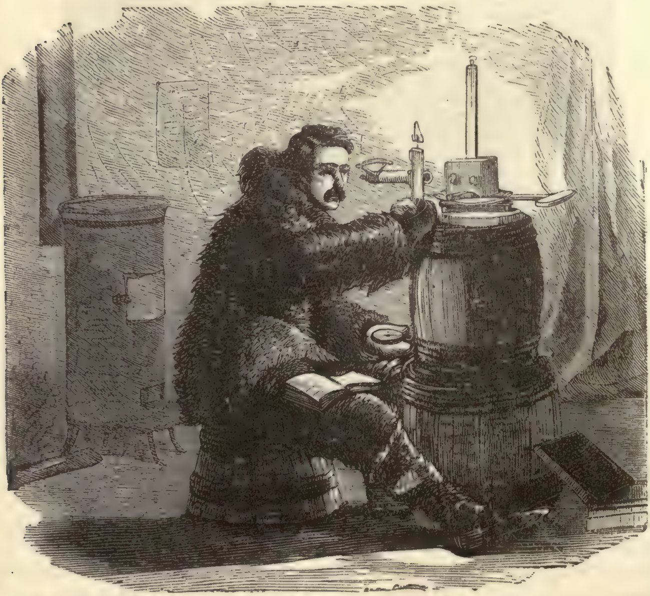

FIG. 167.—A MAGNETIC OBSERVATION.

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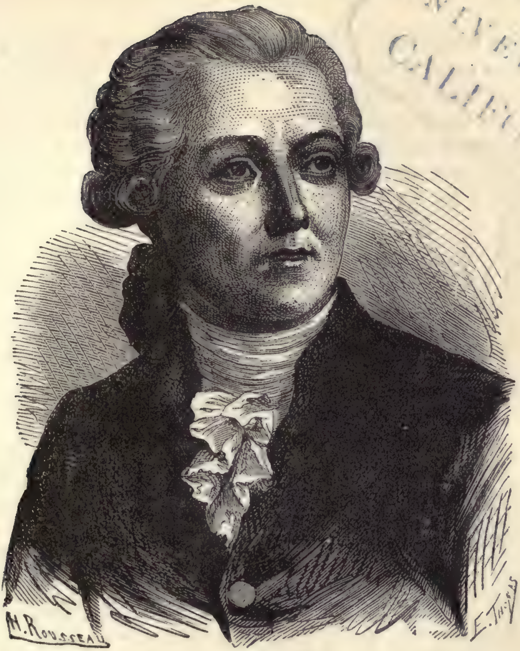

FIG. 168.—LAVOISIER.

## CHAPTER XIV.

### CHEMISTRY OF THE EIGHTEENTH CENTURY.

THE researches of Van Helmont, Boyle, and Mayow had the effect of greatly attracting the attention of chemists to gases. It would almost seem as if they had instinctively foreseen that the whole science would ultimately rest upon the knowledge of gases. It would not be easy in the present day for any man of ordinary education to realize to himself the darkness which at the beginning of the eighteenth century concealed the whole question of gases even from the best intellects. Researches had to be made to obtain even the most elementary knowledge of the properties of the gases in mines, and of those produced in fermentation, respiration, and combustion. In the earlier part of the eighteenth century no one more contributed to increase our knowledge of gases than STEPHEN HALES (1677—1761), by whose labours also juster notions of the economy of plants were promulgated.

It was Hales who first elucidated the functions of the sap. But his name will be ever remembered in chemistry as the inventor of the *Pneumatic Trough*, an apparatus simple enough, but of the greatest importance to the progress of chemistry. As we have already seen, a few experimenters had succeeded in collecting a "*certain kind of air*" (hydrogen) by inverting a bottle filled with dilute acid over pieces of iron immersed in an open vessel of the same liquid. By Hales's arrangement the gas generated in one vessel could be collected in another, and the power which this gave to the chemist of treating the materials in his generating-vessels in any manner he thought fit, will be at once understood from the woodcut representing the apparatus, which is copied in Fig. 169, and renders a detailed description unnecessary. The tube of the retort on the furnace is turned up so as to pass loosely into the neck of a bottle suspended mouth downwards, and completely filled at the beginning with water, which it retains in its inverted position in consequence of its mouth dipping below the surface of the water in the pail. The tube leading the gas from the place where it is generated to the mouth of the inverted vessel is therefore the special feature of Hales' invention. The "pneumatic trough" used at the present day differs from Hales' apparatus only in having a more convenient arrangement of its parts.

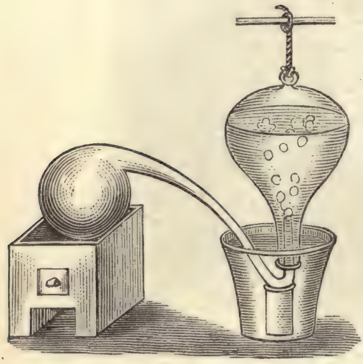

FIG. 169.

Though Hales invented the most convenient apparatus in which gases can be collected, he does not seem to have himself discovered and recognized any gas as such. Yet he actually collected gases resulting from the distillation of wax, etc.—gases which would not be very different from common illuminating coal-gas—and he found that these were inflammable. Unfortunately, a preconceived idea blinded him to the real facts which his experiments disclosed. He considered the inflammable gas to be *air*, owing its inflammability only to the sulphurous and oily vapours with which it was impregnated. It was a settled theory with Hales that atmospheric air was the elementary principle by which the particles of all bodies were bound together.

Many of Hales' experiments were repeated by BOERHAAVE (1688—1738), the celebrated Dutch physiologist, with whom chemistry was a favourite study. He was one of the first to treat the question of the manure applied to land as one based upon scientific principles. He

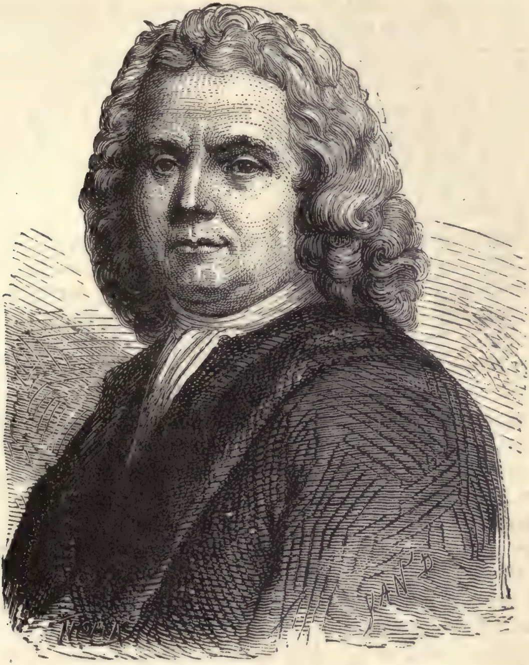

FIG. 170.—BOERHAAVE.

published a treatise on chemistry which was translated into the principal languages of Europe, and was long the established text-book in all the medical and other schools where chemistry was systematically studied. Boerhaave was the first to give to the study of the chemistry of animal and vegetable substances such attention as their importance in the economy of nature demands.

The compounds of magnesia were first distinguished from those of calcium in a treatise published at the beginning of the eighteenth century. FREDERICK HOFFMANN, a German physician of some celebrity, showed that the well-known Epsom salt did not contain lime, but a kind of alkaline calcareous substance different from lime, united with sulphuric acid. Epsom salts had before this period been called *nitre*, a term vaguely used by reason of the form of the crystals, or, for distinction, *calcareous nitre*. When BLACK (p. 286) began to make experiments in chemistry, he was desirous of more closely examining the alkaline earth that Hoffmann had described. The results of his experiments suggested to him a satisfactory explanation of the action of quicklime on alkaline salts, that is, on potashes, etc. (*carbonates*); and, in short, he found himself engaged in researches which were destined to shed a light on some very important points in chemistry. About this time (1754) two medical professors at Edinburgh were engaged in a lively discussion as to whether lime-water made with ordinary lime from limestone, or that made from lime prepared by calcining oyster-shells, were the more efficacious as a medicine. Black was an auditor of these debates, and he determined to make experiments himself, with a view of finding in what manner the various alkaline earths differed from each other.

Black began by preparing magnesia (*i.e., magnesium carbonate*) by mixing a solution of Epsom salts with one of potashes (*potassium carbonate*), when a white powder falls, which, when washed and dried, was distinguished by Black from lime by the presence of the following characters: It effervesces with acids; it forms with them compounds quite different from those which lime produces with the same acids; it precipitates lime from its combination with the acids; when strongly heated, it does not produce quicklime, but a substance insoluble in water. In this last experiment Black's attention was drawn to some remarkable differences between the common magnesia (*carbonate*) and a product obtained by its calcination (*magnesium oxide*). The calcination produced a remarkable reduction in the bulk of the white powder, and a still more remarkable diminution of its weight, for every twelve parts were reduced to five. After calcination, too, it dissolved in acids without effervescence, although the salts that it formed with the several acids were not different from those formed by the uncalcined magnesia. Black now asked himself this question: What is the substance which is removed by the action of heat, and the loss of which causes the diminution in the bulk and weight of the magnesia? In order to find an answer to this query, Black put a weighed quantity of magnesia (*carbonate*) into a glass retort, which was connected with a receiver surrounded by cold water, in order to condense into a liquid any vapours that might be driven off by the heat. He heated his retort to redness, and in the receiver he obtained only a "very small quantity of watery fluid," yet the magnesia had lost the greater part of its weight. He was astonished at this, and Hales' favourite theory suggested itself to his mind, namely, that air had been expelled from the magnesia by heat, and this appeared to be confirmed by the fact of acids expelling air (*carbonic acid gas*) from the uncalcined magnesia. In order to test this theory, he proposed to restore, if possible, to the calcined magnesia the air it had lost. In thinking how this was to be done, he first asked himself: From whence did the magnesia originally receive the air? It was expelled when the magnesia combined with sulphuric acid, so that it did not exist in the Epsom salts, and *therefore it must have received this air from the alkali* (carbonate of potash) *which was used to precipitate it.*

Very notable is Black's next experiment, because it shows that systematic appeal to the balance which constitutes the law of modern chemistry. Here, in the middle of the eighteenth century only, do we find chemistry seeking a basis for its principles in quantitative relations. This was the first step of the great transformation of the science from its elementary condition of a mass of facts, connected only by vague theories, into that state in which its general principles rest on the incontrovertible basis of definite quantitative relations. Black tells us that he took 120 grains of common magnesia (*carbonate*), which he calcined in a crucible, so that it lost 70 grains of its weight. The magnesia thus calcined was then dissolved in a sufficient quantity of dilute vitriolic acid (*sulphuric acid*), the solution taking place without effervescence. To this liquid was added a hot solution of fixed alkali (*carbonate of potash*), and the precipitate produced was collected, washed, dried, and weighed. In this state it had recovered, except a mere trifle, the whole of the weight it had lost by calcination, and in every respect it behaved like common magnesia. This experiment confirmed him in the idea that the magnesia receives from the alkali used in the precipitation a certain quantity of air. Soon afterwards Black made an experiment in which he first prepared the gas we now call carbonic acid by the process still in use. He put a little alkaline salt (*carbonate of potash*), or lime (*i.e., old lime or mortar which contains carbonate of lime*), or magnesia (*carbonate*), into a flask containing a little dilute acid, and closed the flask with a cork through which passed a glass tube bent like a swan's neck, dipping beneath the mouth of an inverted vessel, as in Hales' method (page 342). He soon saw a brisk effervescence, and many bubbles of "elastic aërial matter" (*carbonic acid gas*) rose up to reach the surface of the liquid within the vessel. "It is not," he remarks, "a momentary vapour which escapes, but a permanent elastic fluid (*gas*) not condensable by cold." Black called this "*elastic fluid*" *fixed air*, because it had been fixed in a solid condition in the magnesia. It soon afterwards received the name of *carbonic acid gas*, which it has retained to this day.

In the same year, viz., 1757, Black discovered that carbonic acid gas (as we shall now call it, instead of following Black by calling it *fixed air*) cannot be breathed by animals, and that it had even a poisonous effect on them. Sparrows introduced into an atmosphere of the gas died in ten or eleven seconds, but if their nostrils were stopped with lard before they were placed in the gas, their death took place only at the end of three or four minutes. He declares his opinion that the change which is made by respiration in originally pure air consists chiefly if not entirely in the transformation of a part of the air into carbonic acid, and he describes the now well-known experiment of blowing through a tube into lime-water, which becomes turbid by the precipitation of the lime (*carbonate*). This experiment, which should be familiar to every one, may be repeated with no other apparatus than a clean wide-mouthed bottle. Pour a little clear lime-water into the bottle, cover the mouth of it with the palm of the hand, and shake the bottle vigorously. The lime-water will remain clear. Now breathe into the bottle, quickly cover with the hand, and shake as before. The lime-water will immediately become turbid from the presence of carbonate of lime, formed by the combination of the lime with the carbonic acid of the breath. If the bottle be held for an instant mouth downward over a lighted candle, and lime-water be immediately introduced and shaken up in it, the effect will be the same as with the breath.

Black soon found that the air (*gas*) produced in fermentation is carbonic acid, which in fact had been already recognized in these circumstances by Von Helmont, and described by him under another name (page 230). The same day on which Black made this observation, he proved by means of lime-water that the combustion of coal gives rise to carbonic acid gas as Von Helmont had anticipated. Black's researches establish the fact that the alkalies (*potashes*, or carbonate of potash) contain a certain quantity of carbonic acid, which is expelled by the contact of an acid, such as sulphuric acid, but that the strongest heat fails to expel the carbonic acid from its combination with potash. On the other hand, while lime combines with carbonic acid, which a stronger acid also expels, the gas is completely driven off from its calcareous combination by a moderate heat, and the result of the calcination is *quicklime*, which is caustic, and when exposed to the open air absorbs carbonic acid from it, and thereby loses its causticity. The other caustic alkalies are also in some measure neutralized by absorbing something from the air. Black knew that his "fixed air" (*carbonic acid*) constituted only a portion of the atmospheric air, and that it is this portion which causes a crust to form on the surface of lime-water exposed to it.

A Dublin physician, Dr. Macbride (1726—1778), developed in some respects the views of Black as regards the part played by carbonic acid in the animal economy, and his Essays had at least the merit of attracting the attention of physiologists to this subject. He pushes the theory of carbonic acid as a consolidating material to an extreme, and even bases his medical practice, or fancies that he does, on these slender theoretical foundations. He declares that all bodies owe their strength and consistence, the cohesion of their parts, to the fixed air they contain, and that putrefaction is the result of their losing this fixed air. It is a fact that putrefying bodies disengage carbonic acid gas, but Macbride's inference was erroneous when he concluded that this was the *cause* of their decay, whereas it is in reality one of the *effects*. He recommends scorbutic persons to drink liquids containing carbonic acid, because their disease is a putrid malady caused by the want of that principle which is the bond and cement of the body. On the other hand, clear as were Black's doctrines and convincing his experiments, they were far from receiving immediate assent. Thus Meyer, a German apothecary, while constrained to admit Black's facts, opposes his own theory to that of the eminent Scotchman, and argues in its favour with sufficient plausibility to find adherents. According to him, if limestone is rendered caustic by heat, it is not because it has parted with "*fixed air*," but because it has absorbed from the fire a particular kind of acid, which Meyer calls *acidum pingue.* It might be objected to this theory that the limestone loses weight in the fire instead of gaining it. The theorist is ready with the reply that *acidum pingue* is a substance similar to heat and light, or that it is anti-gravitating, etc. But it may be objected to the theory that *acidum pingue* cannot be produced and proved to have a real and independent existence. No one can for a moment doubt of the validity of this objection in determining his choice between Black's and Meyer's theories; yet *acidum pingue* is not the only imaginary existence to which theoretical plausibility has given acceptance. Caloric, elective affinity, vital force, the luminiferous ether, and many other very respectable theoretical entities—*id genus omne*—are liable to the same objection. But *acidum pingue* has fallen into merited oblivion. It may be interesting to see, by a quotation from a paper presented to the Academy of Sciences in 1764, how troublesome facts are when they do not fit the theories. The writer shows that when lead is converted into litharge (*oxide of lead*), a real increase of weight takes place, amounting to about one-eighth. "This is," he remarks, "truly a paradox, though experiment puts it beyond all doubt. But though it is easy to establish the fact, it is not so easy to give a satisfactory explanation of it; it eludes all the physical notions which we possess, and only from time can the solution of this difficulty be looked for." The resolution to wait for the knowledge that time might bring was wiser than attempting to warp the facts; but had the chemist (Tillet), who so clearly saw the incongruity of the fact with the phlogiston theory, had but the courage to relinquish this theory altogether and seek for a better one, he might have gathered some of the laurels which fell to the lot of Lavoisier a few years later.

Among the English chemists who continued the investigation into the nature of "fixed air" (carbonic acid gas), none surpassed in the precision and success of their researches the celebrated HENRY CAVENDISH (1731—1810). He was the son of Lord Charles Cavendish, the brother of the third Duke of Devonshire. Lord Charles Cavendish died in 1783, when he was the senior member of the Royal Society, which he had entered in 1727. He was a good mathematician, and made some barometrical determinations that were considered of value. His son inherited these scientific tastes, manifesting at an early age a decided taste for scientific pursuits; to which, indeed, he ultimately devoted himself with ardour, sacrificing every ordinary ambition, and contenting himself with a very moderate patrimony. Unexpectedly he came into the possession of a large fortune after he was forty years of age. He became a member of the Royal Society in 1760, and for a period of nearly fifty years he continued to enrich the "Transactions" with valuable papers on chemistry and on some departments of natural philosophy. The accession to his large fortune did not alter Cavendish's simple habits of life. He had, in his youth, accustomed himself to the utmost regularity, and to his last day he never allowed this regularity to be deranged. He attended the meetings of the Royal Society with the greatest punctuality, and his appearance in it constituted the whole scene of his public life. The seclusion in which he lived no doubt caused his name to be less widely known than his scientific merits deserved, but his reputation has always been high among the *savans* of Europe. Cavendish's first paper was a dissertation on carbonic acid gas; but his chemical renown rests mainly upon his researches on hydrogen, the composition of water, that of nitrous acid, and of atmospheric air. Unfortunately, Cavendish had been brought up in the doctrines of the phlogistic school of philosophers, and the prepossession of his mind with these ideas prevented him in many cases from obtaining just inferences from his well-arranged experiments. The authorship of the discovery of the true composition of water has been claimed for several chemists; but the honour of this, like that of many other great discoveries, must in justice be distributed amongst the individuals who contributed the most important steps. Among these were the preparation and the study of oxygen, for the completion of which the several labours of Scheele, Priestley, and Lavoisier were required.

To whom belongs the merit of having *first* experimentally proved the compound nature of water has been again and again the subject of contention. It would be useless to enter here upon a discussion of the various claims to priority which have severally been advanced on behalf of Cavendish, Priestley, Watt, Lavoisier, and others; we may, however, notice some instances in which the fact was observed, but its significance and importance not clearly appreciated, until Lavoisier had distinctly declared the truth.

PIERRE-JOSEPH MACQUER (1711—1784), a French chemist, who published a "Course of Chemistry" and a "Dictionary of Chemistry," described in an edition of the latter work revised in 1781 an experiment which really consisted in forming water from its elements; but he did not follow up the clue he had obtained by any further investigation into the matter. As the experiment is instructive, interesting, and simple, it is represented, together with a portrait of Macquer, Fig. 173: in it is seen a bottle containing diluted sulphuric acid and zinc, and furnished with a bent tube, from the orifice of which the hydrogen issues. The gas having been lighted, a cool porcelain saucer was held near the flame, when the surface of the saucer became moist by a deposit of drops of water. Macquer thus describes his experiment: "I also assured myself, by placing a white porcelain saucer in the flame of the inflammable gas which was burning quietly at the mouth

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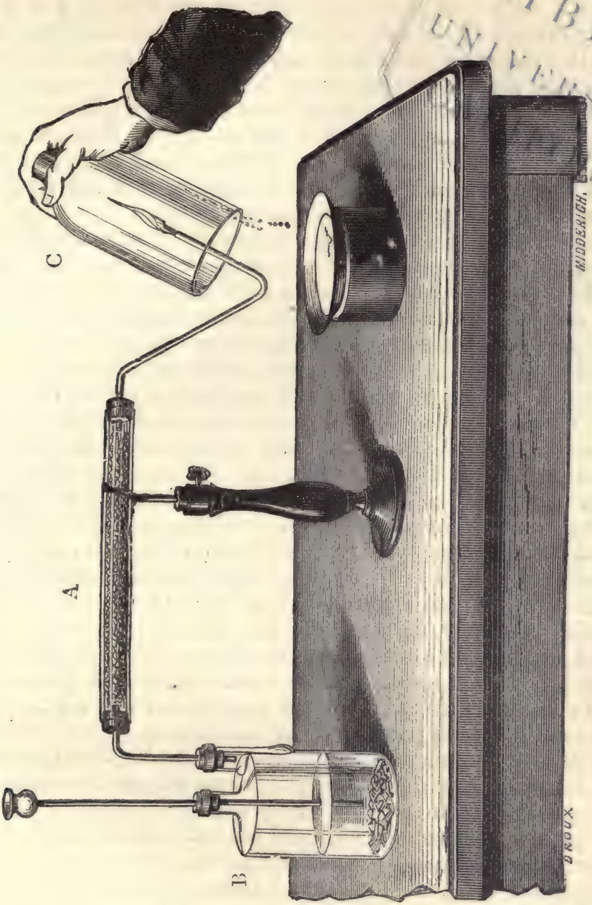


FIG. 171.—IMPROVED FORM OF MACQUER'S EXPERIMENT.

of a bottle, that this flame is accompanied by no smoke, for the part of the saucer touched by the flame remained particularly white, only it was wetted with drops of a liquid like water, *which indeed appeared to me to be nothing else than pure water.*" He did not propose any explanation of this deposit of water in this experiment, which appears to have been made in 1776. This experiment is always shown on the lecture-table in courses of experimental lessons in chemistry, and the more modern arrangement of the apparatus shown in Fig. 171 renders the experiment much more convincing. The hydrogen gas is generated in the bottle B, and before arriving at the jet where it is burnt, it traverses the tube A, filled with chloride of calcium, by which it is thoroughly dried. In the original form the experiment is open to the objection that the water given off from the flame may have been simply carried over by the stream of gas from the generating-bottle. C in the figure is a cold dry glass bell-jar held over the flame. The water immediately bedews the inner surface, and soon runs down in drops. We shall presently see how Lavoisier accomplished the direct union of oxygen and hydrogen gases in such a manner that the true composition of water was demonstrated beyond all cavil.

At the end of 1781 an Englishman named WALTIRE exploded a mixture of air and hydrogen by means of electric sparks, and he noticed the deposition of moisture. In 1782 and 1783 Cavendish and Priestley severally repeated these experiments, and Watt suggested that they afforded indications that air was not an elementary body; but he put forward these views with hesitation, and even withdrew his paper when a portion of it only had been read before the Royal Society. We may see in Watt's diffidence how strongly the notion of the elemental nature of air still possessed men's minds, when he feared publicly to call it in question until he had conclusive experiments to adduce in support of his own views. The phlogistic doctrine gave such a form to the ideas of Cavendish, Priestley, and Watt, that they failed to draw from the experiments with which they were acquainted those conclusions which Lavoisier, who had liberated himself from the illusory theory, about the same time, viz., 1783, clearly and definitely enunciated. In fact, by the year 1784 Lavoisier had distinctly demonstrated the composition of water, not only by producing the liquid from its elements, but by the separation of these elements from the liquid. The mode in which he arranged his experiments will be described when we come to speak particularly of his labours.

At the very period when the overthrow of the *phlogiston* theory was approaching, several of the eminent chemists whose discoveries hastened its downfall were nevertheless its attached partisans, and still strove to put the new wine into the old bottles. Sweden had by this time become conspicuous by the distinguished cultivators of science she fostered, and in the development of chemistry she seemed destined to play a leading part. Two Swedish chemists of the epoch which now occupies our attention must here be particularly mentioned. These are Bergmann and Scheele.

TORBERN BERGMANN (1735—1784) was born at Catherineberg, where his father was collector of taxes. At seventeen years of age Bergmann became a student at the University of Upsal, and there he devoted himself with ardour to the study of mathematics and natural science. At the age of twenty-six he was appointed to a professorship of mathematics, but six years afterwards exchanged his chair for one of chemistry and mineralogy, and in his new capacity he strove to accomplish as much for chemistry as the great Linnæus had done for natural history. His reputation soon spread over Europe, and he was made a member of all the scientific societies. His teaching attracted students of all nations to Upsal. Bergmann's chemical investigations were conducted with something of the rigour of mathematical demonstrations. His works are very numerous, and they include original papers not only on chemistry and mineralogy, but also on geology, astronomy, and physics. The chemical works of Bergmann, with which we are here chiefly concerned, were first his study of the properties of Black's "*fixed air*" (carbonic acid), to which he gave the name of *aërial acid*, for he proved that this substance played the part of an acid. He completed Black's researches by showing that the causticity of the alkalies prepared with quicklime (p. 63) depends upon the fact of the quicklime depriving the alkali (carbonated alkali—*i.e.*, potash, soda-ash) of carbonic acid, and that these caustic alkalies return to their original condition when exposed to the air, because they absorb carbonic acid from it. But there is a notable feature in Bergmann's research, namely, the determination of the *proportions* in which "aërial acid" (*carbonic acid*) enters into the composition of "aërated salts" (*carbonates*). The process he used is one which is still resorted to in the analysis of carbonates. He dissolved a weighed quantity of mild alkali (carbonate) in water in a flask, corked it, and weighed the whole. Another smaller flask containing some acid was also weighed. The acid was then very gradually added to the alkali, with certain precautions to avoid any loss by any liquid carried away during the effervescence. The total loss of weight of the two flasks together would represent the weight of carbonic acid expelled. By evaporating the liquid and heating the residue so as to expel the surplus acid, Bergmann was able also to find the weight of the product, and thus infer the weight of the air required to saturate the alkali. Here is a table of one of his results, but we give the *modern names* of the substances instead of those used by Bergmann :—

100 parts of caustic soda require for their saturation
177 parts of sulphuric acid,
135 ,, nitric acid,
125 ,, hydrochloric acid,
80 ,, carbonic acid.

100 parts of caustic potash require
78 parts of sulphuric acid,
64 ,, nitric acid,
51 ,, hydrochloric acid,
42 ,, carbonic acid.

Bergmann also gave the first analysis of carbonates, such as those of calcium, barium, and magnesium, and made several important observations on their properties. Bergmann had a just opinion of the composition of the atmosphere, which he thus describes: "Common air is a mixture of three elastic fluids; that is to say, of free aërial acid (*carbonic acid*), but in quantity so small that it does not perceptibly change the colour of blue litmus; of an air (*nitrogen*) which is unable to maintain either combustion or the respiration of animals, which we shall call vitiated air until we are better acquainted with its nature; lastly, of an air (*oxygen*) absolutely necessary to combustion and animal life, which constitutes about a fourth part of common air, and which I regard as pure air." In short, Bergmann may be considered to have made a tolerably complete study of carbonic acid gas, excepting its composition and its liquefaction by pressure.

The methods of analysing minerals by solution, precipitation, filtration, dessiccation, etc., were first reduced to precise rules by Bergmann. He accurately analysed cast iron, wrought iron, and steel, and showed how the properties of these substances are affected by their composition.

It is said that when Bergmann was once complimented on his many brilliant discoveries, he observed that his greatest discovery was the discovery of Scheele, which is thus related:—Bergmann had received some saltpetre from a druggist, and in making use of it found it liberated red fumes. He attributed this to impurity in the salt, and accordingly sent it back to the druggist by one of his pupils. The druggist's assistant, however, explained how this was caused by a certain decomposition of the saltpetre, which invariably took place under certain circumstances. When this was reported to Bergmann, he hastened to the shop, and to his surprise and pleasure discovered that the apron of the druggist's assistant covered a profound and accomplished chemist of rare intelligence, who had already discovered a number of facts new to the science.

Charles William Scheele (1742—1786) was born at Stralsund, and at fourteen years of age was apprenticed to a druggist at Gothenburg, and he afterwards acted as assistant in various places before he came to Upsal, where he became acquainted with Bergmann and Linnæus. Scheele's important researches soon made his name famous. Several attractive invitations were made to him to quit his obscure occupation as druggist. Thus, Frederick the Great wished him to reside in Berlin. But to these offers he turned a deaf ear, and continued in his original vocation. His desires were moderate, and a pharmaceutical business being vacant in Kœping, a small town in Sweden, he entered upon it, and continued in that place for about nine years, or until his death at only forty-four years of age. It was during his residence here that Scheele published his most important researches in a series of *mémoires* printed by the Royal Society of Stockholm. Scheele was remarkable for a rigorous application of the methods of experimental science, and he was careful always to verify his facts. But when he attempted to explain these facts he plunged immediately into the slough of speculation which was represented by the doctrine of phlogiston, and all his scientific caution on other points did not hinder him from believing he had proved that phlogiston really existed, that it combined with matter and passed from its combination with one body to join another, that caloric (*heat*) is the result of its combination with inflammable air, etc. Bergmann had charged Scheele to investigate the composition of air, and in his researches on this subject, when he is dealing with facts, Scheele shows himself admirably qualified for the task. The arrangement by which he determined the proportion of "dephlogisticated air" (*oxygen*) contained in the atmosphere is shown in Fig. 172. A bell jar containing air is inverted in the vessel A over water. A tall stand within B supports a small cup C, in which is placed a mixture of iron-filings and sulphur moistened with water. The capacity of the vessel B was determined, and marked on a scale E on the side of the bell jar for various levels of the water within, allowance being made for the space occupied by the stand. After some time the oxygen of the air is completely absorbed. Bergmann thus estimated that $\frac{9}{33}$rds of atmosphere consisted of oxygen.

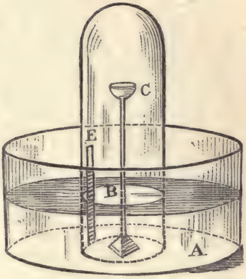


FIG. 172.

In mineral chemistry Scheele is distinguished by his study of a substance which was then called *magnesia nigra*, *i.e.*, black magnesia. This is no other than the substance we now call black oxide of manganese. He found, on heating this substance with sulphuric acid, a pinkish-white salt (sulphate of manganese) was the product. He found also that during the solution of the black oxide of manganese in the acid, gas was evolved, which was no other than oxygen. In trying the action of the several acids on the black oxide he came to hydrochloric acid, or, as it was then called, "muriatic acid;" and the gaseous body evolved when these substances are heated together could not be overlooked. But let us hear his own account of the matter:—"I poured," he says, "an ounce of muriatic acid on half an ounce of powdered black magnesia. This mixture, left to itself in the cold, became yellow coloured, and on warming it a strong smell of *aqua-regia* was perceptible. In order to study this phenomenon, I proceeded in this way: I fastened an empty bladder on the end of the tube of the retort, which contained the mixture of black magnesia and muriatic acid. As the effervescence of the liquid proceeded the bladder swelled up; and when it was filled I removed it, and found that the gaseous substance it contained had coloured it yellow, just as *aqua-regia* would have done. The gas was not fixed air (*carbonic acid gas*); its smell was extremely powerful and penetrating, and it particularly affected the nostrils and the lungs. In fact, one would have taken it for the vapour of *aqua-regia*." Scheele afterwards collected this gas in bottles according to Hales' method (p. 342), and gave a description of the properties of the new substance. It corrodes the corks, turning them yellow; it turns coloured flowers white; it thickens oils and fats; its solution dissolves iron; the gas attacks many metals, and combines with *the fixed mineral alkali* (caustic soda), forming thereby common salt (*chloride of sodium*). A reader acquainted with the merest elements of chemistry will observe that Scheele had discovered chlorine. But that was not the name he gave it. He was entangled in the meshes of the phlogiston theory, and he supposed that the action in the retort consisted in the "black magnesia" depriving the muriatic acid of its phlogiston. He gave, therefore, to the new gas the name of "*dephlogisticated muriatic acid.*" The discovery of chlorine, which will ever be associated with the name of Scheele, was made in 1774.

Scheele also either first discovered the following bodies, or first thought over and studied their nature and properties:—arsenic acid, tartaric acid, malic acid (the acid of apples), crystallized citric acid, fluoric acid, lactic acid, Prussic acid, compounds of tungsten and molybdenum, etc., etc.

The great representative of English chemistry immediately after Cavendish was PRIESTLEY (1733—1804), a remarkable man among the many remarkable men who appeared in the latter part of the eighteenth century. Joseph Priestley was born at Fieldhead, near Leeds, where his father was a dresser of woollen cloth. Priestley was educated at a public school, and by the time he was six years of age he had made considerable progress in Latin, Greek, and Hebrew. As he had always been fond of books, his friends hoped that he would ultimately become a Nonconformist minister, which in fact he did. Though he devoted much time to theological studies, yet by the time he was twenty-three years of age he had acquired also a knowledge of French, Italian, German, Chaldee, Syriac, and Arabic; and had besides written some religious essays. In 1755 he was appointed as assistant minister to some small chapel in Suffolk, where his income did not exceed £30 a year. He soon found, however, that his theological views were not relished by his congregation, for he began to deviate from the rigorous system of doctrines which the sect affected, and accordingly his hearers speedily fell off. He became, too, a marked victim of the *odium theologicum* of the ministers of other bodies, and when he became a candidate for a vacant meeting-house at Sheffield he was rejected. He succeeded better at Nantwich, where in 1758 he was appointed to a chapel, and where he increased his income by keeping a school and by giving private tuition. He taught in the school from seven in the morning till four in the afternoon, and gave private lessons from four to seven in the evening; and yet he contrived to find time to write his English Grammar. In 1761 he succeeded to the office of teacher of languages in a large academy at Warrington. At Warrington he married, and remained there six years, and published essays on a Liberal Education, Government, Biography, etc. On a visit to London he was introduced to Benjamin Franklin and others, by whom he was encouraged to undertake the composition of "A History of Electricity," which work first brought him into notice as a scientific man, for many of his own experiments are described in the book. In 1767 he removed from Warrington to take charge of a chapel at Leeds, and for another six years he remained at Leeds, where he wrote many works, and began his career as a chemist, which soon raised him to the highest position in the ranks of British men of science. It was while residing in Leeds that Priestley discovered oxygen, and this discovery is the brightest jewel of his scientific crown. His researches on carbonic acid and nitrous oxide were also of high importance. Priestley's mental activity was by no means entirely absorbed by his chemical investigations; he was also engaged in metaphysical disputations and theological polemics, and wrote besides a "History of Optics." Priestley's career at this time is a fine instance of a man's inner power triumphing over his circumstances. His position was a contrast to that of Cavendish: an obscure dissenting minister struggling to maintain a family on the slenderest of incomes, his progress in his profession checked by a defect of his voice and the unpopularity of his theological opinions. His invention, however, was stimulated to obviate the smallness of the means he could command in the purchase of apparatus by ingenuity in the construction of new and cheap instruments of investigation. His methodical regularity rendered it easy for him to register every new fact he observed, and the ardour of his scientific curiosity made him a watchful observer. He wrote with care, and his experience in teaching had trained him to a certain clearness of expression.

The scientific celebrity which our dissenting minister had acquired induced Lord Shelburn (afterwards Marquis of Lansdowne) to invite Priestley to take up his residence with him nominally as librarian, with a salary of £250 a year. He accepted, and accordingly left Leeds for Wiltshire, where he completed his experiments on the different kinds of air. His controversial publications on divinity and metaphysics were not suspended, and some of his treatises found for their refutation active employment for theologians of a polemical turn.

It was probably on account of Priestley's religious and political opinions that the relations between his patron and himself soon came to an end. Priestley had been elected to the membership of the Royal Society in 1767, and the Copley Medal was awarded to him in 1772. On leaving Wiltshire, he took charge of a chapel at Birmingham, and at this place associated on terms of intimate friendship with Watt and Wedgwood. He was one of the small band of *illuminati* who called themselves the "Lunar Society." Priestley, when denouncing in his works the selfish passions that corrupt society, often maintained that a high ideal morality must make men better; and in reviewing the progress which had been made in natural science during the years of the eighteenth century that lay behind him, he remarked that he believed other changes were destined to take place which would still more affect the progress and the happiness of mankind. Perhaps it was such expressions as these that suggested to some of those concerned in that great convulsion, the French Revolution, that Priestley should be honoured at their hands! It is certain that he received the title of Citizen of the Republic, and was even made an honorary member of the National Convention. These nominal distinctions brought disaster upon the unfortunate philosopher. When the minds of men were unsettled by the progress of events in France, the fury of a Birmingham mob was aroused against Priestley and some of his political friends, and on the 14th of July, 1791, the populace attacked a place where a few of these friends were assembled to celebrate the anniversary of the downfall of the Bastille; and when they had spent their fury upon the house of meeting, they proceeded to Priestley's house, which he on that day had not quitted. Priestley took refuge in a neighbour's house, and witnessed the wreck of his dwelling, which was set on fire. His library, his scientific instruments, his manuscripts, were soon turned into a heap of ashes, and he had probably a narrow escape with his life. He removed to London, but found that he was in some sense a marked man. He entertained, rightly or wrongly, the opinion that the Birmingham mob was artificially excited by concealed agents of the Government of the day. He was therefore again compelled to carry forth his household gods, and as under such circumstances his residence in England became unendurable, he quitted for ever his native land, and, in his sixty-first year, embarked in 1795 for America. He died in 1804 at Northumberland, a small town on the Susquehanna, a few hours after he had arranged all his literary concerns and inspected the proof sheets of his last theological work.

It was in 1772 that Priestley published his first "Observations on Different Kinds of Air" in the "Transactions of the Royal Society." It will be readily understood from what has gone before, that the expression "different kinds of air" means, as we say now, the different *gases*. The first gas studied by Priestley was carbonic acid, and he tells how, when living near a brewery, he became curious about the nature of the gas which was given off during fermentation. His experiments with carbonic acid did not add much to the knowledge of fixed air, except in respect to his discovery that, when condensed by pressure, a much larger quantity of the gas is absorbed by water. He was thus, in fact, the discoverer of the method of preparing the well-known beverage, soda-water.

"There are, I believe, very few maxims in philosophy that have laid firmer hold upon the mind than that air, meaning atmospheric air (free from various foreign matters, which were always supposed to be dissolved in and intermixed with it), is a *simple elementary* substance, indestructible and unalterable, at least as much so as water is supposed

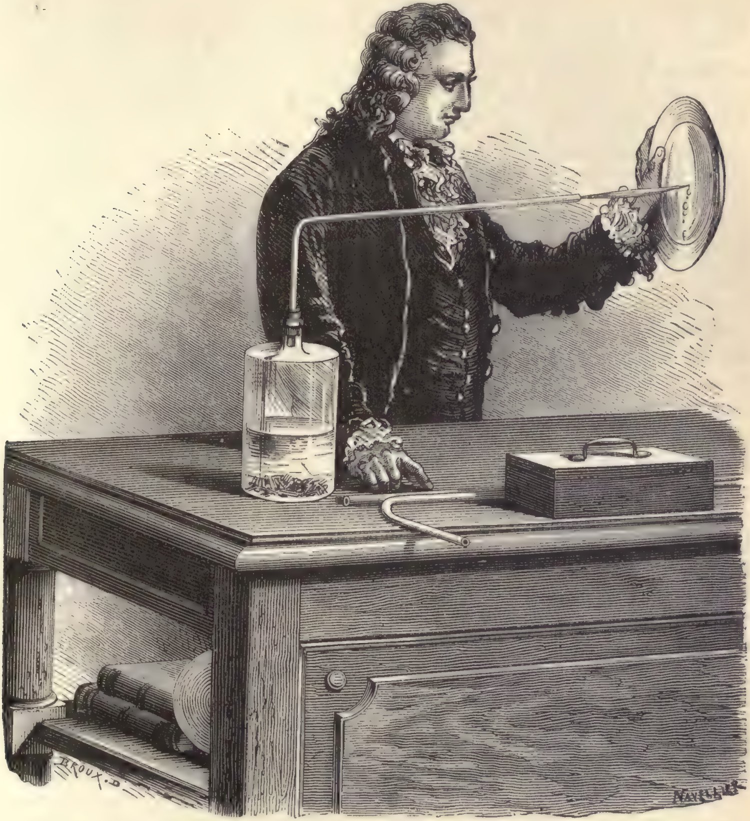


FIG. 173.—MACQUER'S EXPERIMENT, WITH PORTRAIT (*see page* 348).

to be. In the course of my inquiries I was, however, soon satisfied that atmospheric air is not an unalterable thing; for that, according to my first hypothesis, the phlogiston with which it becomes loaded from bodies burning in it, and animals breathing it, and various other chemical processes, so far alters and depraves it as to render it altogether unfit for inflammation, respiration, and the purposes to which it is subservient; and I had discovered that agitation, the processes of vegetation, and probably other natural processes, restore it to its original purity. But I own I had no idea of the possibility of going any further in this way, and thereby procuring air purer than the best common air. I might, indeed, have naturally imagined that such would be air that contained less phlogiston than the air of the atmosphere; but I had no idea that such a composition was possible.

"Having procured a lens of 12 inches diameter and 20 inches focal distance, I proceeded with great alacrity to examine by the help of it what kind of air a great variety of substances, natural and factitious, would yield, putting them into small phials made with round bottoms and very thin, filled with mercury and inverted in a basin of the same. With this apparatus, on the 1st of August, 1774, I endeavoured to extract air from *mercurius calcinatus per se* (*red oxide of mercury*); and I presently found that by means of this lens air was expelled from it very readily. Having got about three or four times as much as the bulk of my materials, I admitted water to it and found that it was not imbibed by it. But what surprised me more than I can well express, was that a candle burned in this air with a remarkably vigorous flame, very much like that enlarged flame with which a candle burns in nitrous air, exposed to iron or liver of sulphur; but as I got nothing like this remarkable appearance from any kind of air besides this particular modification of nitrous air, and I know no nitric acid was used in the preparation of *mercurius calcinatus*, I was utterly at a loss to account for it. In this case, also, though I did not give sufficient attention to this circumstance at the time, the flame of the candle, besides being larger, burned with more splendour and heat than it did in that species of nitrous air; and a piece of red-hot wood sparkled in it exactly like paper dipped in a solution of nitre, and it consumed very fast: an experiment which I had never thought of trying with dephlogisticated nitrous air (*nitrous oxide gas*).

"At the same time that I made the above-mentioned experiment, I extracted a quantity of air, with the very same property, from the common *red precipitate* (red oxide of mercury), which, being produced by solution of mercury in spirit of nitre (*nitric acid*), made me conclude that this peculiar property, being similar to that of the modification (*nitrous oxide*) of nitrous air (*nitrous acid*) above mentioned, depended upon something being communicated to it by the nitrous acid; and since the *mercurius calcinatus* is produced by exposing mercury to a certain degree of heat, where common air has access to it, I likewise concluded that this substance had collected something of *nitre*, in that state of heat, from the atmosphere.

"This, however, appearing to me much more extraordinary than it ought to have done, I entertained some suspicion that the *mercurius calcinatus* on which I had made my experiments, being bought at a common apothecary's, might in fact be more than red precipitate, though, had I been anything of a practical chemist, I could not have entertained any such suspicion. However, mentioning this suspicion to Mr. Waltire, he furnished me with some which he had kept for a specimen of the preparation, and which he told me he could warrant to be genuine. This being treated in the same manner as the former, only by a longer continuance of heat, I extracted much more air from it than from the other.

"This experiment might have satisfied any moderate sceptic; but, however, being at Paris in the October following, and knowing that there were several very eminent chemists in that place, I did not omit the opportunity, by means of my friend Mr. Magellan, to get an ounce of *mercurius calcinatus* prepared by M. Callet, of the genuineness of which there could not possibly be any suspicion, and at the same time I frequently mentioned my surprise at the kind of air which I had got from this preparation to M. Lavoisier, M. le Roy, and several other philosophers who honoured me with their notice in that city. At the same time I had no suspicion that the air which I had got from the *mercurius calcinatus* was even wholesome, so far was I from knowing what it really was that I had found, taking it for granted that it was nothing more than such kind of air as I had brought nitrous air (*nitrous acid*) to be by the processes above mentioned (*i.e.*, *exposure to deoxidizing agents*); and in this air I have observed that a candle would burn sometimes quite brightly and sometimes with a beautiful enlarged flame, and yet [the gas would] remain perfectly noxious.

"At the same time that I had got the air above mentioned from *mercurius calcinatus* and the red precipitate, I had got the same kind from *red lead* or *minium*. In this process, that part of the minium on which the focus of the lens had fallen turned yellow. One-third of the air in this experiment was readily absorbed by water, but in the remainder a candle burned very strongly and with a crackling noise. This experiment with red lead confirmed me more in my suspicion that the *mercurius calcinatus* must have got the property of yielding this kind of air from the atmosphere, the process by which that preparation and this of red lead is made being similar. As I never make the least secret of anything that I observe, I mentioned this also, as well as those with the *mercurius calcinatus* and the red precipitate, to all my philosophical acquaintance in Paris and elsewhere, having no idea at that time to what these remarkable facts would lead.

"Presently, after my return from abroad, I went to work upon the *mercurius calcinatus* which I had procured from M. Callet, and with a very moderate degree of heat I got from some of it an ounce measure of air, which I observed to be not readily imbibed, either by the substance itself from which it had been expelled (for I suffered them to continue for a long time together before I transferred the air to another place), or by water over which I suffered this air to stand a considerable time before I made my experiment upon it.

"In this air, as I had expected, a candle burned with a vivid flame; but what I observed new at this time (19th of November, 1774), and which surprised me no less than the fact I had discovered before, was that, whereas a few moments' agitation in water will deprive the modified nitrous air (*nitrous oxide*) of its property of admitting a candle to burn in it, yet, after more than ten times as much agitation as would be sufficient to produce this alteration in the nitrous air, no sensible change was produced in this. A candle still burned in it with a strong flame, and it did not in the least diminish common air, which I had observed that nitrous air in this state (*i.e.*, *nitrous oxide*) in some measure does.

"But I was much more surprised when, after two days, in which this air had continued in contact with water (by which it was diminished about one-twentieth of its bulk), I agitated it violently in water about five minutes, and found that a candle still burned in it as well as it did in common air. The same degree of agitation would have made phlogisticated nitrous air fit for respiration, indeed, but it would have extinguished a candle.

"These facts fully convinced me that there must be a very material difference between the constitution of the air from *mercurius calcinatus* and that of dephlogisticated nitrous air (*nitrous oxide*), notwithstanding their resemblance in some particulars. But though I did not doubt that the air from *mercurius calcinatus* was fit for respiration after being agitated with water, as every kind of air had been, without exception, on which I had tried the experiment, I still did not suspect that it was respirable in the first instance; so far was I from having any idea of this air being, which it really was, much superior in this respect to the air of the atmosphere.

"In this ignorance of the real nature of this kind of air I continued from this time (Nov. 1774) to the March following (1775), having in the meantime been intent upon my experiments on the vitriolic acid air, and the various modifications of air produced by spirit of nitre (*nitric acid*). But in the course of this month I not only ascertained the nature of this kind of air, though very gradually, but was led by it to the complete discovery, as I then thought, of the constitution of the air we breathe.

"Till the first of March, 1775, I had so little suspicion of the air from *mercurius calcinatus*, etc., being wholesome, that I had not even thought of applying to it the test of nitrous air (*nitric oxide*); but thinking, as my reader must imagine I frequently must have done, on the candle burning in it after long agitation in water" (*he had found that nitrous oxide is readily absorbed by water*), "it occurred to me at last to make the experiment, and putting one measure of nitrous air (*nitric oxide*) to two measures of this air (*i.e.*, *the oxygen*), I found not only that it was diminished, but that it diminished quite as much as common air, and that the redness of the mixture was likewise equal to that of a similar mixture of nitrous and common air.

"After this I had no doubt but that the air from *mercurius calcinatus* was fit for respiration, and that it had all the properties of genuine common air. But I did not take notice of what I might have observed, if I had not been so fully possessed by the notion of there being no air better than common air, that the redness was really deeper, and the diminution something greater than common air would have admitted.

"I now concluded that all the constituent parts of the air were equally, and in their proper proportion, imbibed in the preparation of this substance (*mercurius calcinatus*), and also in the process of making red lead. For at the same time that I made the above-mentioned experiment on the air from *mercurius calcinatus*, I likewise observed that the air which I had extracted from red lead, after the fixed air (*carbonic acid*) was washed out of it, was of the same nature, being diminished by nitrous air like common air; but at the same time I was puzzled to find that air from the red precipitate was diminished in the same manner, though the process for making this substance is quite different from that of making the two others. But to this circumstance I happened not to give much attention.

"I wish my reader be not quite tired with the frequent repetition of the word *surprise*, and others of similar import, but I must go on in that style a little longer. For the next day I was more surprised than ever I had been before, by finding that after the above-mentioned mixture of nitrous air and the air from *mercurius calcinatus* had stood all night (in which time the whole diminution must have taken place, and consequently, had it been common air, it must have been made perfectly noxious, and entirely unfit for respiration in inflammation) a candle burned in it even better than in common air." (*A surplus of oxygen remained.*)

"I cannot at this distance of time recollect what it was that I had in view in making this experiment; but I know I had no expectation of the real issue of it. Having acquired a considerable degree of readiness in making experiments of this kind, a very slight and evanescent motive would be sufficient to induce me to do it. If, however, I had not happened for some other purpose to have had a lighted candle before me, I should probably never have made the trial, and the whole train of my future experiments relating to this kind of air might have been prevented.

"Still, however, having no conception of the real cause of the phenomenon, I considered it as something very extraordinary, but as a property that was peculiar to air extracted from these substances, and *adventitious;* and I always spoke of the air to my acquaintance as being substantially the same thing with common air. I particularly remember my telling Dr. Price that I was myself perfectly satisfied of its being common air, as it appeared to be so by the test of nitrous air,—though, for the satisfaction of others, I wanted a mouse to make the proof quite complete.

"On the 8th of this month (March, 1775) I procured a mouse, and put it into a glass vessel containing two ounce measures of the air from *mercurius calcinatus*. Had it been common air, a full-grown mouse as this was would have lived in it about a quarter of an hour. In this air, however, my mouse lived a full half-hour; and though it was taken out seemingly dead, it appeared to have been only exceedingly chilled; for upon being held to the fire it presently revived, and appeared not to have received any harm from the experiment.

"By this I was confirmed in my conclusion, that the air extracted from *mercurius calcinatus*, etc., was *at least as good* as common air; but I did not certainly conclude that it was any better; because, though one mouse would live only a quarter of an hour in a given quantity of air, I knew it was not impossible but that another mouse might have lived in it half an hour: so little accuracy is there in this method of ascertaining the goodness of air; and, indeed, I have never had recourse to it for my own satisfaction since the discovery of that most ready, accurate, and elegant test that nitrous air (*nitric oxide*) furnishes. But in this case I had a view to publishing the most generally satisfactory account of my experiments that the nature of the thing would admit of.

"This experiment with the mouse, when I had reflected upon it some time, gave me so much suspicion that the air into which I had put it was better than common air, that I was induced the day after to apply the test of nitrous air to a small part of that very quantity of air which the mouse had breathed so long; so that, had it been common air, I was satisfied it must have been very nearly, if not altogether, as noxious as possible, so as not to be affected by nitrous air; when, to my surprise again, I found that though it had been breathed so long, it was still better than common air. For, after mixing it with nitrous air in the usual proportion of two to one, it was diminished in the proportion of four and a half to three and a half; that is, the nitrous air had made it two-ninths less than before, and this in a very short space of time; whereas I had never found that in the longest time any common air was reduced more than one-fifth of its bulk by any proportion of nitrous air, nor more than one-fourth by any phlogistic process whatever. Thinking of this extraordinary fact upon my pillow the next morning, I put another measure of nitrous air to the same mixture, and, to my utter astonishment, found that it was further diminished to almost one-half of its original quantity. I then put a third measure to it; but this did not diminish it any further, but left it one measure less than it was even after the mouse had been taken out of it.

"Being now fully satisfied that this air, even after the mouse had breathed it half an hour, was much better than common air, and having a quantity of it still left, sufficient for the experiment—viz., an ounce measure and a half—I put the mouse into it; when I observed that it seemed to feel no shock upon being put into it, evident signs of which would have been visible if the air had not been very wholesome, but that it remained perfectly at its ease another full half-hour, when I took it out quite lively and vigorous. Measuring the air the next day, I found it to be reduced from one and a half to two-thirds of an ounce measure. And after this, if I remember well (for in my *register* of the day I only find it noted that it was *considerably diminished* by nitrous air) it was nearly as good as common air. It was evident, indeed, from the mouse having been taken out quite vigorous, that the air could not have been rendered very noxious.

"For my further satisfaction I procured another mouse, and putting it into less than two ounce measures of air extracted from *mercurius calcinatus* and air from red precipitate (which, having found them to be of the same quality, I had mixed together), it lived three-quarters of an hour. But not having had the precaution to set the vessel in a warm place, I suspect that the mouse died of cold" (Priestley passed up the mice into the inverted vessel of gas through the water). "However, as it had lived three times as long as it could probably have lived in the same quantity of common air, and I did not expect much accuracy from this kind of test, I did not think it necessary to make any more experiments with mice.

"Being now fully satisfied of the superior goodness of this kind of air, I proceeded to measure that degree of purity with as much accuracy as I could by the test of nitrous air, as if I had been examining common air, and now I observed that the diminution was evidently greater than common air would have suffered by the same treatment. A second measure of nitrous air reduced it to two-thirds of its original quantity, and a third measure to one-half. Suspecting that the diminution could not proceed much further, I then added only half a measure of nitrous air. By this it was diminished still more, but not much, and another half-measure made it more than half of its original quantity; so that, in this case, two measures of this air took more than two measures of nitrous air, and yet remained less than half of what it was. Five measures brought it pretty exactly to its original dimensions.

"At the same time, air from the *red precipitate* was diminished in the same proportion as that from *mercurius calcinatus*, five measures of nitrous air being received by two measures of this without any increase of dimensions. Now, as common air takes about one-half of its bulk of nitrous air before it begins to receive any addition to its dimensions from more nitrous air, and this air took more than four half-measures before it ceased to be diminished by more nitrous air, and even five half-measures made no addition to its original dimensions, I concluded that it was between four and five times as good as common air. It will be seen that I have since procured air purer than this."

Antoine Laurent Lavoisier (1743—1794) was born at Paris, and his father, a wealthy merchant, was in a position to afford him the best education. He showed at an early age a decided taste for science, and when twenty-one years old he gained a prize offered by the Government for the best essay on the most effectual means of lighting the streets of Paris. When twenty-five years of age he was admitted to the Academy of Sciences, and he would probably have devoted his splendid abilities to mathematics had the brilliant discoveries of Black (page 344) not determined his choice to chemistry. He published a series of essays in 1772 giving an historical review of the progress of the science, and in the last one defending the teaching of Black regarding fixed air. He soon perceived that Stahl's phlogistic theory gave an insufficient and inconsistent explanation of chemical phenomena, and he devoted great attention to the elaboration of some more satisfactory doctrine. He began by carefully repeating the experiments of Black, Priestley, Scheele, and others, and contrived various ingenious arrangements by which he could make quantitative estimates of the substances he used, and the gaseous and other products he obtained. Except the volume of essays mentioned above, all Lavoisier's scientific papers were published in the *Mémoires* of the Academy of Sciences. In 1789 he published a work in which his own investigations and conclusions were embodied in a systematic manner, and these may be said to have included in some form the latest discoveries of that time. Lavoisier's work effected a revolution in chemical science relatively greater than that great convulsion through which France was then passing effected in the political world. Sad to say, the brilliant chemist was himself a victim of that terrible period. When the sanguinary Robespierre was in power, Lavoisier was accused of having defrauded the revenue, of which he had been a receiver. He was thrown into prison, and on the 8th of May, 1794, was led to the guillotine, in the fifty-first year of his age.

The closeness of the reasoning in which Lavoisier discusses his experiments; the accuracy, neatness, and elegance, so to speak, of the experiments themselves, have never been excelled. He, if any one, is the founder of modern chemistry, and the science of to-day bears the impress of his hand. The overthrow of the phlogistic theory which he effected; his demonstration of the part played by oxygen; his investigations on the composition of water and of the atmosphere; and his views on the composition of oxides, acids, and salts, are the chief subjects which we shall discuss in the present chapter.

He took a retort with a long neck, and having bent the neck as in Fig. 174, filled the vessel A with quicksilver, and placed it on a furnace, B, as shown in the figure, so that the end of the neck E passed into the inside of a bell jar, which stood inverted in a vessel of mercury, M. The quantity of air within the bell jar was so adjusted that the quicksilver stood at the same level within and without the jar, and this height was carefully marked by pasting a slip of paper on the jar. A fire was then lit in the furnace B, and kept up continually for twelve days, so as to keep the quicksilver in the retort A always very near its boiling-point. Nothing remarkable took place during the first day: the quicksilver, though not boiling, was continually evaporating, and collected in the upper part of the retort in small drops, which from time to time passed down again to the bulk of the quicksilver at the bottom of the vessel. On the second day small red particles began to appear on the surface of the liquid, and during the next four or five days the quantity of these gradually increased; but after that time no further change was observed, although the experiment was prolonged for twelve days. The fire was then extinguished, and the apparatus allowed to cool. The capacity of the retort, including its neck, and that of the bell jar, having been determined before the beginning of the experiment, Lavoisier knew what volume of air filled the parts of the vessel not occupied by the quicksilver at the beginning and at the end of the experiment. He found that at the end *one-sixth* of the air had disappeared, and that the residual air was no longer fit either for respiration or for combustion. Animals put in it were suffocated in a few seconds, and a taper plunged into it was extinguished as quickly and completely as if it had been put into water.

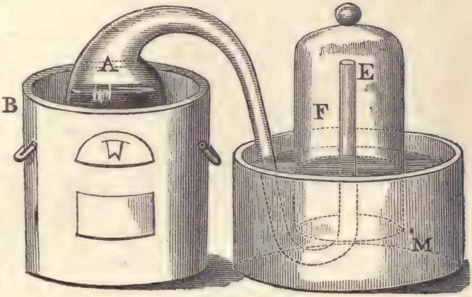


FIG. 174.

The red matter which had formed on the mercury in the experiment just described was collected by Lavoisier, and put into a small glass retort, which was connected with a proper apparatus for receiving such liquid or gaseous products as might be given off when a strong heat was applied. The result of heating the red substance was that it gradually decreased in bulk, and soon altogether disappeared; and when 45 grains of it were so heated, the receiver was found to contain 41½ grains of liquid mercury, and into a bell jar 7 or 8 cubic inches of a gas had passed. This gas was found capable of supporting respiration and combustion more vigorously than common air. Lavoisier remarks that this species of air was discovered at nearly the same time by Priestley, Scheele, and himself; and, indeed, there is little doubt that at about the same period these three chemists did, independently of each other, discover oxygen. Priestley, as we have seen, called it "*dephlogisticated air;*" Scheele called it *empyreal air;* and Lavoisier at first named it *air eminently fitted for respiration*, but afterwards substituted the shorter term *vital air*, and finally gave it, for reasons which will have to be discussed presently, the name it still bears, viz., *oxygen.*

The conclusions which Lavoisier drew from his experiments were very different from the *phlogistic* explanations of Priestley. The mercury during its calcination, he said, absorbs the salubrious and respirable part of the air included in the retort C and bell jar F, and the remaining air is a mephitic kind, supporting neither combustion nor respiration. Atmospheric air consequently is composed of two elastic fluids of different and opposite qualities. As a confirmation of this last inference Lavoisier, in another experiment, added to the residual "*mephitic air*" the *vital air* which the red substance had given off, and the air thus produced had qualities in every respect agreeing with those of common atmospheric air. He soon changed the terms *vital air*, mephitic air, etc., for others; and the reasons for choosing the new name for the *respirable air* are based on Lavoisier's own discoveries of certain chemical properties of this "*air*." It will be necessary before proceeding to discuss these to mention his views on heat and the nature of gases. He considers that the phenomena of heat are best explained by attributing them to the effect of a real and material but very subtle fluid, which, insinuating itself between the particles of bodies, and by separating these particles from each other, produces the expansion and the changes from solid and liquid to the vaporous state, etc. In order to prevent any confusion arising from one word being used to describe the sensation we term heat and the cause of that heat, Lavoisier gave the latter the name of *igneous fluid* and *matter of heat;* but soon afterwards, in arranging with some of the most eminent French chemists a new chemical nomenclature, it was agreed that the "exquisitely elastic fluid" that is the cause of heat should be called *caloric.* But Lavoisier considers that this word need not be appropriated to any one theory. Now, it was part of Lavoisier's theory to regard every gas as a compound of *caloric* with some substance which constituted what he termed the *base* of the gas. Thus he considered the respirable part of the atmosphere was a compound of *oxygen and caloric.* Lavoisier thus expresses himself on this subject: "We know in general that all bodies in nature are imbued, surrounded, and penetrated in every way with caloric, which fills up every interval left between their particles; that, in certain cases, caloric becomes fixed in bodies, so as to constitute a part of even their solid substance, though it more frequently acts upon them with a repulsive force, from which, or from its accumulation in bodies to a greater or lesser degree, the transformation of solids to liquids, and of liquids to the aëriform state of elasticity, is entirely owing. We have employed the generic name *gas* to indicate the aëriform state of bodies produced by a sufficient accumulation of caloric; so that when we wish to express the aëriform state of muriatic acid, etc., we do it by adding the word *gas* to their names; thus, *muriatic acid gas*, *aqueous gas*, etc." It was on account of its power of forming acid compounds that Lavoisier gave to "vital air" the name of *oxygen*, by which it is now so well known. The name is derived from two Greek roots, meaning *acid* and *to produce.*

Lavoisier burnt a weighed quantity of phosphorus in a known quantity of oxygen gas. The combustion was very rapid, and accompanied by an extremely brilliant light and much heat; and as it went on, large quantities of white flakes were formed in the vessel. This white matter was collected and weighed, and its weight was found to be exactly the sum of the weights of the phosphorus and of the oxygen which had disappeared. The white substance is very soluble in water and intensely sour, properties which are absent from the phosphorus. Sulphur burnt in oxygen also produces an acid, and charcoal unites with oxygen, forming "fixed air," to which Lavoisier first gave the name of *carbonic acid gas.* The gaseous acid produced by the combustion of sulphur had long been known under the name of sulphurous acid, and Lavoisier found that sulphur combined with a greater proportion of oxygen than existed in the gaseous acid, constituted the vitriolic acid. But Lavoisier and certain other French chemists remodelled entirely the system of naming chemical compounds, by adopting the intelligible principle that the name should express the composition of the substance. In such cases as that of the two acids of sulphur, it was agreed that the compound containing the less proportion of oxygen should be termed sulphur*ous* acid; while the other, having the higher proportion of oxygen, should be distinguished as sulphur*ic* acid. Before the great reform of chemical nomenclature introduced by Lavoisier and Morveau, substances often received names according to the source from which they were obtained, or sometimes according to the dictates of caprice and suggestions of fanciful analogies. The acid obtained from green vitriol (sulphate of iron) was called *vitriolic acid;* that produced by burning sulphur was called *sulphurous acid;* but it was not until the discoveries of Lavoisier that the connection of these substances was recognized and expressed by the names he gave them.

There was one well-known acid, however, which Lavoisier was unable to bring under his rules of naming, for its true composition was unknown at the time. When common salt is heated with sulphuric acid, a strong volatile acid distils over, to which it had been usual to give the name of "*spirit of salt.*" Lavoisier acknowledges that he is unable to discover the base of this acid, but he is firmly convinced that it consists of some substance united to oxygen, and, although unknown, he calls this substance the *muriatic base*, or *muriatic radical*, from the Latin word *muria* which was anciently used to signify sea-salt, while the acid itself he termed *muriatic acid.* This name became established, and remained in use even after the real nature of the acid was ascertained to be very different from that which Lavoisier attributed to it. It will be remembered that Scheele had discovered the gas we now call *chlorine* by heating muriatic acid and black oxide of manganese. The nature of the action which took place was long misunderstood. It was supposed that the muriatic acid derived oxygen from the oxide of manganese, and hence chlorine was called "oxygenated muriatic acid." This supposition appeared to be completely confirmed by some of the known properties of chlorine. For example, it was known that when water saturated with chlorine was exposed to the sunshine, oxygen gas was given off and muriatic acid reproduced.

The principle of giving chemical substances such names as will express their composition was first definitely propounded in 1782 by a French barrister named GUYTON DE MORVEAU, who devoted his leisure to the study of chemistry. Lavoisier at once recognized the value of this principle, and Morveau on his side became a convert to the new anti-phlogistic doctrines of Lavoisier. As might have been expected, the old and accepted phlogistic doctrine was not without defenders, but the obstinacy with which some of these resisted conviction recalls the pertinacity of the opponents of the Copernican planetary system. Macquer declared that rather than adopt the new ideas he would renounce the science altogether, and other chemists, even in France, offered a strenuous opposition; but at a meeting of the Academy of Sciences in 1785, Berthollet, one of the most eminent of the French chemists, declared himself a convert to Lavoisier. Fourcroy, who was the Professor of Chemistry at the Jardin des Plantes, and others, soon afterwards did the same. Lavoisier, Morveau, Berthollet, and Fourcroy combined together in the composition of a treatise on the nomenclature of chemistry, which was published in 1787. Lavoisier and his coadjutors also established a monthly journal, the "*Annales de Chêmie*," in which only the new nomenclature was employed. The eminence of the editors and the great value of the papers which appeared in this journal secured for it the attention of chemists everywhere, so that the new nomenclature was soon made known in every civilized country. The English chemists—no doubt in some degree affected by the general British determination to oppose all French innovations—almost to a man clung to their beloved phlogiston. Cavendish published an able defence of the old theory, but finding that the new opinions were nevertheless gaining ground, he relinquished chemical studies altogether. Priestley died in the phlogistic faith, and other British chemists imitated Cavendish by throwing up the study in disgust. One of the stoutest defenders of phlogiston was an English chemist named KIRWAN (1750—1812), a man of deservedly high reputation. He published a very elaborate defence of the phlogistic doctrines, in which he sought to identify the phlogiston of Stahl with inflammable air (*i.e.*, hydrogen gas). According to Kirwan's essay, "inflammable air" is a constituent of charcoal, sulphur, and all metals, and when these are burnt it escapes from them and enters into new combinations. This modification of the phlogistic theory was at first favourably received by many British and continental chemists; but Lavoisier and his confederates translated the "Essay on Phlogiston" into French, and published it with a complete refutation

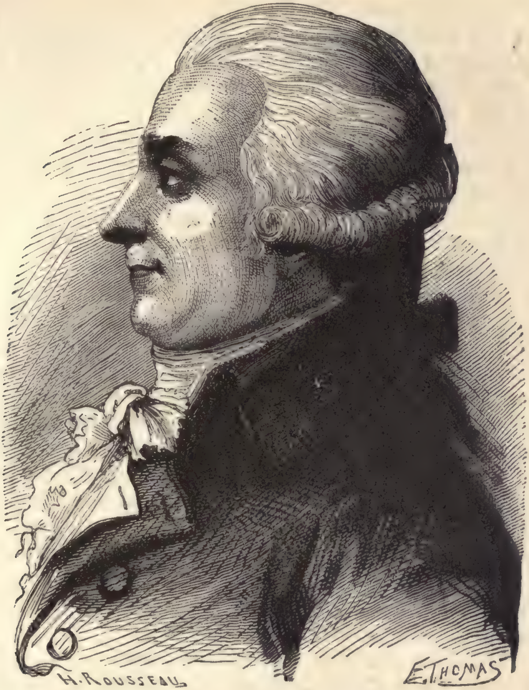


FIG. 175.—GUYTON DE MORVEAU.

of it section by section, each section having its refutation appended at the end. So clear and convincing were the arguments adduced by the French chemists, that Kirwan acknowledged that these arguments could not be withstood, and he now candidly declared himself a convert to the very doctrines his essay was intended to oppose. This defection to the enemy's camp drove the British phlogistonists from the field; and, in fact, chemistry had for a time but few active cultivators in this country, while France became the scene of new discoveries and the source of all knowledge of the science for some years, so that even the manuals of elementary instruction were all translations of French works.

In illustration of the reluctance with which Lavoisier's new ideas were adopted by men of science in Britain, might be quoted the objections raised by Dr. T. Thomson:—Though, said he, the new nomenclature was well adapted to the state of the science in 1787, new facts will be discovered from time to time which will make a systematic nomenclature more and more imperfect, and in a few years cause it to be discarded altogether. "Indeed, the establishment of a new nomenclature in any science ought to be considered as high treason against our ancestors; as it is nothing less than an attempt to render their writings unintelligible, to annihilate their discoveries, and to claim the whole as our own property." So far Dr. T. Thomson. Now thus Lavoisier expresses himself on the same topic: "The impossibility of separating the nomenclature of a science from the science itself is owing to this, that every branch of physical science must consist of three things—the series of facts which are the objects of the science, the ideas which represent these facts, and the words by which these ideas are expressed. Like three impressions of the same seal, the word ought to produce the idea, and the idea ought to be a picture of the fact. And as the ideas are preserved and communicated by means of words, it necessarily follows that we cannot improve the language of science without at the same time improving the science itself; neither can we, on the other hand, improve a science without improving the language or nomenclature which belongs to it. However certain the facts of any science may be, and however just the ideas we may have formed of these facts, we can communicate only false or imperfect impressions of these ideas to others while we want words by which they may be properly expressed."

The system of names adopted by Lavoisier has the merit of a simplicity which is perhaps unsuspected by persons unacquainted with chemistry who casually meet with the names of chemical compounds. It required no small exercise of the memory to recall the old names, uncommon and barbarous as they are, and with no indications of the *class* of substances to which they belong, and with little or no suggestion of the relationships of the substances to each other. Such were *powder of algaroth*, *salt of alembroth*, *pompholix*, *turbith mineral*, *colcothar*, etc., etc. And where the names indicated something of the constituents or sources of the substances, they often at the same time suggested false ideas as to its nature, thus: *oil* of vitriol; *butter* of antimony; *sugar* of lead; *liver* of sulphur; *flowers* of zinc, etc., etc. Without entering into unnecessary details, it will be easy by a few examples to show the meaning of the French system of nomenclature. The substances which result from the combination of a metal or other body with oxygen were called *oxides*. *Oxide* was therefore the name of a class of substances all agreeing in containing oxygen united with another substance, and agreeing also by the characteristic of being *not* acid, but capable of uniting with acids. The kind of substance with which the oxygen is united is indicated by another word, thus *lead oxide* or *oxide of lead* or *plumbic* (Lat. *plumbum*, lead) *oxide*, is the name of a compound of lead with oxygen. And as not unfrequently the same body unites with oxygen in several distinct proportions, the particular oxide is indicated by a prefix to the generic, or by a suffix to the specific name; thus protoxide of iron or ferr*ous* oxide means the lowest degree of oxygenation of iron, while peroxide of iron or ferr*ic* oxide implies the highest degree. Similarly, suboxide of copper or cuprous oxide has the less proportion of oxygen, while copper oxide or cupric oxide has the greater. In some instances some characteristic physical properties of the compound is used to mark it, thus: *magnetic* oxide of iron, black oxide of manganese, red oxide of lead, etc. But there is another class of compounds also, containing only oxygen and another substance, which Lavoisier called *acids*. The solutions of these substances are all sour, and redden blue vegetable colours. Acids are distinguished also by their power of combining with oxides, and in this way forming *salts*. The names of acids are given on the same plan as those of oxides, and the like terminations are used to express the degree of oxidation; *e.g.*, sulphur*ous acid*, sulphur*ic acid*. Occasionally Greek prefixes are employed when still higher or lower degrees of oxygenation have to be expressed, as *hypo*sulphurous acid, etc. The names of acids of oxides being thus arranged for, it remains to be seen how *salts* are named. The name of each salt is duplex, one word indicating the oxide, and the other the acid from which it is formed. The name of the acid determines the class, that of the oxide the kind, of the body. Many salts are formed from acids and substances which Lavoisier only *supposed* by analogy to be oxides, *e.g.*, magnesia, lime, etc., and in such case the magnesia, etc., was called the *base*. Examples of names of salts: sulph*ate* of lead = sulphuric acid + oxide of lead; nitrate of potash = nitric acid + potash; sulph*ite* of lead = sulphur*ous* acid + oxide of lead; nitr*ite* of potash = nitrous acid + potash, etc.

This nomenclature holds its ground to the present day with only some slight modifications. It had the merit of simplicity, apparent agreement with the facts, and it even suggested new views which the progress of discovery confirmed. The view it presents of the constitution of salts became a central idea, which still predominates in the conceptions of chemists; at least, with regard to a very numerous class of compounds. That view considers all salts as the result of the combination of two distinct and indeed contrasted principles, namely, an *acid* and a *base*. Thus, for example, *lime* is a base which absorbs and combines with the gaseous acid called *carbonic acid*, the resulting salt being the substance called in common language chalk, but which is chemically named *carbonate of lime*. The *base* and *acid* are supposed to exist in this combination, and are said to *neutralize* each other because the compound has no longer either *acid* or alkaline properties. Now, in the time of Lavoisier lime had as yet not been decomposed; but he, remarking the absence of any tendency of that substance and of other alkaline earths to unite with oxygen, sagaciously divined that this proceeded from these substances being metallic elements already saturated with oxygen. This surmise was soon afterwards completely verified, and lime was proved to be the oxide of a metal, to which the name of *calcium* was given. Carbonate of lime therefore contains the three elements, calcium, carbon, and oxygen; but according to Lavoisier's views, the oxygen enters into the compound on two different footings, one part attracted to the metal calcium, the other forming the acid by union with carbon. The constitution of carbonate of lime according to this view is *dualistic*, and may be represented thus :—

$$\left.\begin{matrix}\text{carbon}\\ \text{oxygen}\end{matrix}\right\}+\left\{\begin{matrix}\text{calcium,}\\ \text{oxygen,}\end{matrix}\right.$$

whereas the results of final analysis may be more simply indicated thus :—

$$\text{carbon}+\text{oxygen}+\text{calcium.}$$

Much discussion has subsequently been raised as to the merits of the *dualistic* and the *unitary* method of considering the composition of salts and other compounds which correspond respectively with the formulæ given above. These discussions have sometimes been raised on a false issue, namely, the actual and inner constitution of matter, whereas all that the chemist's knowledge can compass is a knowledge of reactions; and the Lavoisier conception of salts prevails to this day simply because it corresponds with the more common reactions in which salts are concerned.

Lavoisier considered oxygen as the sole acidifying principle, but it was soon remarked that oxygen is altogether absent from certain bodies which possess undeniable acid or base-saturating properties, such as *Prussic acid*, for example. Then there was the case of *muriatic acid* already mentioned (page 367); but though oxygen had not actually been obtained from this last body, it was held to be nevertheless an oxygenated compound until Davy showed its true character. One result of Davy's discoveries has been an important change in the views of chemists regarding acids: oxygen is no longer regarded as the essential acidifying principle. The name of *acid* is given only to substances which contain *hydrogen* and are capable of neutralizing bases.

A tabular view of the chemical elements and their binary combinations with oxygen, from Kerr's English translation of Lavoisier's "Elements of Chemistry" (Fourth Edition, Edinburgh, 1799), will serve to give the reader a general view of the scope of chemical science at the close of the eighteenth century.

### SIMPLE SUBSTANCES.

Light, caloric, oxygen, azote (nitrogen), hydrogen.

*Oxidizable and acidifiable non-metallic substances.*—Sulphur, phosphorus, carbon, muriatic radical, fluoric radical, boracic radical.

*Oxidizable and acidifiable metallic bodies.*—Antimony, arsenic, bismuth, cobalt, copper, gold, iron, lead, manganese, mercury, molybdenum, nickel, platina, silver, tin, tungsten, zinc.

*Salifiable earthy substances.*—Lime, magnesia, barytes, alumina, strontia.

### COMPOUND OXIDIZABLE AND ACIDIFIABLE RADICALS.

*Mineral.*—The radical of muriatic acid.

*Vegetable.*—The radicals of tartaric, malic, citric, pyrolignic, pyromucic, oxalic, acetic, succinic, benzoic, camphoric, and gallic acids.

*Animal.*—The radicals of lactic, saccho-lactic, formic, bombic, sebacic, lithic, and Prussic acids.

## TABLE OF THE BINARY COMBINATIONS OF OXYGEN WITH SIMPLE SUBSTANCES.

| | | First degree of oxygenation. | Second degree of oxygenation. | Third degree of oxygenation. | Fourth degree of oxygenation. |
|---|---|---|---|---|---|
| *Combination with non-metallic simple substances.* | Caloric | Oxygen gas | | | |
| | Hydrogen | Water | | | |
| | Azote | Nitrous oxide | Nitrous acid | Nitric acid | Oxygenated nitric acid. |
| | Carbon | Carbonic oxide | Carbonous acid | Carbonic acid | Oxygenated carbonic acid. |
| | Sulphur | Oxide of sulphur | Sulphurous acid | Sulphuric acid | Oxygenated sulphuric acid. |
| | Phosphorus | Oxide of phosphorus | Phosphorous acid | Phosphoric acid | Oxygenated phosphoric acid. |
| | Muriatic radical | Muriatic oxide | Muriatous acid | Muriatic acid | Oxygenated muriatic acid. |
| | Fluoric radical | Fluoric oxide | Fluorous acid | Fluoric acid | |
| | Boracic radical | Boracic oxide | Boracous acid | Boracic acid | |
| *Combination with simple substances.* | Antimony | Grey oxide of antimony | White oxide of antimony | Antimonic acid | |
| | Silver | Oxide of silver | . . . | Argentic acid | |
| | Arsenic | Grey oxide of arsenic | White oxide of arsenic | Arsenic acid | Oxygenated arsenic acid. |
| | Bismuth | Grey oxide of bismuth | White oxide of bismuth | Bismuthic acid | |
| | Cobalt | Grey oxide of cobalt | . . . | Cobaltic acid | |
| | Copper | Brown oxide of copper | Blue and green oxides of copper | Cupric acid | |
| | Tin | Grey oxide of tin | White oxide of tin | Stannic acid | |
| | Iron | Black oxide of iron | Yellow and red oxides of iron | Ferric acid | |
| | Manganese | Black oxide of manganese | White oxide of manganese | Manganic acid | |
| | Mercury | Black oxide of mercury | Yellow and red oxides of mercury | Mercuric acid | |
| | Molybdenum | Oxide of molybdenum | . . . | Molybdic acid | Oxygenated molybdic acid. |
| | Nickel | Oxide of nickel | . . . | Nickelic acid | |
| | Gold | Yellow oxide of gold | Red oxide of gold | Auric acid | |
| | Platinum | Yellow oxide of platinum | . . . | Platinic acid | |
| | Lead | Grey oxide of lead | Yellow and red oxides of lead | Plumbic acid | |
| | Tungsten | Oxide of tungsten | . . . | Tungstic acid | Oxygenated tungstic acid. |
| | Zinc | Grey oxide of zinc | White oxide of zinc | Zincic acid | |

However disputed have been the claims of various chemists to the honour of having first suggested and proved by *synthetical* experiments that water was composed of inflammable air and vital air, it is admitted that Lavoisier is entitled to the priority in having *analytically* demonstrated this great truth. First, let us see how he showed that metallic "*calxes*" were combinations of metals with oxygen; that is, according to his nomenclature, they were *oxides.* Oxygen, he says, has a stronger affinity for metals that are heated to a certain degree than for caloric (page 310). In consequence of this all metallic bodies, excepting gold, silver, and platinum, have the property of decomposing oxygen gas by attracting its base from the caloric with which it was combined. Lavoisier showed experimentally that iron wire burns with splendour to the last particle in oxygen gas. He found that the metal is converted into what the old chemists called *martial ethiops*, and that every 100 grains of iron burnt yielded 135 grains of this substance (which is, in fact, oxide of iron), and that the weight of the oxygen which had disappeared was equal to that which the iron had gained. In such experiments it is in general necessary to apply heat to start the combustion; for instance, the iron wire in the above experiment was set on fire by a piece of ignited tinder. Lavoisier had a theory about this, namely, that the heat is required to separate the particles of the metals from each other, so as to overcome in some degree at least their mutual attractions. Metals by uniting with oxygen lose their lustre, and become changed into dull pulverulent matters, which the older chemists (by false analogy with the effects of heating limestone) called *calxes.* Now, when Lavoisier had established the fact that iron directly unites with oxygen, producing an "oxide" possessed of certain properties, the following experiments, which he performed with the apparatus depicted in Fig. 176, were perfectly conclusive. G″G is a glass tube passing through the furnace F. The tube G″G, which was made of difficultly fusible glass, and was coated externally with a lute, to defend it from the too powerful action of the fire, had a slight inclination from G towards G″, and was connected at G with the retort I, containing a weighed quantity of pure distilled water. At G″ the glass tube was luted to the upper end of a worm-tube, and surrounded by cold water in the vessel H. The lower end of the worm-tube led into the double-necked bottle J, from which a tube passed under the bell jar K, standing in the pneumatic trough L. The experiments proceeded thus: first the fire was lighted in the furnace F, and when the tube G″G had become red hot, heat was applied to the retort I, so as to boil away all the water. The steam was condensed in the refrigerating vessel H, and the weight of water found in J was equal to that which had passed from the retort. So far, then, the operation was a simple distillation, and the result proved that the water had undergone no change by passing though the red-hot glass tube G″G. The apparatus was then rearranged for a second experiment exactly as before, except that 274

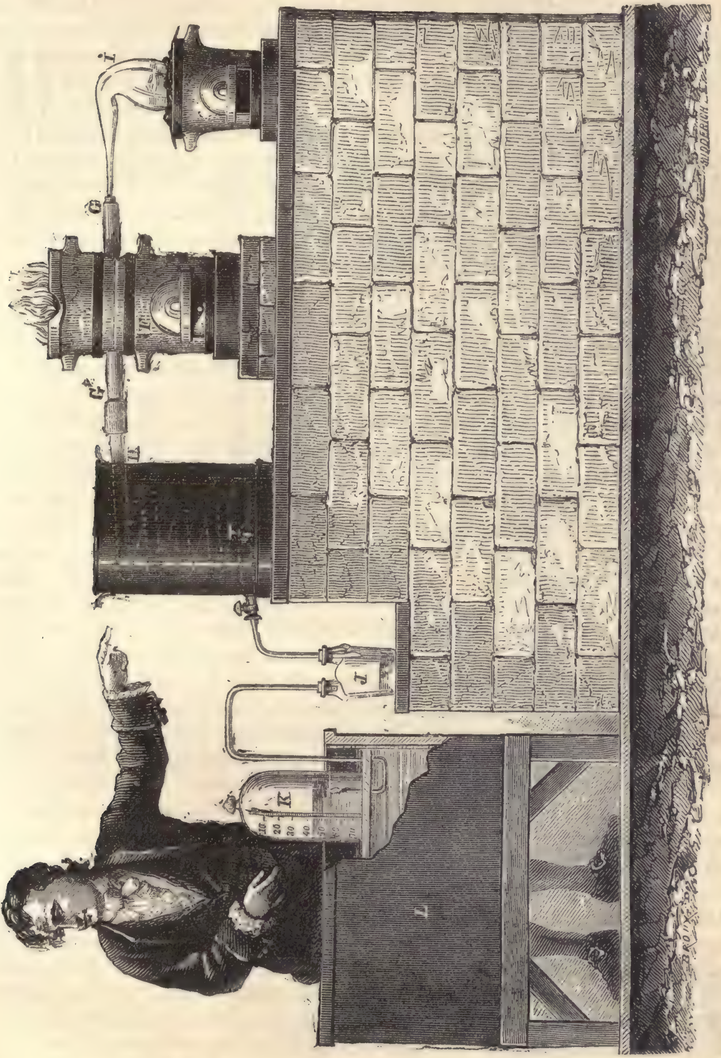


FIG. 176.—DEMONSTRATION OF THE COMPOSITION OF WATER.

grains of soft iron turnings were introduced into the tube G″G. This was heated to redness, and the steam then passed through. The results were that a quantity of gas, weighing 15 grains, was collected in the bell jar; the 274 grains of iron in the tube were found converted into

359 grains of *oxide* of iron, precisely like that produced by the combustion of iron in oxygen gas; and 100 grains of water had disappeared from the retort I, etc. Thus 100 grains of water had been decomposed into 85 grains of oxygen and 15 grains of an inflammable gas, for which principle as it exists in water Lavoisier proposed the name *hydrogen* (Greek, ὑδωρ, water; γεν, produce); while hydrogen *gas* is the name to be given to what he terms "its combination with caloric." It may be mentioned that the relative proportions of oxygen and hydrogen in water, as found by Lavoisier in this and other experiments, are erroneous by excess of hydrogen, the true proportions being eight parts of oxygen to one of hydrogen.

Lavoisier conducted the reverse or synthetical experiment so as clearly enough to prove the production of water from the combination of hydrogen and oxygen only; but the opposition and discussion which arose on this experiment were not finally set at rest until Lavoisier's disciples, Fourcroy, Seguin, and Vauquelin repeated the experiment in 1790 on a larger scale, by means of the apparatus shown in Fig. 117, which had been used by Lavoisier. The oxygen gas was contained in the gas-holder B; the hydrogen, in bulk double the oxygen, filled the vessel B′. A is a strong glass globe which was exhausted of the air at the commencement by means of the tube G. The hydrogen was burnt at a jet (not shown in the figure) near *a b*, which are metallic conductors arranged so that an electric spark might be passed between them in order to inflame the hydrogen in the first instance. C C are tubes for indicating the pressures of the gases, in order that they may be kept equal, while the flow of gas is so regulated that twice as much hydrogen as oxygen passes into the globe. In this way it became possible to maintain the combustion of hydrogen indefinitely; and, in fact, the experiment was carried on for six days continuously, day and night, from the 13th to the 22nd of May, 1790, each of the three experimenters taking his turn. More than half a pint of pure water was produced in the experiment, and when this was withdrawn from the glass, there was of course no difficulty in proving that the liquid was water and nothing but water. Some of the identical liquid is still preserved in the laboratory of the Museum of Natural History at Paris; and a large part of the apparatus employed by Lavoisier and others in experiments like these, memorable in the history of science, may also yet be seen at the Conservatoire des Arts et Métiers.

Lavoisier's great work in chemistry consisted in the overthrowing of the erroneous phlogistic theory, and in the giving to the world true ideas of combustion, of the constitution of the atmosphere, of the combination of elements to form acids and bases, and of the union of these to produce salts. In his memorable researches Lavoisier appeals to the balance, as when he argues that the products of combustion being heavier than the combustible body, the former cannot be the elements of the latter. He also distinguishes degrees of oxidation, and often

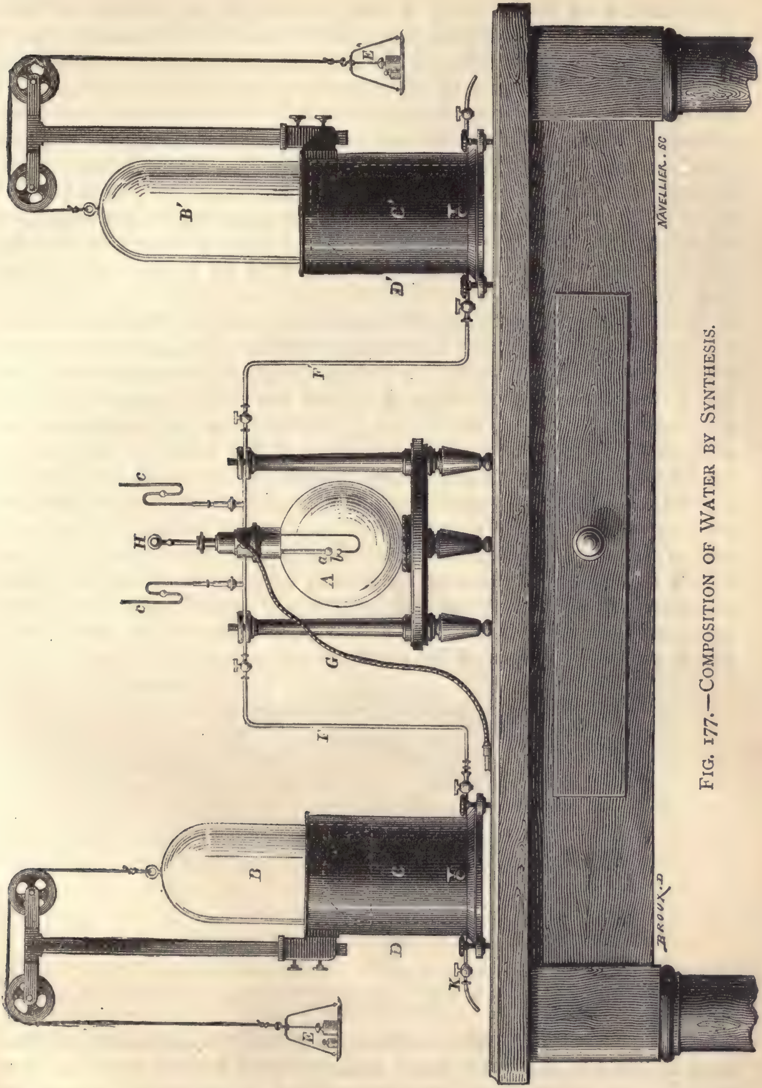
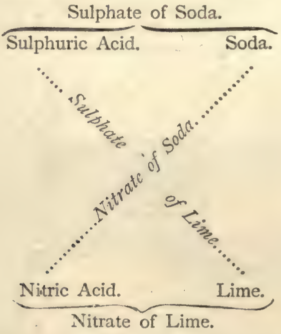

B'
C'
D'
E'
F'
H
c
c'
A
a
b
G
F
B
C
D
E
K
NAVELLIER. SC
BROUX. D

FIG. 177.—COMPOSITION OF WATER BY SYNTHESIS.

assigns the proportions in which certain elements unite, as, for instance, that 1 lb. of hydrogen in burning absorbs $5\frac{2}{3}$ lbs. of oxygen, the produce being $6\frac{2}{3}$ lbs. of water, etc. He thus tacitly recognizes a definite constitution of compounds in the ponderable proportions of their constituents. He nowhere, however, formulates the *law of definite proportions.* It is not improbable that the defects in the methods of quantitative analysis used by the chemists of Lavoisier's time prevented the constancy of the proportions in which substances combine from emerging from the analytical results as the absolute law. We can hardly wonder at doubts and denials of the idea of fixed proportions arising in the minds of chemists of that time, when we find one of the most skilful (Berthollet) determining by an experiment that 100 parts of sulphuric acid contained 69 of sulphur and 31 of oxygen, and by another experiment that the proportions were 72 of sulphur to 28 of oxygen.

About the time, however, that Lavoisier was laying the foundations of modern chemistry, two comparatively unknown German chemists experimentally established the fact of definite proportions, and, besides, introduced a new conception into the science, namely, that of *equivalents.* WENZEL, of Freiburg, published in 1777 a treatise on chemical affinity. He explained the fact of two neutral salts forming, by interchange of acids and bases, two new salts, also neutral—a circumstance which had struck the chemists of that time. This fact may be illustrated thus: If a solution of sulphate of soda is mixed with one of nitrate of lime, there is immediately produced sulphate of lime, which falls to the bottom of the vessel as a white powder, and also nitrate of soda, which remains in solution. The exchange of acids and bases which takes place in this action may be illustrated by this diagram, where the names of the products are printed in *italics.*



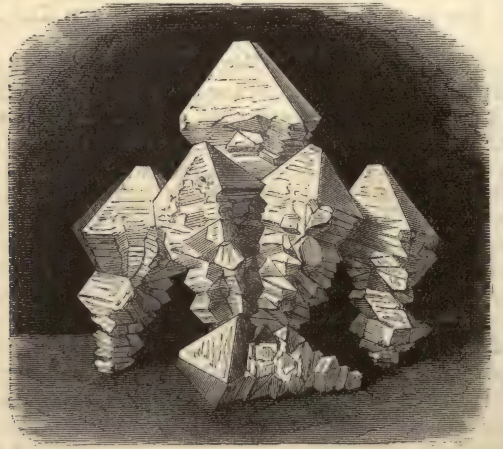
The noticeable thing was that whatever quantity of lime left the nitric acid to unite with the sulphuric acid, replaced just so much soda as was able to perform with the nitric acid the same function of forming a neutral salt. There was also the like reciprocal equivalence of the acids. It is easy to mix the solutions in such proportions that all the sulphuric acid and all the lime are both precipitated; and from the quantities of the salts present the inference may be made as to what proportions of the two acids are equivalent to each other, and what quantity of one base replaces a given quantity of the other.

The law of equivalent proportions as regards acids and bases was placed in a still clearer light some years afterwards by J. B. RICHTER, who in 1794 published a work in three volumes on the art of estimating chemical elements, and in other publications from 1796 to 1798 developed still further the doctrine of equivalency. Richter found the quantities of the several bases which were required to separately neutralize a fixed quantity of each acid. He took, for instance, 1,000 grains of sulphuric acid, and found the quantity of potash required to neutralize it. Then he found the quantity of soda required to neutralize another 1,000 grains of sulphuric acid, and so on with other bases. The like determinations were then made with some fixed quantity of nitric acid. Now, the proportions which subsisted between the quantities of the several bases required for the one acid were found to be identical with those required for the other acid. It was possible to express by numbers the quantities of each base which are equivalent to each other in their acid-saturating powers. Thus, 74 parts by weight of lime were found to be equivalent to 80 of soda, or to 112 of potash, and so on.

It will be readily understood that the law of equivalency in bases and acids necessarily implies the law of definite proportions in the composition of salts. The consequences deducible from the facts established by Wenzel and Richter had, however, to wait for the recognition of their importance; and their theoretical interpretation came from an illustrious Englishman, whose labours also made an important addition to the facts themselves. This was John Dalton of Manchester.

The labours of CLAUDE LOUIS BERTHOLLET (1748—1822), the eminent French chemist referred to on the preceding page, call for special notice, as they powerfully contributed to the progress of chemical science and its applications to the arts. Berthollet was a native of Annecy, in Savoy, and after having graduated in medicine at the University of Turin, he came to Paris, where he obtained a lucrative appointment as physician. His tastes, however, determined his attention to the study of chemistry, and he was one of the first to adopt the new views propounded by Lavoisier, taking part, as we have already seen, in the establishing of the modern nomenclature of the science. Berthollet's talents obtained for him the most distinguished professorial and official appointments in connection with science and art, while he was remarkable for his modesty of character and unostentatious manners. His investigations included an extension of the researches on ammonia which Priestley had begun, and an examination of the nature and properties of "oxy-muriatic acid," or *chlorine* as we now call it. The useful salt called *chlorate of potash* was one of his discoveries.

His name deserves always to be remembered in connection with two great chemical industries he may be said to have created; for he was not only the first to propose (1785) the application of chlorine to bleaching, by which that operation is now accomplished in as many hours as it formerly took weeks, but he was also the first to reduce to a system and explain by scientific laws the operations of the dyer. His contributions to the theory of chemistry were numerous and important, as his profound treatises on the "Laws of Chemical Affinity" and on "Chemical Statics" sufficiently testify. He points out that the tendency to chemical combination is possessed by bodies in different degrees, and that it is influenced and modified by the various physical forces, such as elasticity and cohesion. He proves that when solutions of two salts are mixed together, two new salts are formed by the partition of each base between the two acids, and by this exchange of acids and bases four salts co-exist in the solution. The decomposition of the original salts proceeds only until a certain condition of equilibrium among the contending forces is reached. But if one of the four salts be by any means removed from the liquid mixture, the state of equilibrium is destroyed, and fresh portions of the original salts are decomposed. The removal of one of the salts occurs if it is a substance insoluble in the liquid, in which case it is *precipitated*—that is to say, it falls to the bottom in a solid form; or, if it be volatilized by a sufficiently high temperature. Berthollet held, however, that definite proportion was not the essential law of chemical combinations, but merely an accidental result determined by the equilibrium of opposing *physical forces.*



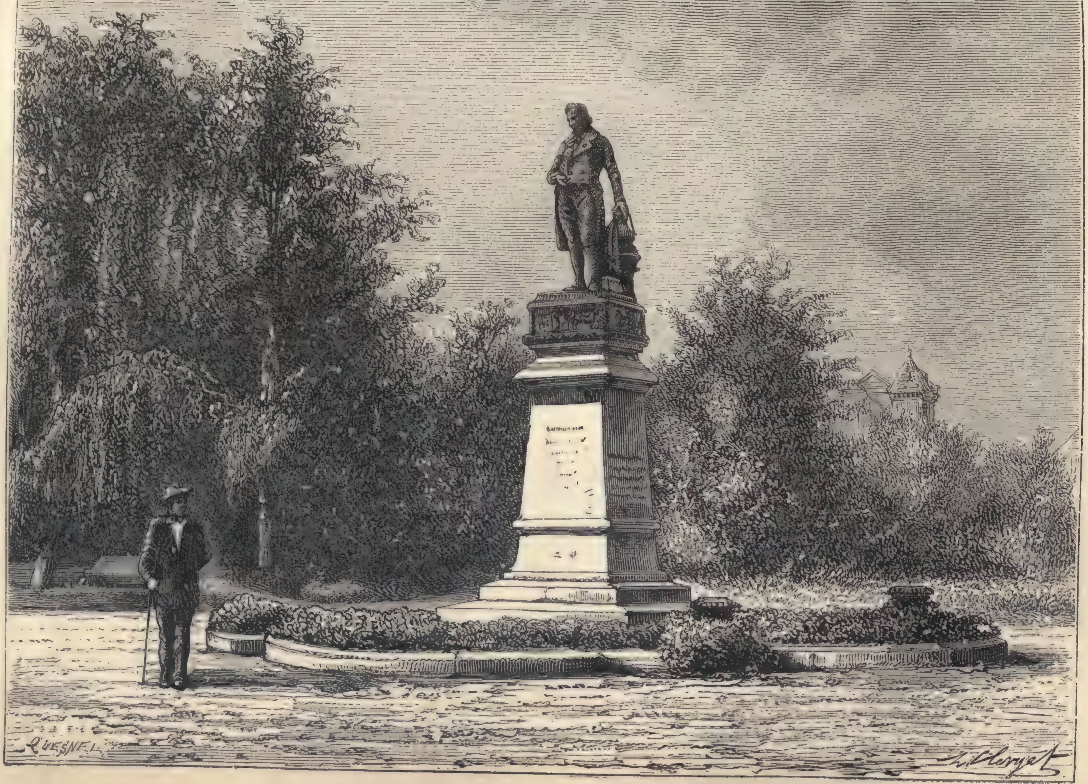


PLATE IX.—STATUE OF BERTHOLLET AT ANNECY.

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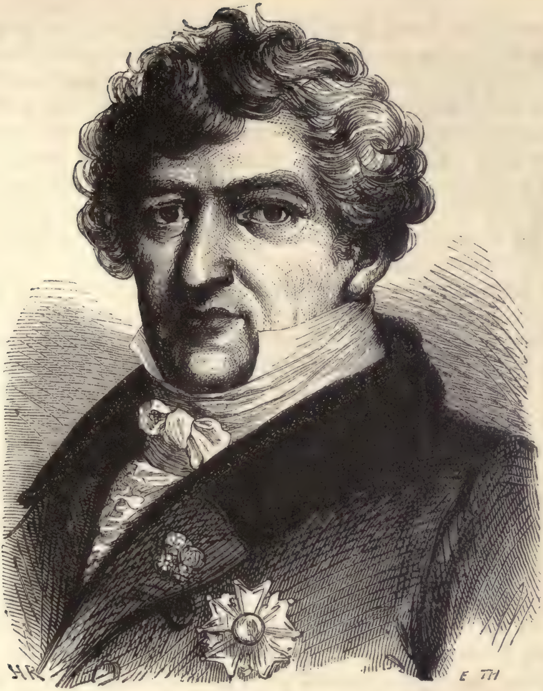

FIG. 178.—CUVIER.

## CHAPTER XV.

### NATURAL HISTORY OF THE EIGHTEENTH CENTURY.

EACH of the sciences which we here group together by the title of "Natural History Sciences" covers a vast field of knowledge, and comprehends extensive subdivisions, any one of which would require for its complete and successful cultivation a great part of any ordinary lifetime. The phenomena which are the objects of these sciences are more numerous, more varied, and especially more complex than those which engage the attention of the astronomer, the physicist, or the chemist. The philosophic naturalist has not only to take into account the operations of all the physical and chemical forces under infinitely varied conditions and in endless combinations; but in the study of vital phenomena he has besides to deal with new and strange considerations. It will therefore be obvious that in attempting to sketch within the compass of a single chapter the progress effected in the natural history sciences during the last century, we can for the most part touch only upon their general aspects. Such are the theories of the earth, as indicating the progress of discovery in Geology; and the schemes of classification in Botany and Zoology, as reflecting the knowledge of living things and of their relations. The limited space remaining will not permit of more than very brief notices of the careers of but a few of the great men whose labours raised the natural history sciences to their present lofty position.

KARL LINNÆUS was born on the 24th of May, 1707, at Råshalt, in in the province of Småland, in Sweden. His father was a village pastor, from whose example Linnæus probably derived his taste for the study of nature. The child who was destined to become so famous as a botanist, cultivated at a very early age a portion of the garden assigned to him by his father with full liberty to make whatever use of it he pleased, and the small plot was soon stocked not only with the flowers of the cultivated garden, but with the wild leafy beauties of the neighbouring woods and meadows. The pastor himself had some store of botanical knowledge, for we are told that when Linnæus was only four years of age, he heard his father explaining to a company of his friends the properties of the various plants in the garden. Young Karl then began continually to be asking his father the names and qualities of all the plants he met with. The pastor's home seems to have been favourably placed for fostering that love of nature which was a characteristic of Linnæus through life, for the house was near a beautiful lake, and was surrounded by hills and villages, woods and fields. When Linnæus passed from school to the gymnasium, or college, where he was to pursue studies preparatory to his adoption of the clerical profession, it was found after a time that he had no taste for Greek, Hebrew, ethics, metaphysics, rhetoric, or theology, and accordingly his preceptors reported to his father that the youth was unfit for the clerical or other learned profession, and recommended some handicraft trade as more suitable for him. But a professor of medicine had observed the genius of the boy for natural science, and he offered to receive him into his house as a student of medicine. Linnæus accordingly followed the prescribed course of studies at Lund, and afterwards at Upsala, at the same time eagerly pursuing botanical studies, in which he became so skilled as to attract the notice and acquire the friendship of the professor of botany and other persons distinguished for their scientific acquirements.

Linnæus had his attention attracted to the *stamens* and *pistils* of flowers by some remarks in a review of Vaillant's "*Discours sur la Structure des Fleurs*," and having closely examined these organs and discovered that they were essential to all flowering plants, he conceived the idea of making their arrangement the foundation of a new system of botanical classification; and in 1729 he actually drew up the first sketch of his scheme in an essay entitled, "Γαμος Φυτων, *Sive Nuptiæ Arborum.*" Linnæus was soon afterward (1730) appointed as deputy professor of botany at Upsala, after having lived there only two years. In 1731, Rudbeck, the senior professor, recommended that Linnæus should be sent by the Academy of Sciences to make a botanical tour in Lapland, in order that the natural history of that country should be thoroughly explored. Linnæus accordingly set out on his expedition in May, 1732, and spent the summer in examining the productions of Lapland. He left the public roads and took paths through the woods, and though a stranger to the language and manners of the people, he trusted himself entirely to their hospitality, and did not trust in vain. He speaks of the innocence, simplicity, and healthiness of the lives of the Lapps, and mentions with admiration the strength and vigour of two old men, his guides, who without fatigue carried his luggage in journeys by which he himself, young and robust as he was, was frequently exhausted. He had to meet many hardships and some narrow escapes in climbing precipices and crossing rivers, and he suffered not a little from heat and cold, hunger and thirst. On his return he presented to the Academy of Sciences a summary of his observations, but these were not published *in extenso* until some years afterwards.

Linnæus now strove to add to his limited means by lecturing on mineralogy, and endeavoured also to establish a medical practice. Having failed in this last project, he went to Harderwick in Holland, in order to extend his studies, and obtain the M.D. degree at the university there. He was soon afterwards introduced to the celebrated Boerhaave, by whose recommendation he became acquainted with a wealthy gentleman named Clifford, residing at Hartecamp near Haarlem. This gentleman had a magnificent garden and collection of plants, and prevailed on Linnæus to take up his residence with him. Here Linnæus was maintained and treated in every respect with the greatest consideration; but notwithstanding all the advantages which the opulence of his patron placed at his disposition, Linnæus formed the resolution of returning to his native country. During his stay at Hartecamp he set the botanical garden in order, and drew up a catalogue of the plants, which was published under the title "*Hortus Cliffortianus.*" He visited England under the auspices of Clifford, and made the acquaintance of the best of our botanists, and subsequently visited Paris, where he was cordially received by several distinguished men of science. On returning to Sweden he practised for a short time as a physician at Stockholm. In 1743 he was appointed Professor of Medicine and Natural History in the University of Upsala. He made the botanical garden here the finest and richest in Europe. He died at Upsala in 1778, in the seventy-first year of his age.

Of the writings of Linnæus, which are very voluminous, the most celebrated are the "*Systema Naturæ*," 1735, "*Fundamenta Botanica*," 1736, "*Genera Plantarum*," 1737, "*Species Plantarum*," 1762. In powers of observation and classification Linnæus has never been surpassed, and he created what may be called a new language for clearly and concisely expressing the facts he perceived.

The "*Systema Naturæ*," though first published in 1735, received in the successive editions which appeared during the author's lifetime great additions and improvements, and only the volumes which appeared at Stockholm under this title in 1766, 1767, and 1768 can be considered as Linnæus's completed work.

The "*Systema Naturæ*" treats of the animal, vegetable, and mineral kingdoms. In the first division of animals into sub-kingdoms Linnæus bases the characters upon the internal structure, thus:—

| | | |
|---|---|---|
| Heart having two ventricles and two auricles. Blood warm and red. | Viviparous . . . | Mammalia. |
| | Oviparous . . . | Birds. |
| Heart having one ventricle and one auricle. Blood cold and red. | Respiration voluntary . | Amphibia. |
| | Respiration by gills . | Fishes. |
| Heart having one ventricle and no auricle. No red blood. | Having antennæ . . | Insects. |
| | Having tentacula . . | Vermes. |

He gives, besides, other characters of each sub-kingdom, founded on differences of the organs of respiration, of sense, of locomotion, etc. At the head of each Class is given a concise description of its distinguishing characters, and this description is so worded as at the same time to give us an explanation of all the *terms* belonging to the Class. Then follows a list of all the more important writings relating to the Class. After this the author lays down the characters of the *Orders* of each Class, and then he divides the several Orders into *genera*, giving the character of each *genus*. For instance, he divides the Mammalia into seven Orders, distinguished chiefly by differences in the number, position, or shape of the teeth. The teeth are also made, in a great measure, the distinguishing marks of the divisions of the Orders of Mammalia into genera. The distinctions of the Orders of Birds are taken chiefly from the beak; Amphibia and Fishes are divided according to certain characters of their respiratory and locomotive organs. The sub-kingdom *Insecta* was almost without methodical arrangement before the time of Linnæus, although, of course, an immense number of facts relating to the development, metamorphoses, and structure of insects had already been accumulated, and the various species of insects had been described and figured. The arrangement of Linnæus divides insects into Orders founded on the differences in the number and texture of their wings. The Class of *Vermes* was divided by our author into five Orders, founded on very various characters.

We have just seen that Linnæus divided the animal kingdom, first, into six *Classes*, or sub-kingdoms; secondly, each Class into *Orders*;

thirdly, each Order into *genera.* The next step he makes is to divide each *genus* into *species.* The name assigned by Linnæus to each *species* is expressive of the essential difference between that and every other species belonging to the same genus. His definitions or descriptions of the species include generally, in a very few words, some distinction which completely identifies the animal in question, whereas a long and laboured description was previously required. The importance of definite plans of arrangement, in the study of the almost infinite diversity of objects which nature presents, will be obvious to every person who has ever had occasion to deal with a series of objects of even limited number and diversities. For example, every one who has a few scores of books on his shelves is obliged to arrange them in some fashion or other. The basis of the arrangement may be the sizes, or the bindings, or the dates, or the subject matter, according to the convenience of the person who is to use them. Similarly a man of business may arrange his letters according to the names of his correspondents, or those of the places from which they are sent, or by the dates or the subjects to which they relate. Each arrangement, having for its object nothing but facility of reference in the examples just adduced, may bring together books or letters which have no other connection with each other; and the same thing will be true also, to a greater or less extent, of classifications of natural objects which regard facility of reference. Thus, reversing some of the plans of classification which writers on botany had recourse to before the time of Linnæus, we find some authors adopting an *alphabetic* arrangement; others classing plants according to their time of flowering; others, according to the *places* in which they grow; others, according to their medicinal properties. But Nature, though following no arrangement or system, has established certain similarities about numbers of her productions that cannot be overlooked. Thus, in the vegetable world there are general resemblances of appearance, habit, flowers, fruit, etc., amongst extensive series of different kinds of plants. An arrangement founded upon such groups of character, duly co-ordinated, would give what is termed the natural method, in which objects originally allied would appear in due relation with their neighbours. Botanists, however, for a long time drew up systems founded rather on distinctions of some one part of a plant, which would give a ready plan of reference. Gesner, Cæsalpinis, Morison, Ray, Rivini, Boerhaave, Tournefort, selected either the fruit, or the flower, or both. Magnal published in 1720 a scheme in which the forms of the *calyx* (the cup-shaped circle of leaves which usually support the gayer coloured petals of the *corolla*) are used as the basis of the arrangement. Linnæus was the first who made the stamens and the pistils the bases of an *artificial* system of classifying plants, and he was induced to select these organs on account of their importance and the readiness with which they could be observed. In the concise descriptions he gives of these specific differences the genius of Linnæus is particularly displayed. To each species Linnæus attached a *trivial* name, in some cases indicating the place where the plant or animal is found, or its shape, colour, or other remarkable peculiarity; and in other cases where the species has been already commonly distinguished by some well-known name, he retains that name. Thus we have *Viola palustris* (the marsh violet); *Viola odorata* (the sweet violet); *Viola tricolor* (the heartsease or pansy): *Sciurus Javensis* (the Javan squirrel); *Sciurus macrotis* * (the long-eared squirrel); *Sciurus niger* (the black squirrel). Again, *Tetrao* (grouse) is the name of one of the *genera* included under the *Order* of Gallinæ (poultry), and the partridge and quail, which belong to this *genus*, are named respectively *Tetrao perdix* and *Tetrao coturnix*. These examples illustrate the improvement in scientific nomenclature that was effected by Linnæus, when he made the name of each species of animal, plant, or mineral to consist of two words, the first indicating the *genus* to which the *species*, expressed by the second word, belongs.

The grasp which a well-arranged system of classification gives of objects existing in numbers so vast that no human mind could study them without some co-ordination, will be obvious when we consider that though there are now nearly a hundred thousand known species of plants, these are so classified that any given plant can be readily referred to the species to which it belongs. Without some scheme of division and subdivision, it would have been impossible even to find names for each of these hundred thousand species. The binomial system of Linnæus enables us to give the name of any species with ease and readiness. The system of classification which he applied to plants was, as already observed, an *artificial* one. It surpassed all preceding systems in the simplicity of the characters upon which the distinctions were founded; its larger subdivisions were easily retained in the memory; and though it separated in many cases nearly allied species, this imperfection was obviated by some modifications in the rigidity of its application. Again, some of the characters are not invariable; and another defect is that unless a plant were given in full flower, it is impossible to determine from it the Class and Order to which it belongs. Its author was himself sensible of these imperfections, and at the same time that he elaborated his system as the most useful *index* to the place of a given plant in the vegetable world, he declared that a *natural* system was the great *desideratum* of classificatory botany. Linnæus divided the vegetable kingdom into *Classes*, the Classes into *Orders*, the Orders into *genera*, and genera into *species*.

The characters of the Classes are founded on the number, situation, relative lengths or arrangement of the *stamens*, which usually admit of easy observation. The Classes are twenty-four in number, and they are thus named:

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* A word of Greek derivation, signifying long-eared.

## LINNÆAN CLASSES OF PLANTS.

| | | | | | | |
|---|---|---|---|---|---|---|
| CLASS | I. | MONANDRIA...... | Stamens separate, | of nearly equal length, | and in number | 1 |
| ,, | II. | DIANDRIA......... | ,, | ,, | ,, | 2 |
| ,, | III. | TRIANDRIA ...... | ,, | ,, | ,, | 3 |
| ,, | IV. | TETRANDRIA ... | ,, | ,, | ,, | 4 |
| ,, | V. | PENTANDRIA ... | ,, | ,, | ,, | 5 |
| ,, | VI. | HEXANDRIA...... | ,, | ,, | ,, | 6 |
| ,, | VII. | HEPTANDRIA ... | ,, | ,, | ,, | 7 |
| ,, | VIII. | OCTANDRIA ...... | ,, | ,, | ,, | 8 |
| ,, | IX. | ENNEANDRIA ... | ,, | ,, | ,, | 9 |
| ,, | X. | DECANDRIA ...... | ,, | ,, | ,, | 10 |
| ,, | XI. | DODECANDRIA ... | ,, | ,, | ,, | 12—19 |
| ,, | XII. | ICOSANDRIA ...... | ,, | rising from calyx, | | 20 or more. |
| ,, | XIII. | POLYANDRIA...... | ,, | rising from receptacle, | | 20 or more. |
| ,, | XIV. | DIDYNAMIA ...... | ,, | two long and two short. | | |
| ,, | XV. | TETRADYNAMIA. | ,, | four long and two short. | | |
| ,, | XVI. | MONADELPHIA... | Stamens united | by filaments | into one bundle. | |
| ,, | XVII. | DIADELPHIA...... | ,, | ,, | into two bundles. | |
| ,, | XVIII. | POLYADELPHIA.. | ,, | ,, | into more than two. | |
| ,, | XIX. | SYNGENESIA ...... | ,, | by the anthers. | | |
| ,, | XX. | GYNANDRIA ...... | ,, | with pistil on a column. | | |
| ,, | XXI. | MONŒCIA ......... | Stamens and pistil | in different flowers | but on same plant. | |
| ,, | XXII. | DIŒCIA............ | ,, | ,, | on different plants. | |
| ,, | XXIII. | POLYGAMIA ...... | ,, | on the same flower, and on different flowers in the same or on different plants. | | |
| ,, | XXIV. | CRYPTOGAMIA ... | Flowers absent or inconspicuous. | | | |

The first ten Classes proceed in a regular series, and are named according to the number of free stamens. The eleventh Class has twelve as the number of stamens, as no plants are known with eleven stamens. The names of all are derived from the Greek numerals, and the words *ανερ*, male; *γυνε*, female; *δυναμις*, power; *αδελφος*, brother; *γαμος*, marriage; *κρυπτος*, concealed; *οικος*, house; *συν*, together; and *γενεσις*, origin.

Under each *Class* are arranged *Orders*, but the characters on which these are founded are not all drawn, like the characters of the *Classes*, from a single part of the plant. The Orders of the first thirteen Classes are, however, defined by the number of pistils, and are named Monogynia, Digynia, Trigynia, etc. The nature of the fruit and other distinctions furnish characters for the other eleven Orders.

It is in these greater divisions that the Linnæan system is artificial, for the *species* of plants, as distinguished by Linnæus, fall into *genera*, in which the natural affinities are recognized; and this part of the classification is conducted on similar principles in all systems. As in the case of animals, the scientific name of a species of plants consists of two *Latin* words, the first of which is a substantive, and designates the *genus;* the second is an adjective, and indicates the *species; e.g., Rosa canina.* Giving the equivalent in English, we usually put the adjective first; *e.g.*, the plant just named is termed the Dog Rose.

Linnæus himself had sketched an imperfect *natural* system of classification of plants, but abandoned the attempt to complete it. Ray put forward a system founded on the natural method, but it was neglected until Jussieu (1748—1836) developed it, 1789. The primary divisions in Jussieu's scheme depend on the characters of the embyro and of the petals, and on the positions of the stamens.

Jean-Baptiste-Pierre-Antoine de Monet (1744—1829), better known as Lamarck, was born at Bazentin, a village in Picardy. His father intended him for the Church, and his education was entrusted to the Jesuits of Amiens. On his father's death, which occurred when Lamarck was sixteen years of age, the youth was free to gratify his own ardent and cherished desire of bearing arms in the service of his country. Lamarck began his military career with great brilliancy, and was very soon made lieutenant. Fortunately for science, an affection of the glands of the neck obliged him to submit to a surgical operation, and to retire from the army. He was allowed a small pension (400 francs), and he obtained employment at a bank in Paris. Drawn irresistibly towards the study of nature, he used to contemplate the plants in the botanical garden, and soon came to feel that in scientific discovery he might find the path to a fame as honourable and more lasting than any that martial achievements would confer. His own studies and observations having shown him the imperfections of the botanical system then in use, he wrote a French *Flora*, with which Buffon was so pleased that he had it printed at the royal printing office. This was in 1778, and the following year Lamarck was elected a member of the Academy of Sciences. He was now engaged to write some volumes on botanical science, in the great "*Encyclopédie Methodique*" of D'Alembert and Diderot. At this time he depended chiefly upon his pen for the means of subsistence, and, like many others in a similar position, he had for fifteen years many difficulties to contend with. But when the great Museum of Natural History was established, Lamarck was appointed co-professor with Geoffroy St.-Hilaire (1772—1844), who at twenty-one years of age had been ordered by Daubenton to undertake to teach zoology as far as regards the higher classes of animals. To Lamarck was entrusted the Invertebrate class of animals, namely, insects, mollusca, worms, zoophytes, etc. Lamarck, after a year of preparation, began his course of instruction at the Museum in 1794. He then divided the animal kingdom into the two great sections of Vertebrates and Invertebrates. While keeping the classification of Linnæus as regards the Vertebrates, Lamarck in 1794 divided the Invertebrata into mollusca, insects, worms, echinoderms, and polypes. In 1799 he separated the *Crustacea* from *Insecta*, with which they had hitherto been confounded. Again, in 1800, he distinguished *Arachnida* (spiders) from insects. Later, in 1802, he separated *Annelida* from *Vermes* (worms), and *Radiata* from polypes.

Some works relating to physics, chemistry, meteorology, and geology, which Lamarck published at various periods, need not here be noticed, as we are now concerned chiefly with his contributions to zoological science. It may, however, be remarked that Lamarck was one of the first to discard the notion of violent convulsions as the agents of the vast changes which geology recognizes as having occurred on the surface of our planet. He conceived that such agents as are now slowly producing changes would be competent to produce all the transformations the earth's surface has undergone, in a space of time sufficiently extended; and as, in nature, time presents no difficulties and has no limitations, so may thus the greatest effects have been produced by the prolonged operation of comparatively small causes. Palæontology also is indebted to Lamarck for the important distinction of littoral from deep-sea fossils. In 1802 Lamarck published a work on the Organization of Living Bodies, and in 1809 appeared "Zoological Philosophy," in which the ideas enunciated in the former work were more fully developed. His "Natural History of the Invertebrate Animals," published in seven volumes, from 1816 to 1822, is his largest work, and it was received by scientific men with universal approbation. This work is concerned only with the description and arrangement of the vast number of species embraced in the section of the animal kingdom to which it relates. It remains a monument of the scientific labour of its author, but of the amount of labour which such an undertaking involves those only who have themselves wrought in the same field can have any adequate conception. The greater part of the Invertebrate division of the animal kingdom was then almost unexplored, in a scientific sense. The classifying genius of Linnæus, after dealing successfully with the Vertebrata, left the other division in a state of chaos; for, separating only *Insecta*, he left the class of *Vermes* (worms),—a kind of limbo to which were consigned creatures of most dissimilar and incongruous descriptions. It was by exploring this confused and comparatively unknown region that Lamarck has earned the eternal gratitude of the systematic zoologist. The achievements of this illustrious Frenchman will appear the more extraordinary, when we observe that he did not commence the study of zoology until he was fifty years of age. The scientific reputation which Lamarck and Geoffroy Saint-Hilaire ultimately attained justified, in the most remarkable manner, the choice of these two men for the professorships of natural science at the Museum. Strangely enough, each of them only began the study of his special subject when he entered upon his professorship, and the one was a mere youth, the other a man already middle-aged. Saint-Hilaire at twenty-one years of age was studying mineralogy, under Haüy, when Daubenton said to him, "I take all the responsibility of your inexperience upon myself; have but the courage to undertake to teach zoology, and one day it may be said that you have created a French science." Fortunately for zoology, and especially for philosophical zoology, Saint-Hilaire had the courage. The investigation by Lamarck of the Invertebrata, involved a vast amount of work with lenses and microscopes, and this, it is said, occasioned such a strain upon his eyes, that they began to fail, and ultimately he lost his sight altogether. To add to his misfortune, his early savings were lost by some unfortunate investments, and, having now only his small salary on which to depend, his position became, it is feared, one of comparative poverty. He passed the last ten years of his life in a state of total blindness, but tended by the affectionate care of his two daughters. Lamarck died on the 18th of December, 1829, at the age of eighty-five years. The amount of work he had accomplished in his own department may be judged by the circumstance, that when he died the number of known invertebrate animals had been so increased by his researches, that it became necessary to divide the department between two professors.

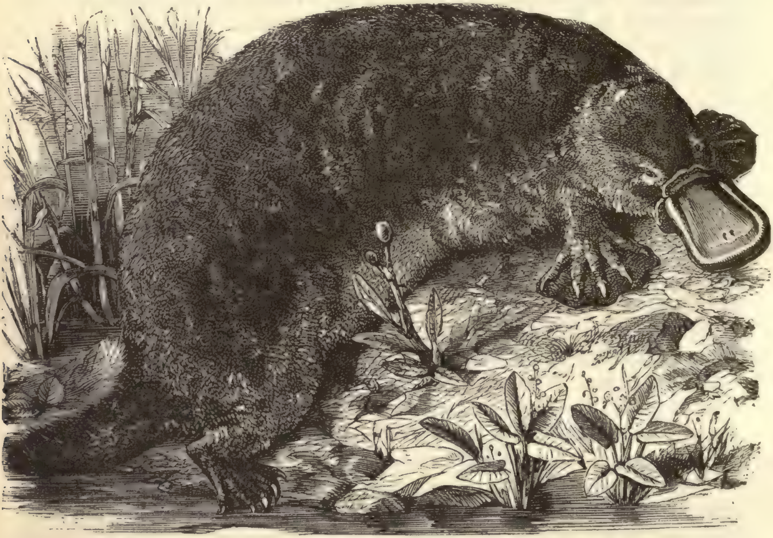


FIG. 179.—THE DUCK-BILLED PLATYPUS.

As the views advanced in Lamarck's "Zoological Philosophy" have in recent times become very prominent, and have had no little influence on scientific thought; although they attracted comparatively little notice at the time they were first given to the world, it will be proper to place those views before the reader. Lamarck begins his work by asserting that all the divisions which have been established between the various productions of nature are essentially artificial; that is, that nature produces the individuals without regard to distribution into genera, species, etc. It is necessary for us to distinguish by some order the infinitely numerous objects which are presented to our observation, but the classification which enables us to do this is in reality arbitrary and artificial. Where nature has insensible gradations, we have found it desirable to draw lines which have (but vainly) been supposed to indicate hard and fast natural distinctions recognized by Nature herself. There are indeed cases in which it would at first sight appear that nature had set a class distinct apart from the rest. Take the instance of mammals and birds, which appear to constitute classes definitely separated by nature. This, however, is merely an illusion caused, says Lamarck, by our ignorance of animals which exist or have existed. The *Ornithorynchus*, or *Duck-billed Platypus* (Fig. 179), an aquatic creature found living in Australia, is justly cited by Lamarck as a surviving member of a group which once filled the existing gap between mammals and birds. This creature is four-footed, and covered with hair, but it has spurs like a fowl, and its mouth is like a duck's bill. Even the division of the animal kingdom made by Lamarck himself into the two great and apparently discontinuous sections of Vertebrates and Invertebrates has since his time been rendered much less sharp by the discovery of the little creature called the Lancelet (*Amphioxus lanceolatus*), represented in the annexed cut, Fig. 180. The amphioxus is now classed along with fishes, but on its first discovery it was regarded as a mollusc. It is in fact a connecting-link between the two classes, and therefore between the Vertebrata and the Invertebrata. It is about 2 inches long; it has no skull, no brain, no jaw, no limbs, no distinct heart; but there extends from one extremity to the other a structure called

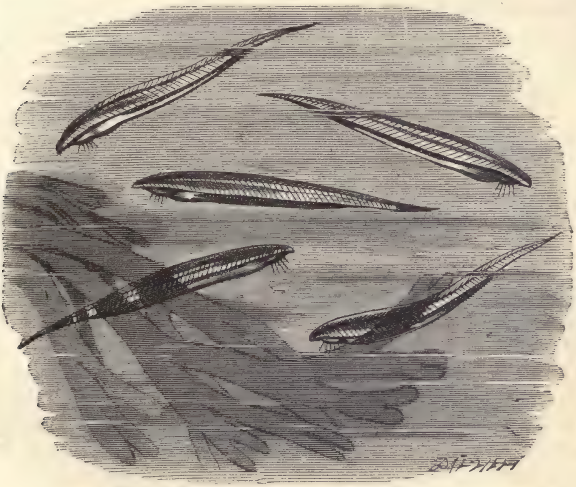


FIG. 180.—AMPHIOXUS LANCEOLATUS.

the notochord, a slender rod which is precisely the embryonic condition of the spinal column in the higher vertebrate animals. Thus if Nature appears in any case to draw distinct lines of separation, it is only that a gap occurs at that part of her otherwise insensibly gradated series, and the progress of science tends continually to obliterate these lines of demarcation instead of intensifying them, as would be the case had Nature herself fixed barriers between living things by absolute distinctions. Further, the circumstance of so many different schemes of classification having been successively brought forward, to the great and increasing complication of science, proves that Nature has not herself made those distinctions which for our convenience of study and reference we find it necessary to make artificially and arbitrarily. This is of course quite consistent with the fact that the productions of nature have relations with each other with regard to which they fall into one order of arrangement rather than into another. The continuity of the animate world is strongly dwelt upon by Lamarck, but he does not say that the existing races of animals form a simple linear and uniformly gradated series; he asserts that they form series ramifying and irregularly gradated, and with a continuity not the less real because it is not linear. The species with which each branch ends are connected on one side at least with one species into which they shade off. In all this there are no hypotheses, no suppositions. This gradation of species is the great difficulty of the terminological zoologist and botanist. When but few species of a given genus were known, it was easy to distinguish them, but new discoveries are constantly filling the gaps and obliterating the distinction, which at length can hardly be made minute enough. Lamarck then points out that well-known facts show that the individuals of a species are liable to be modified by the influence of surrounding circumstances, such as change of climate, soil, food, etc. This modification may, he thinks, well proceed so far that a naturalist would come to class the modified plants or animals as different species. The seeds of the grass which flourishes in the moist meadow may be wafted on the slope of a neighbouring hill, from which perhaps after in numberless successive generations its characters are changed, its seeds may reach still higher ground, some dry and mountainous region say, in which its characters undergo further change from the original type flourishing in the lower and damper soil. From many such considerations Lamarck concludes that the notion of fixed unvariable species is an illusion. He enunciates as clearly and definitely the hypothesis of evolution as applied to living things, as Laplace had expressed the same doctrine in explanation of cosmical phenomena in his celebrated nebular hypothesis. According to Lamarck, we may gather from a general survey of the life of the globe, that organized bodies are the products by natural operations acting through a long space of time; that nature commenced and is ever recommencing to form the simplest organizations, these being the only organizations directly formed from lifeless elements by a kind of spontaneous generation; that these rudimentary sketches of animals and vegetables, placed in suitable surroundings, develop little by little various organs under the influence of the special circumstances by which they are environed; that the modification thus acquired tends to be preserved by the property inherent in all living things of producing beings similar to themselves; that the change which every part of the surface of the earth has successively undergone has given rise to varied conditions, which have so modified the organs of living things, that they have been by insensible degrees brought to that variety of form and development in which we now behold them; that species even now have only a relative stability, and are not as old as the establishment of the present order of things on the surface of the earth. Lamarck anticipates an objection which nevertheless has since actually been again and again urged against the transformation of species. The animals which have been embalmed in the Pyramids of Egypt for several thousand years can be recognized as identical in every respect with those which still exist there. Now, says Lamarck, it would be very singular if they were otherwise, for the position and climate of Egypt are to-day very much the same as they were three thousand years ago. The stability of things in nature, he truly says, is but an illusion, for man judges only by the changes he witnesses: compared with the changes which he observes the earth has undergone when he considers its condition at periods relatively to him immensely remote, an intermediate age may appear, by reason of the shortness of his existence, a stable condition indefinitely prolonged. Lamarck then cites the known changes in organization which changes of habit have produced, as in the case of domestic fowls, etc., and refers to the extinct animals and plants as proving at once the continuity of nature and the existence of diverse environments in past geological epochs.

Naturalists would waste their time in vain by describing new species and noting minute shades of difference if their labours had no other result than to add to the long list of registered species; and equally vain would be the labour of arranging and rearranging species into genera by one set of considerations after another, unless these rearrangements tend to present the animal series in relations more and more conformable to those in which the animals stand by the operation of nature. Our classifications, artificial though the lines of demarcation, will be perfect only when they present the succession and connection of species in the order and relation in which nature has placed them. These relations are discoverable only by comparison of the organization of the individuals, and the degree of importance of the different kinds of organs must be considered. Lamarck lays great stress on the *progressive* "degradation" or simplification which is observed in the organization of animals as we descend in the scale from the more to the less highly organized classes. The indication of this progressive degradation in organization may be crossed, confused, and concealed by the modifications of organization which adapt animals to different habits of life. Taking the Mammalia, he places the hoofed vegetable-eating mammals below the digitated or carnivorous, and above the *Cetacea* (whales, etc.), which have only one pair of limbs, with the digits not externally separated, and without masticating organs. Although the Amphibia as a class are below the Mammalia, it does not follow that all the Amphibia are beneath the lowest of the Mammalia in organization. Birds, by the organization of the heart, warm blood, and some other important circumstances, constitute the class ranking immediately below Mammalia; but they exhibit a marked degradation in being oviparous. The *diaphragm* dividing the chest from the abdomen, and the *mammæ*, which are wanting in the birds, do not reappear in any lower class. Birds retain a very important feature of the circulatory system of the higher classes, inasmuch as they have a complete pulmonary circulation, the *whole* of their blood passing through the lungs, and this arrangement is not found again in any lower class. Reptiles resemble all the classes below them in being cold-blooded. They have a heart with only one ventricle, and are the last class in the series which breathes by lungs. The respiratory organs of all the classes below are never in a proper sense lungs. The lungs of reptiles are, however, much simpler than those of the animals above them, and only a portion of the blood passes through them. Some species breathe by lungs only in the mature state, and others are without the limbs which all other Vertebrates possess. The lowest class of Vertebrates is the Fishes, not because their outward form is so different from other Vertebrates, but by reason of their inner organization. Their respiration is aquatic, and performed by means of the *branchia* (or gills). The oxygen necessary to respiration is derived from air dissolved in the water, and the water is taken by fishes into the mouth in order to reach the respiratory organ. Fishes are the lowest class of animals which breathe by the mouth. Their skeleton may be called a rudimentary sketch of that found in the higher animals; the heart has only one ventricle; the brain is very small. In passing down the scale, organs which are of the greatest importance with higher animals successively disappear. The eyes become simpler in structure, and then cease to be found at all; the nervous system becomes continually less complex, until at length mere traces of its existence are all that can be distinguished. The head, which is distinct enough in some of the higher classes of Invertebrates, ceases to be found in the lower. We come at length to creatures in which almost the only organ is a stomach or cavity for the reception of food. Finally we reach the confines of the animal kingdom in minute, transparent, gelatinous bodies of apparently uniform structure throughout, with little consistence, yet exhibiting the irratibility which characterizes animal substance, contractile, receiving nourishment from the liquids they inhabit by general absorption. These points of animated jelly appear to be perfectly homogeneous through their whole mass, and having little consistence, they cannot be said to possess organs at all.

Lamarck's explanation of these facts is that all the species of animals have been formed by nature, beginning with more simple or imperfect, and proceeding gradually to the more complex forms. The modification of organs in different animals he attributes to changes in the habits of the animals occasioned by changes in the surrounding circumstances. That changes in the circumstances in which a plant or animal is placed will give rise to changes in its form, colours, etc., he adduces many well-known cases to prove,—as, for instance, the differences which cultivation produces in plants. Changes of the surrounding circumstances will cause changes in the wants of animals; changes in the wants will determine changes in the habitual actions; and habitual actions will influence the organs according to the well-known law that constant disuse of an organ weakens and diminishes that organ, so that at length it may become powerless; while, on the other hand, habitual use will increase the power of an organ. Lamarck's idea was that these effects on the individual are transmitted to the offspring, and if the same conditions are continued through a long succession of generations, the divergences from the original stock widen until the differences of organization are such as we recognize in the various species.

The explanation of the origin of the difference in species by changes induced in individual animals by their habits of life, was the weakest part of the Lamarckian theory. To explain, for example, the peculiarities of the wading birds, he supposes that originally some birds, compelled to resort to shallow waters and the margins of streams for their prey, being unwilling to swim or even to wet their bodies in the water, used every effort to stretch out their feet, and that by long-continued efforts of this kind it came to pass that the birds were at length provided with legs on which they are mounted as if on stilts. The inadequacy of Lamarck's theory on this subject is perhaps more obvious in the following illustration from the same chapter:—"Among the carnivorous animals are some which are obliged to catch their prey by their speed; now such an animal, whose wants, and therefore habits, require that it

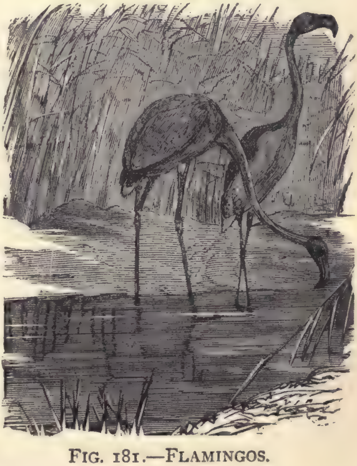
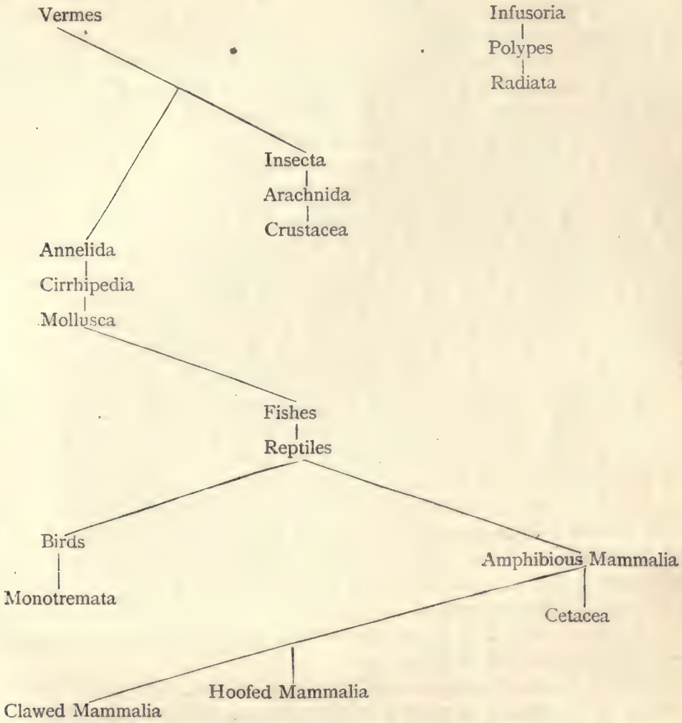


FIG. 181.—FLAMINGOS.

should seize others by deeply burying its claws into their bodies, must, *by these repeated efforts*, get claws of a size and curvature that would impede its motion in walking or running over stony ground; it has come to pass in this case that the animal has been obliged to make other efforts to draw back these too prominent and hooked claws by which it was inconvenienced, and the result has been the formation by little and little of those peculiar sheaths into which cats, tigers, lions, etc., retract their claws when they are not in use." The reader will hardly fail to notice how very unsatisfactory, not to say impossible, is this explanation of the process by which the modification of organs was brought about; and this was doubtless the reason why these views on the evolution of species attracted comparatively little attention until a later period, when the fatal defect of Lamarck's theory was removed by certain new views brought forward by a celebrated English naturalist still living. It is because Lamarck's theory, after having slowly acquired support and recognition from various quarters, and especially after having received from Darwin's speculations increased vitality, has obtained in some form a very general acceptance amongst scientific naturalists, that the views of the French naturalist deserve a degree of attention which they could not claim merely by their boldness and originality.

Lamarck's theory, then, regards the various species of animals as produced by modifications of originally simple forms, increasing progressively in complexity of organization by the operation of laws which have been always the same. The modifications which present themselves to us in the various forms of animal life have required for their evolution a period of time to which the duration of human life, or even the interval which has elapsed since the epoch of our most ancient annals, may count as but a second. The changes actually in progress are unperceived, by reason of their relative slowness. A supposititious comparison will make this clear. Suppose that the duration of human life extended only to a *second* of our actual time, and that it was given to us to contemplate the face of a clock such as we now possess. No individual could ever notice the motion of the hour hand in the course of his own life, and even the comparison of the recorded observation of sixty generations might fail to detect it; but even if some persons, by comparison of still older observations, had acquired the conviction of the reality of the motion, others would not share this conviction; for seeing the hour hand stationary on the dial, as their fathers before them had also seen it stationary, they would declare that those who asserted its motion were mistaken in their inferences.

In a supplement to the "*Philosophie Zoologique*," Lamarck gives a table exhibiting the conjectural descent of the several sub-kingdoms of animals. He thinks that the lowest part of the scale may have two unconnected divisions, and that the series in series sends off branches which have terminated their development at a comparatively low point in the scale. Here is the table :—


Vermes
Infusoria
Polypes
Radiata
Insecta
Arachnida
Crustacea
Annelida
Cirrhipedia
Mollusca
Fishes
Reptiles
Birds
Monotremata
Amphibious Mammalia
Cetacea
Hoofed Mammalia
Clawed Mammalia

In another part of his work Lamarck points out that in science the only positive truth obtainable by man is the knowledge of facts—that is, of the actual succession of phenomena. Beyond there is room for any amount of uncertainty; but nevertheless certain inferences, theories, opinions, etc., as to the actual connection and causes of phenomena, are much more probable than others. He places two conclusions before the choice of his readers: the one, that which had up to his time been in vogue, namely, that the Author of Nature had abruptly created animals in species having organizations determined and invariable in all their parts, and being compelled therefore to live in such climates and places only as were compatible with that organization; the other, which is his own conclusion, that the Supreme had created such an *order of things* as gives rise by degrees to one species of animals after another, beginning from the simplest or most imperfect, and gradually ascending to the more highly organized or perfect; all undergoing the influence of the circumstances surrounding them, and by modification of their habits and organs, ever coming into that harmonious adaptation to their environment which we now observe. That the conditions which surround living things at the surface of the earth have changed, and are still changing, were facts well established in Lamarck's time; and he observes that those persons who adopt the first of the two above-mentioned conclusions will have to admit that the Creator at the beginning foresaw all the various sets of conditions under which living things would have to exist, and gave to each species its immutable organization once for all. Moreover, the species supposed to have been originally created at one time must, by reason of their organization, incapable by hypothesis of change or adoption, have become one after the other unable to survive the admitted changes in their habitations and surroundings. If special interventions and exertions of creative power be invoked for the production of new species from time to time, the difficulties of the position are not removed. Such special interventions in the order of things are not claimed for the production of the individual plants and animals of any species, nor for the production of the *varieties* of a species. Now the distinctions into species, Lamarck contends, are arbitrary, as one species shades insensibly into another; the characters which naturalists rely upon for their discrimination are often very minute and insignificant; and he thinks that it is more likely that the Author of Nature should have exerted His power in creating a few organizations capable of development and adaptation, than that He should have engaged in the immediate production of forms, distinguishable only by modifications of a kind which it is acknowledged the ordinary operations of nature are competent to produce.

George Louis Leclerc Buffon (1707—1788) was born at Montbar in Burgundy of very wealthy parents, and he enjoyed all the advantages of education and training which wealth could procure. Buffon while a youth made the acquaintance at Dijon of a young English nobleman, whose tutor happened to be well versed in scientific studies, and it was perhaps this circumstance which turned Buffon's thoughts to scientific pursuits. At twenty-one years of age he succeeded to his mother's estate, which put him in possession of an annual income equal to £12,000 sterling. His wealth did not cause him to relax his exertions in the pursuit of knowledge. Having visited Italy, sojourned for a time in England, he returned to France, and soon afterwards published translations of two famous English scientific works,—Hale's "Vegetable Statics" and Newton's "Fluxions." He also carried out an experimental investigation into the strength of timber, and made a very large burning-glass with which he performed some striking experiments that attracted much attention. Du Fay, who was at the head of the management of the Jardin du Roi (now

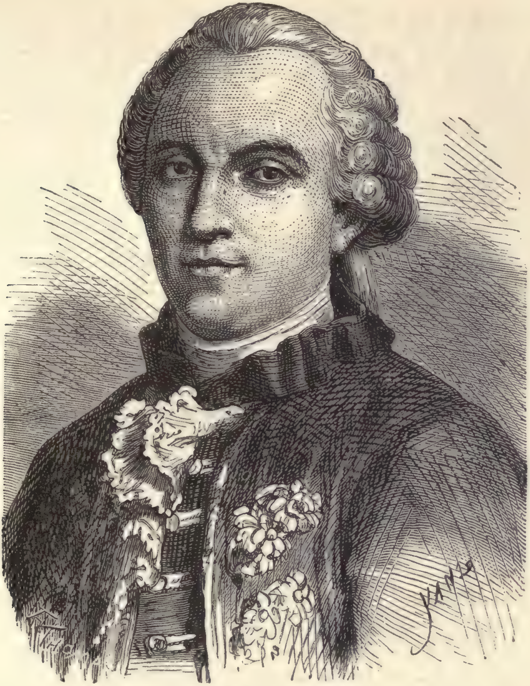


FIG. 182.—BUFFON.

called the Jardin des Plantes), on his death-bed named Buffon as his fittest successor, and the person who better than any other could rightly direct that national establishment. Buffon was accordingly appointed in 1739, and profited by the opportunities which were placed within his reach for the prosecution of the study of natural history, to which he had manifested the most devoted predilection. Buffon held this appointment until his death, and continued during the whole period of half a century to occupy himself with the study of natural history and the composition of the voluminous work for which he is so famous, "*L'Histoire Naturelle.*" This great work, which fills thirty-six octavo volumes, was intended to cover the whole field of natural history by giving a complete description of animate and inanimate nature. Though Buffon possessed great industry, and laboured at his task for fifty years, only a portion of this great scheme was completed. The work actually produced remains, however, a monument of the genius and perseverance of its author. It is not a series of systematic treatises containing in technical language all scientific details belonging to its subjects, nor does it possess those qualities of undeviating precision and minute accuracy which scientific experts would most highly value. Buffon was in fact not qualified by his mental constitution for the production of a formally scientific synopsis. He delighted in general views, wide theories, and pictorial descriptions. In executing the first part of his undertaking, Buffon was fortunate in obtaining the aid of a coadjutor who possessed in an eminent degree precisely those qualities in which he himself was most deficient. The first fifteen volumes of the "*Histoire Naturelle*," which treat of the theory of the earth, animals in general, man, and viviparous quadrupeds, are the joint work of Buffon and DAUBENTON (1716—1799). The latter had been trained as a physician, and was an excellent anatomist, and it was he who contributed to these volumes the descriptions of the anatomy and external form of the animals, while the theoretical views, description of general phenomena, and delineation of the habits of animals, were the work of Buffon. In the subsequent volumes, the anatomical descriptions are meagre and sometimes inaccurate, as in them Buffon had to depend upon coadjutors less competent and less conscientious than Daubenton.

In the composition of his famous work Buffon took extreme pleasure, sometimes passing twelve or fourteen consecutive hours at his writing-table. He took great pains in correcting his writings by revision, until every fault in expression had been detected and repaired. He wrote and copied, read his chapters to his friends, and copied again until he was entirely satisfied. It is said that of the manuscript of the "Epochs of Nature" he made no fewer than eleven transcripts. In point of literary style Buffon was hardly surpassed by any of his contemporaries. The splendour of language with which he invested his descriptions gave an extraordinary impulse to the general study of natural history. The science thus expounded, and divested of dry technical phraseology, became attractive to every one. Buffon eschewed any attempt at classification, and even declined to trouble himself or his readers with any accurate nomenclature for the things he described. His style was captivating by its richness and harmony; but at the present day, when the tendency is to regard clearness of statement as the great and all-sufficing quality of scientific exposition, many passages of the "*Histoire Naturelle*" are liable to be considered pompous and inflated. It was doubtless in allusion to the artificial grandeur of the style, that when some one named the "*Histoire Naturelle*," Voltaire interposed the words, "*Pas si naturelle*." There can, however, be no doubt that Buffon's work diffused the taste for natural history, which, no longer confined to the studies of the learned few, became the delight of all. "Buffon will continue to excite a useful enthusiasm for the natural sciences; and the world will long be indebted to him for the pleasures with which a young mind for the first time looks into nature, and for the consolations with which a soul, weary of the storms of life, reposes on the spectacle of the tranquil submission of an infinitude of beings to necessary and eternal law."

George Cuvier (1769—1832), afterwards Baron Cuvier, is the most illustrious zoologist of the period we are considering. He exhibited extraordinary talents in very early life, for he was able to read fluently before he had completed his third year. Cuvier's case contradicted the observation that precocity of this kind seldom fulfils its promise by the attainments of maturity. His acquirements increased with his years, and at an early age he showed remarkable proficiency in drawing, literature, law, and natural science, while attending the Military School at Stuttgard. But the subjects which engaged Cuvier's attention at one time or another include also languages, philosophy, poetry, physiology, mathematics, chemistry, physics, and agriculture. He may be considered the creator of two new sciences, namely, Comparative Anatomy and Palæontology. It was he who from the scattered bones reconstructed for us the skeleton of the huge extinct vertebrates, the *plesiosaurus*, the *megatherium*, the *palæotherium*, and others. He first enunciated the law of the correlation of organs, according to which a certain conformation of structure in one organ is always found in conjunction with a certain conformation in another.

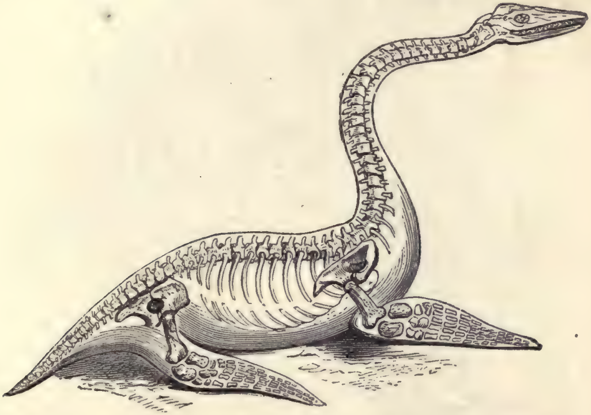


Fig. 183.—Skeleton of Plesiosaurus (an extinct vertebrate).

Cuvier, by tracing the connection which exists between the internal organization and outward form of animals, and by observing how these accorded with their habits and economy, approached nearer than his predecessor, Buffon, to the perfect natural classification. He published in 1795 his first memoir on the Invertebrata, and two years later his "*Tableau Elementaire de l'Histoire Naturelle des Animaux.*" His "Comparative Anatomy" followed in 1800 and 1805; then a long series of memoirs on Mollusca; then his great work on Fossil Bones; and in 1817 "The Animal Kingdom," in which he attempted to arrange all known animals according to their natural affinities as deduced from a comparative view of the whole organizations. Buffon in his "*Histoire Naturelle*" had been contented to follow Pliny, by taking merely the external appearance as the basis of his system. Cuvier improved systematic zoology by making the differences of organization revealed by comparative anatomy his guide. Cuvier divided the animal kingdom into four sub-kingdoms—viz., the Vertebrata, the Mollusca, the Annulosa, the Radiata—because these respectively exhibited four great fundamental types on which all animals appear to have been modelled, and of which all their forms may be regarded as comparatively slight modifications, in which certain organs are more or less developed in various directions while the general organization of the typical form is adhered to. He rejected Lamarck's twofold division into *Vertebrate* and *Invertebrate*, on the ground that there was between the subdivisions of the last as great a difference of structure as between the two divisions themselves.

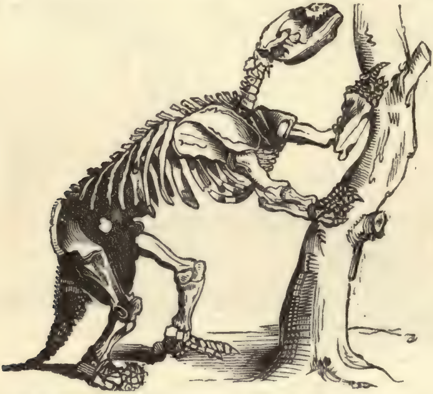


FIG. 184.—SKELETON OF MYLODON ROBUSTUM (an extinct vertebrate).

The latest work undertaken by Cuvier was an admirable description of the geology of the environs of Paris, and in the composition of this work he had for coadjutor ALEXANDER BRONGNIART (1770—1847), who was Professor of Natural History and of Mineralogy at Paris. Brongniart began life as an officer in the army, but after he had shown his ability in natural science, he was in 1800 appointed Director of the Sèvres Porcelain Manufactory, where his scientific acquirements proved of high value. It was he who first divided the *Reptilia* into *Saurians*, *Batrachians*, *Chelonians*, and *Ophidians*.

HALLER (1708—1777) was one of the most indefatigable anato-

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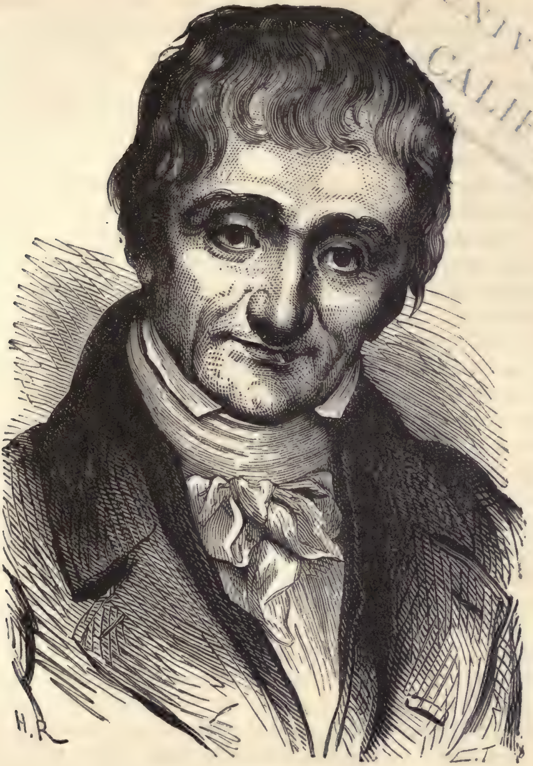


FIG. 185.—ALEXANDER BRONGNIART.

mists who ever lived, and was also distinguished by the achievements of his literary industry. He was born in Berne, and his childhood is remarkable for the precocity of the talents it displayed. At four years of age he not only was able to read the Bible, but was in the habit of expounding it for the benefit of the servants in his father's house. At nine years of age he wrote Greek, and while still a boy he was instructed in anatomy by attending the lectures of an eminent professor. Having selected medicine as his profession, he was attracted to Leyden by the celebrity which Boerhaave and Albinus had conferred on that university. Leaving Leyden, he lived for some time in the house of a skilful anatomist in London. Paris was the next place he visited, and here he placed himself under the most celebrated anatomists, and also took pains to obtain the best instruction in other branches of knowledge. Haller, after holding various academical appointments at different places, was nominated to the professorship of anatomy at the University of Gottingen, then newly established, and where special care was taken to secure the services of the best qualified persons for the several academical chairs. Haller remained at Gottingen for some time, but was afterwards obliged to resign his appointments from want of health, and return to his native country. For a period of half a century he continued from time to time to publish scientific dissertations and treatises to the number of nearly two hundred.

Haller was the first person who proved that in muscular tissue there resides the special property of contracting under the influence of certain stimuli. The movement of contraction is very rapid and marked, and is accompanied by a swelling and wrinkling of the fibres of the muscle. Haller affirmed that a necessary condition of this property, which he termed *irritability*, was the distribution to the muscle of some nervous power. The latter was, however, distinct from the proper *irritability* of the muscular fibres which existed in themselves, although the distribution of nervous power was necessary for its exercise. In the nerves Haller conceived *sensibility* was present, but not *irritability*. Thus, to muscular tissue belongs irritability; to nervous tissue, sensibility: these were the special and peculiar functions of each tissue. He denied that the *irritability* could be derived from the nerves, because although it was obvious that some action of the nerve may stimulate the muscle, the motion produced cannot be derived from the nerves, for it is impossible to suppose that they could impart to other things that which they do not themselves possess. Haller also proved that there are certain tissues of the body devoid of either sensibility or irritability. But irritability, while residing in every part of the body that is possessed of muscular fibre, is present in different degrees and intensities in the various parts. It is most observable in the heart, and more in the left ventricle than in the right. Next in order come the intestines, the diaphragm, the voluntary muscles. From repeated experiments Haller concluded that the heart and other involuntary muscles are not excited to contract by a stimulation derived from the nerves which are supplied to them from other parts of the system, but they require their own special stimuli; for example, it is the blood within the ventricle which is the stimulus to its contraction. Haller, indeed, reduced all the vital functions to these two—sensibility and irritability; the one seated in muscular, the other in nervous tissues. This doctrine was strongly opposed by some physiologists of the time. It was seen that those parts of the body which possessed neither of Haller's two essentially vital properties were not the less alive. Other physiologists defended the new doctrines, and from the discussions which ensued materials for more accurate views were obtained, as criticism detected errors or experiment ascertained facts. It was shown that Haller had failed to recognize the excitability of tissues other than the muscular and the nervous simply because he had not applied the stimulus proper to those other tissues. It was also pointed out that it was an error to confound nervous influence with sensibility.

Among the defenders of Haller's views was CHARLES BONNET (1720—1793), a native of Geneva, who, originally trained for the profession of the law, became from an early age so devoted to natural history, that, although, by complaisance to his father's wishes, he prosecuted his legal studies until at the age of twenty-three, and took his degree of Doctor of Laws, he then relinquished law, to give himself up to follow the bent of his mind. Bonnet pursued so assiduously investigations into the structure of insects, that his eyesight began to be affected by his incessant use of lenses and microscopes. He was reluctantly obliged to lay aside these instruments, and turn to branches of inquiry which would less tax his vision. He then studied plants, and especially the ascent of the sap. In 1754 he published anonymously, in London, an "Essay on Psychology," and a few years afterwards there appeared a treatise by him on "The Faculties of the Soul." In 1769 he published his "*Palingenesie Philosophique*," which comprised some strange notions concerning animal life. Bonnet devised and performed many curious experiments on plants and animals.

No naturalist ever possessed in a higher degree the power of accurate observation than SPALLANZANI, a native of the Duchy of Modena (1729—1799), who at a very early age was Professor of Logic at the University of Reggio, and afterwards held a professorship at Modena. In 1765 he conclusively proved that animalculæ were really animals, the nature of these minute organisms having up to that time been a matter of dispute. In 1785 Spallanzani became Professor of Natural History at Padua, and his lectures there attracted great numbers of students.

Comparative anatomy had in the celebrated JOHN HUNTER (1728—1793) one of its most ardent cultivators. John Hunter was born in Lanarkshire, and his father having died when he was only ten years of age, he was allowed to grow up with little or no education whatever. He was apprenticed to a cabinet-maker, with whom he remained for some time, and, but for the circumstance of his master failing, the genius which displayed itself in discovering the mysteries of animal structure might have been employed in fabricating chairs and tables. When his master failed, John Hunter was in the twentieth year of his age, and, left without any other resource, he wrote to an elder brother, William Hunter, who had established himself in London as a physician and lecturer on anatomy, offering his services as an assistant in the dissecting-room. Hunter intimated that, should this arrangement be impracticable, he would enlist in the army. Fortunately for science, Dr. William Hunter invited his brother to proceed to London, and enter upon the employment he had proposed. John made so rapid a progress in anatomy, that before he had been a year in London he was considered qualified to conduct a class of his own on that subject. His extraordinary ability and the patronage which his brother could command soon obtained for Hunter a surgical appointment at one of the great London hospitals, and he acquired a large private practice as a surgeon. But nearly every shilling that Hunter could save from what he gained by his profession was expended in collecting specimens and subjects for the study of comparative anatomy. Even while his income was as yet comparatively small, he bought a piece of ground in a suburban village, built on it a house in which to deposit his collections, and laid out the ground as a zoological garden, in which he could study the habits of living animals. In spite of his numerous engagements—extensive private practice, lectures at medical school, dissections for students at his own house, duties of surgeon to St. George's Hospital, and of Surgeon-General to the army—Hunter found time to work in his museum, where he used to spend the mornings from sunrise to eight o'clock. He found, too, time for the composition of some important works, and took an active part in the meetings of the Royal Society. In order to obtain subjects for examination, he applied to the proprietors of all the menageries in London for the bodies of such of their animals as died, and in consideration of this he would give them other rare animals to exhibit, on condition of receiving the bodies of these animals when they died.

Hunter died in the sixty-sixth year of his age, and after his death his museum was purchased by the Government for £15,000, and was deposited in the premises in Lincoln's Inn Fields belonging to the Royal College of Surgeons. This magnificent collection, which is said to have cost Hunter in money five times the above-named sum, is one of the most splendid monuments of what the skill, labour, and industry of an individual can effect.

Although Geology took rank as a science only in the eighteenth century, it will not be supposed by the reader that observation and speculation regarding matters which belong to this science were altogether wanting in preceding ages. We have already seen (p. 17) that some of the teachings of the Pythagoreans gave evidence of certain striking geological facts having been observed in ancient times. With the exception of Palissy's doctrine regarding fossil shells, we have not thought it necessary to record the opinions of the earlier writers—who, indeed, occupied themselves merely with vague and fanciful speculations and arguments concerning fossils, Noah's Flood, earthquakes, and so on. Every theory, how foolish soever it might be in a scientific sense, which agreed with the received ideas on these subjects, attracted some supporters. Fossils, as already mentioned, were called "freaks of nature," and some writers were ready to explain the way in which nature proceeded in the productions—*e.g.*, "by setting fatty matter into fermentation," or pouring in the "lapidifying juice," and giving shape to her materials by "tumultous movements of terrestrial exhalations." When ancient learning and science were revived, the country which took the lead in the revival was that in which arose the first rays of the dawn of geological inquiry. Cardano, in a work published in 1552, explained the presence of fossil shells in the mountains by the sea having formerly reached the places where the fossils are found. Majoli suggested that the shells might have been cast up by some submarine volcanic eruptions. It would be tedious to enumerate all the early Italian writers and their opinions; but it may be remarked that, long before similar inquiries were pursued in other countries, we find Steno, a professor of Padua, teaching many of the fundamental facts of geology. He recognized a secondary formation, in which fragments of other rocks were imbedded; he distinguished marine from fresh-water strata, and he believed that all such strata were originally horizontal, but had been inclined by the action of fires from below upheaving the earth's crust. Scilla, a Sicilian artist, published a work on fossils in 1670, in which he found it necessary to argue in favour of their being the remains of animals which once really existed. Scilla seems to have supposed that all fossil shells were traces of the effects of the Mosaic Deluge. This view was subsequently advanced again and again; it was as often shown to be inadequate to explain the facts.

A few authors there are who wrote at the close of the seventeenth century, whose geological speculations deserve some attention as the precursors of the more modern doctrines. We will first name Leibnitz, who was so illustrious by his attainments in many different branches of learning and science. In a work published in 1680, Leibnitz presents a sketch of his general views, and even in the present day it would be difficult more clearly to describe the probable course of the evolution of the earth, such as we now see it, from the molten glowing mass, which he assumes to have been its primordial state. He supposes that the outer crust cooled down and solidified into certain fundamental rocks. The crust, in solidifying, formed cavernous hollows, and when it had become sufficiently cool to allow of the deposition of aqueous vapours a vast sea enveloped the earth; but, by the breaking in of the vaulted hollows in the crust, the level of this ocean was lowered. The disruptions of the crust of the earth and these submarine disturbances must have caused great oscillations of the water, which would wear down much solid matter that in times of quiescence would fall to the bottom of the sea. The strata so formed would, by consolidation, be brought into compact masses. By the repetition of similar frequent alternations a succession of strata would be produced, until the causes of disturbance would be reduced to a nearly balanced condition, and a more permanent state of things would result, after the face of the earth had been many times renewed.

Among the "Posthumous Works of Robert Hooke," published in 1705, appeared a "Discourse on Earthquakes." Hooke was, as we have already had occasion to remark, a very extraordinary man, with regard to the extent of his acquirements and the originality and suggestiveness of his speculations. He followed Palissy in asserting that fossils were truly animal remains. Nature had never, he thought, in a playful mood, formed in stone useless imitations of living things. Hooke observes that many species of animals found fossil in England were no longer to be met with living; and though he considers it not unlikely that such species may still inhabit the depths of the sea, he suggests that some species may have become entirely extinct, and connects their disappearance from the earth with the great convulsions which, according to his idea, had been produced by earthquakes. It was, according to Hooke, the operations of earthquakes "which have turned plains into mountains, and mountains into plains, seas into land, and land into seas, made rivers where there were none before, and swallowed up others that formerly were, etc., and which since the creation of the world have wrought many changes in the superficial parts of the earth, and have been the instruments of placing shells, bones, plants, fishes, and the like, in those places where, with much astonishment, we find them."

Ray, the naturalist (page 238), proposed in 1692 a theory of the changes the earth had undergone, which in its general outline much resembled that of Hooke. But Ray was the first English writer who traced the effects of running water, and of the action of the sea upon the shores, in carrying down the materials of the land. **Woodward**, a contemporary of Hooke and of Ray, examined carefully many of the British strata, and specimens he collected are still preserved at Cambridge as arranged by himself. Woodward, and certain other writers of that period, were too biassed by their preconceived physico-theological views to elucidate new truths. Hutchinson and others objected even to Newton's theory of gravitation, because they could not find it in the Bible, which they maintained comprised a perfect system of natural philosophy. **Moro** (1687—17..), an Italian, attributed (1740) the formation of strata to the effects of earthquakes, which changed seas into lands and lands into seas; and his ideas were a few years later (1749) expounded by **Generelli**, a learned Carmelite friar. Generelli shows that continents and mountains are continually being worn down by running water, and this gives rise to the following observations: "Is it possible that this waste should have continued for six thousand, and perhaps a greater number of years, and that the mountains should remain so great, unless their ruins had been repaired? Is it credible that the Author of Nature should have founded the world upon such laws as that the dry land should for ever be growing smaller, and at last become wholly submerged beneath the waters? Is it credible that, amid so many created things, the mountains alone should daily diminish in number and bulk, without there being any repair of their losses? This would be contrary to that order of Providence which is seen to reign in all other things in the universe. Wherefore I deem it just to conclude that the same cause which, in the beginning of time, raised mountains from the abyss, has down to the present day continued to produce others, in order to restore from time to time the losses of all such as sink down in different places or are rent asunder, or in other ways suffer disintegration. If this be admitted, we can easily understand why there should now be found upon many mountains so great a number of crustacea and other marine animals."—(*Generelli, as quoted by Sir C. Lyell.*)

LEHMANN, a German mineralogist, writing in 1756, distinguished three different classes of mountains: first, those formed at the creation of the earth, and before animals had appeared (these contained no fragments of other rocks, and of course no organic remains); second, rocks resulting from the partial destruction of the former, and also without organic remains; third, those produced by local convulsions, and in part by the Deluge of Noah.

The great earthquake of Lisbon, which in 1755 caused the death of 60,000 persons, strongly attracted men's minds to the subject of earthquakes. The Rev. JOHN MICHELL, Professor of Mineralogy at Cambridge, in 1760 published in the "Philosophical Transactions" an "Essay on the Causes and Phenomena of Earthquakes." These he attributed to the sudden generation of steam produced by the contact of water with intensely hot matter, which he supposes exists at certain depths below the surface. The steam thus generated forces its way between the strata of the earth, heaving up one tract of country after another. If the internal fire lie far below the surface, the earthquake will move with great velocity over a large extent of country; but when the fire is near the surface the earthquake will affect a smaller extent of country, and will move with less velocity. Michell, in pointing out the application of his theory, describes the general horizontality of the strata in low countries, and their disturbed state in the vicinity of chains of mountains. His views are remarkable as anticipating those at which geologists in general arrived only at a later date.

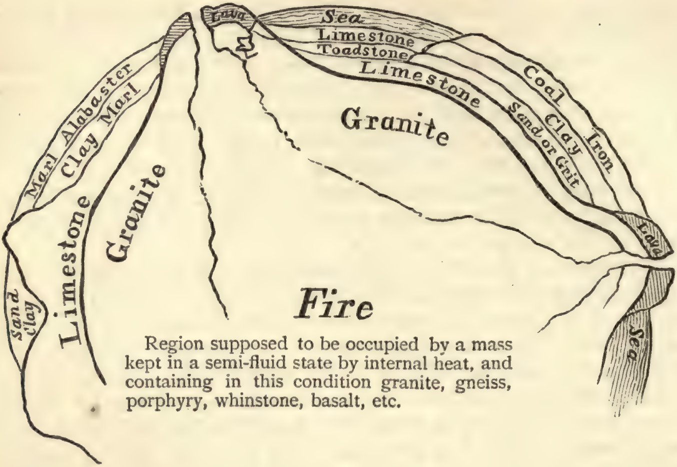
FUCHSEL, a German physician, writing in 1762—1773, describes geologically a certain district of Germany, and he recognizes the relation of the fossils of the various strata to their ages. The separation of the rocks of the globe into separate groups became quite general towards the end of last century. In 1778 PALLAS (1741—1811), after having examined the two great mountain chains of Siberia, announced that the granite rocks were in the middle, the schistose on their sides, and the limestone outside of these last. He conceived that in all mountain chains composed of primary rocks the same law would hold. Worthy of mention is the discovery by the same naturalist of the entire bodies of an elephant and a rhinoceros of extinct species, preserved for ages by being frozen up in ice-banks.

The results obtained by the observations of all the preceding geologists were reduced to a methodical system by WERNER (1750—1817), who was Professor of Mineralogy at Freyberg in Saxony, and his lectures attracted such a degree of general attention to geology as it had never before received. From this period geology began to take the form of a systematic science. Werner was the first to point out the great practical advantage to miners of a knowledge of general geological principles. He also showed the various industrial uses of minerals, and the influence of the nature of rocks and soils upon the wealth and pursuits of the races of mankind.

Werner's lectures were very eloquent, and attracted great numbers of auditors and disciples from every country in Europe, and many eminent men of science studied the German language with the view of learning the principles of the new science from the lips of the great teacher of the day. As he had a distaste for writing, and published only two small essays, his general views were spread chiefly by his pupils, who always returned from their studies in the mining school, to which Werner had imparted the rank of a university, inspired with unbounded confidence in their teacher's doctrines, and full of admiration of his personal qualities. These doctrines—at least, as they were generally understood in the latter part of the eighteenth century—were briefly something like the following. The different rocks of which the earth is composed were counted to number thirty-six, and they were not promiscuously mixed, but had a definite relative position with respect to each other. In general, they extend over the earth in layers enclosing the central nucleus, and lying one upon another like the several coats of an onion; but from several causes they by no means exhibit the kind of continuity and regularity which the comparison suggests. They rise and fall with more or less abrupt inclination from one place to another; at some parts they are entirely absent, either having never been deposited, or having been removed by some cause after their deposition. The position of the different rocks as superimposed upon each other determines their classification. The rocks which are supposed to have had a common origin are called a *formation*, and of these *formations* five were recognized, viz. :—1. Primitive; 2. Transition; 3. Floetz; 4. Alluvial; 5. Volcanic. The rocks which constitute the *Primitive* formation do not by any means everywhere lie at the greatest depth below the surface. On the contrary, they frequently constitute mountain chains; and, in fact, the loftiest mountains are composed of this formation. The *Transition* and *Floetz* formation also frequently form mountains. The Primitive formation itself consists of five kinds of rock, always following each other in the same order, which, beginning from the lowest, is granite, gneiss, mica slate, clay slate, porphyry. It is true there are certain other kinds of rock interposed occasionally and to a limited extent between these, but they are regarded as merely *subordinate*. Such are primitive traps, quartz, flinty slate, gypsum, etc. The Primitive rocks contain no "petrifaction" and no fragments of other rocks; they must, therefore, have been formed antecedently to all others. The Transition formation consists likewise of several different kinds of rock, such as greywacke, trap, transition limestone, etc. These rocks also contain fossils, but the animals belong only to the lower orders in the scale, and are of species which no longer exist. The knowledge of the Floetz formation is more limited, and it is found in level countries where all the strata are for the most part below the surface. The rocks of this formation are the old red sandstone, limestone, gypsum, trap, coal, basalt, greenstone, etc. The *Alluvial* formation consists of loose soil, gravel, sand, moss, etc. The *Volcanic* formation comprises the lavas, ashes, etc., ejected from volcanoes.

This imperfect sketch of the Wernerian system of geology will serve to indicate the most notable differences between it and a rival theory which was originated in Britain by James Hutton (1726—1797). Hutton was born in Edinburgh, and was educated for the medical profession, but after having taken his degree at Leyden, he returned to Edinburgh, and resolved to live upon the means which he had inherited from his father in order that he might devote himself to the study of nature. He was a man of great industry, and he traversed many parts of England and Holland in order to study the geological phenomena. His views were first published in a paper contained in the "Edinburgh Philosophical Transactions" in 1788, but afterwards, 1795, they were more fully developed in a separate treatise. This was an epoch-forming treatise, for it was the first in which geological changes were referred to causes whose nature and mode of action are identical with those still operating.

The diagram in Fig. 186 will serve to exemplify the Wernerian views of the structure of the earth which were prevalent in England up to the time of the publication of Hutton's paper in the "Edinburgh Philosophical Transactions," and before the appearance of his book in 1795. The lowest known stratum of the earth was believed to be granite, and the highest mountains in the world were supposed to be composed of it, or at least the great central nucleus of their principal chains. Porphyry, trap, whinstone, slate, and basalt were all held to be varieties of granite, all being supposed to have been volcanic productions that have been exposed to different degrees of heat. It was admitted that the greater part of the stratified rocks had been deposited as sediment from water. Speculations dealing with the remotest condition of our planet and of our system supposed that our sun might have itself been originally a planet revolving about some still greater luminary which has long since dissipated its heat and light, but about which our system may still revolve. The earth, it was suggested by some of the more speculative geologists, was probably projected from the sun as a semifluid mass, which by cooling became superficially solid, at least, and at length its temperature so far decreased that many substances, at first existing only as vapour, were condensed into the liquid or solid form. Thus the watery vapour condensing into water formed the ocean.



Region supposed to be occupied by a mass kept in a semi-fluid state by internal heat, and containing in this condition granite, gneiss, porphyry, whinstone, basalt, etc.

FIG. 186.

Granite, basalt, etc., were the original matter of the earth, or have since been formed by subterraneous action. On the granitic nucleus which had become covered with the ocean were found beds of limestone, marble, and chalk from the *exuviæ* of marine animals. The globe was then rent by the violence of the confined central fires, islands and continents were heaved up, consisting in some parts of granite, in others of lava or molten granite, in others of stratified limestone, and great valleys and depressions were formed, into which the ocean retreated. On some parts of these continents and islands morasses were formed by the accumulation of vegetable matter, and from these morasses, heated by fermentation, were produced clay, marl, coal, etc. During the elevation of the mountain masses many deep fissures were produced, and in these many of the metals and minerals were deposited, partly from materials descending from above, and partly from vapours raised from below by the subterranean fires. By rain and snow precipitated on the summit of the new mountains, still glowing with heat, their materials were cracked into innumerable fragments, which descended into the valleys, and were there rolled about in the vast rivers, and so formed the great beds of gravel. Subsequent earthquakes and convulsions at various periods produced local derangements, so that the gravel which was once in the beds of the rivers was in some cases raised to the tops of the mountains. The coal deposit was washed down into the valleys and sea from the high masses, and became covered with clay or gravel, or limestone deposit of marine shells, etc.

The present structure of our planet is, according to Hutton, the remains of a former world; our present continents are formed out of the ruins of former continents washed down to the bottom of the ocean, there compacted, and again upheaved after the lapse of ages; sometimes changed by subterranean heat, or fractured, or bent and contorted. The forces which reduced the materials of the ancient continents to the condition of mere sediment subsiding to the bottom of the ocean are still at work, destroying even the hardest of the existing rocks, transporting their *débris* to the sea, and spreading it out on the floor of the deep, to form new strata like those of former times.

Hutton was convinced that basalt and many other kinds of *trap-rock* had been originally ejected from below, and had penetrated fissures of the sedimentary rocks, and he succeeded in discovering ocular demonstrations of the truth of this surmise, by finding in Glen Tilt, in 1785, *veins of red granite diverging from the principal mass and intersecting the stratified rocks.* What now became of the doctrines of Werner and of preceding geologists, who had declared that granite was the oldest of all the rocks? Glen Tilt showed that so far from granite having been the primordial substance or nucleus of our planet, it was more recent than the strata derived from still older rocks; and might not these be themselves the results of a cycle of changes like that to which our present rocks are traceable, and so on *ad infinitum?* Again, may not granite itself, now proved less ancient than the strata containing organic remains, be simply the result of the fusion of some rock originally sedimentary, but lying the deepest in the series, and fused by the intensity of the internal heat?

According to Hutton's theory, the cycles of destruction by aqueous agencies, rearrangement, consolidation, and upheaval by igneous actions, have continued and will continue indefinitely. His system implied a duration of earth through a period of time, vast indeed compared with the six thousand years or thereabouts which had commonly been supposed to have elapsed since the universe came into being as we now see it, after its gradual formation had occupied the short period of time represented by one hundred and forty-four hours in our present mode of computing time. "In the economy of the world, I can find no traces of a beginning, no prospect of an end," said Hutton, reading the testimony of the rocks to the best of his ability. Theologians took the alarm, and declared that it was Hutton's object to invalidate revealed truth by reviving the doctrines of some pagan philosophers concerning an eternal succession. Dr. Playfair, who illustrated and explained Hutton's doctrines, remarks on the attacks which had been made upon Hutton on other than scientific grounds, and particularly with regard to this passage, that, "In the planetary motions, where geometry has carried the eye so far, both into the future and the past, we discover no mark either of the commencement or termination of the present order. It is indeed unreasonable to suppose that such marks should anywhere exist. The Author of Nature has not given to the universe laws which carry in themselves the elements of their own destruction, like the institutions of men. He has not permitted in His works any symptom of infancy or of old age, or any sign by which we may estimate either their future or their past duration. He may put an end, as He no doubt gave a beginning, to the present system at some determinate period of time; but we may rest assured that this great catastrophe will not be brought about by the laws now existing, and that it is not indicated by anything which we perceive." Hutton and other geologists were for many years subjected to the charge of wilful perversion or blind misconception of the facts of nature, in the supposed interests of infidelity and atheism. The persons who made these charges imagined that all true religion was bound up with the acceptance of the Mosaic cosmogony in the precise sense in which they themselves had been accustomed to accept it. There can be no doubt that the progress of geological science in the eighteenth and in the earlier part of the nineteenth century was greatly impeded by the prejudices entertained by those who believed that the Bible was intended to teach physical science.

It would be unnecessary to describe here the reproaches and denunciations which were hurled at the unfortunate geologists. After a time we find some of the most eminent cultivators of geology among English beneficed clergymen, of whom Dean Conybeare and Dean Buckland may be named as examples; and we find also that so far from the Mosaic record being, as formerly supposed, inconsistent with geological theories, a score of different writers have published books in which geology and Genesis are reconciled in as many different ways. After a time we find also that the age of the earth, a point on which much angry disputation was raised, had come to be generally acknowledged as indisputably greater than the cosmically insignificant period of six thousand years; and at the present day the great antiquity of our planet may be considered as generally accepted by all who would admit the kindred doctrine of its revolution round the sun.

The theories of the earth advanced by Hutton and Werner respectively divided geologists themselves into two opposed camps, and a great and memorable contention arose as to whether fire or water was the principal agent in producing the actual condition of the earth.

As a matter of fact both these agents are still at work, modifying the condition of the earth. In the eruption of volcanoes (*See* Plate X.) the changes produced by igneous agency are too obvious to be overlooked. The action of water is in general slow and imperceptible, but it is constant. For example, a spring quietly issuing from a mountain's side, and flowing gently downwards with crystal clearness (*See* Plate XI.), would appear anything but a destructive agent; and yet the water may have silently removed vast quantities of limestone, and hollowed huge caverns within the mountain.

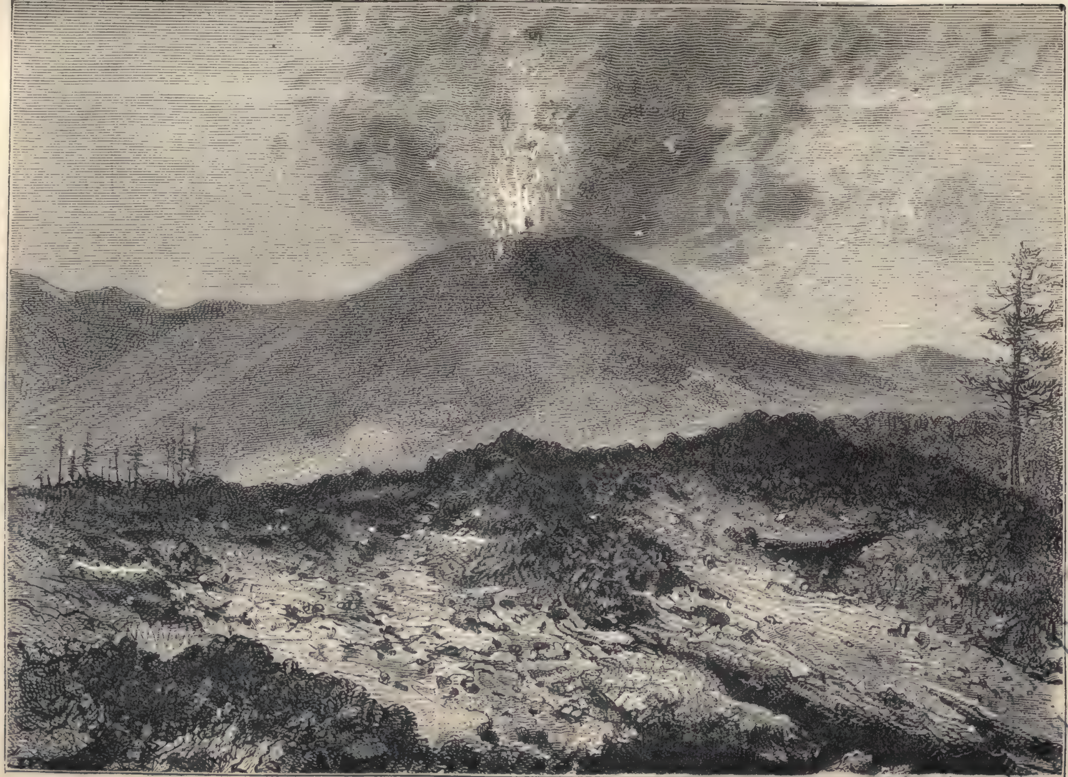


PLATE X.—A VOLCANO IN ERUPTION.

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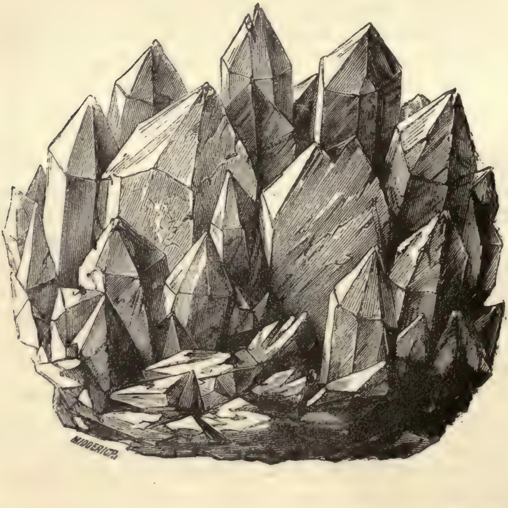
The contending geologists, or the Vulcanists and Neptunists, as the respective sects were called, soon came to occupy themselves more with framing ingenious arguments for attack and defence of their theories than with discovering and comparing actual facts. A new school emerged from this dispute, whose disciples resolved to devote themselves to assiduous observation and a far more extensive accumulation of facts, instead of framing theories. The mere accumulation of facts unrelated by general principles could not, however, as we have already had occasion to remark, ever constitute a science. By a reaction from the former immature speculations, geologists in the earlier part of the present century certainly did desist from framing systems, and devoted themselves to the patient multiplication of observations. This conduct had the great advantage of furnishing more solid data, and of disarming the hostility which the bold speculations of Hutton and others had raised against the infant science. The establishment of palæontology as a distinct science, and the foundation of the Geological Society of London, both of which date from the beginning of the present century, may be considered as the commencement of a new epoch in geological science, and will engage our attention in a subsequent chapter.

The student of the mineral kingdom is conversant with objects which possess an interest and beauty of which the uninitiated and the inattentive are little aware. Let any person, who has been accustomed to hurry through a museum with merely a passing glance at the cases of mineral specimens, examine these specimens in detail, and he will not fail to be struck with the variety and beauty of the colours, forms, and conditions of surface which minerals can display. The apparently inexhaustible diversity of the specimens, and the gorgeous or elegant beauty of many of them—so different from anything that might be anticipated from ordinary observation of the mass of materials constituting our planet—will astonish the intelligent beholder. He will see the most splendid tints of birds and flowers rivalled or excelled; he will see iridescent plays of colours in every key, from fiery coruscation to the delicate dreaminess of the opal; he will see the whole gamut of colours, hues, and tints, in conditions ranging from the most perfect lucidity to the deepest opacity: the ruby and the emerald, the sapphire and the topaz, will be included in the objects of his contemplation; strange phenomena of refracted light and electrical polarity will display themselves; every quality of texture, from fibres soft and delicate as silk to the hardness and compactness of adamant, will come under observation. Strangest of all, in minerals may be seen Nature's geometry of straight lines and planes, which are found in no other of her productions than in crystals.

Mineralogy, as a science of classification, relies upon discrimination of chemical constitution, crystalline forms, physical properties,—such as hardness, specific gravity, colour, lustre, optical properties, etc. The classification of minerals has never attained to the same perfection as those of animals and vegetables. They exhibit every shade of gradation in almost all of their properties, and, unlike animals and plants, the species are connected by none of those relations of affinity or descent to which the much-sought-for *natural systems* would in reality be the clue. The system of classification of minerals proposed by Linnæus was much inferior to those of the same author in the two other kingdoms of nature. Werner used the external character of minerals for the determination of his species, and he endeavoured to assign certain stratagraphical relations among certain divisions. It has since been found impossible to rely upon physical properties alone, and indeed these have been made subordinate to distinctions founded on crystalline form and chemical composition. The Abbé Haüy (1745—1822) will be remembered as the first to methodize crystallography, or the study of the crystalline forms of minerals. Haüy discovered a beautiful simplicity, symmetry, and constancy concealed beneath, apparently irregularity and complexity in crystals. Measurements of the angles of crystals by instruments in some cases reveal differences inappreciable by the unassisted eye, and these differences are known to be related to differences of chemical composition.



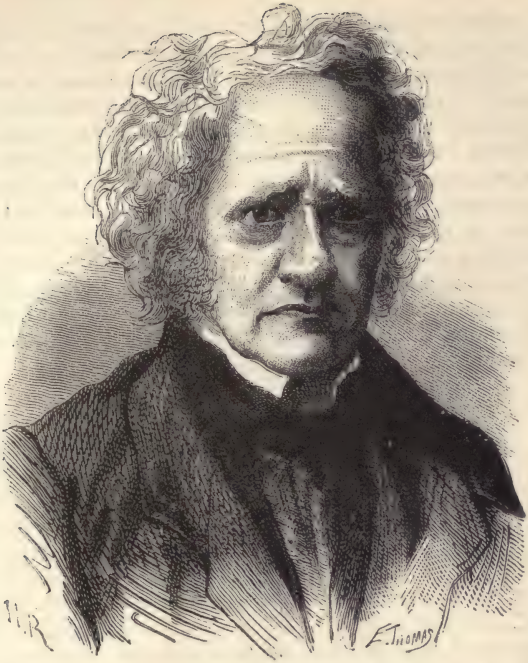


FIG. 187.—SIR JOHN HERSCHEL.

# CHAPTER XVI.

## ASTRONOMY OF THE NINETEENTH CENTURY.

THE discovery by Galileo of the satellites of Jupiter added to the number of the planetary bodies which had been known from time immemorial, and besides which the existence of no others had been suspected. True, the "Medicean stars" were only secondary bodies, but their discovery dissipated for ever the idea that the heavenly bodies *must be seven* in number, namely, the sun, the moon, Mercury, Venus, Mars, Jupiter, and Saturn. No doubt much patient watching of the heavens must have been practised before even these anciently-known planets were distinguished. But it would seem that the planet which Sir W. Herschel added to our system in 1781 (page 260) had escaped observation, although it is just visible under favourable conditions. It is not without probability that the explanation of the *eighth* planet of the old Burmese astronomers is ascribed to the planet Uranus. A few years before the discovery of Uranus, Professor Bode of Berlin published a notice of a singular numerical relation among the mean distances of the planets from the sun. This relation is known by the name of *Bode's Law*, and may be expressed in several ways, as, for example, if we write down the numbers 3, 6, 12, 24, 48, 96 (each of which is double its predecessor in the series), and add four to each number, or, what amounts to the same thing, if we take the formula $3\times2^n+4$, and calculate its values when the index has the respective values 0, 1, 2, 3, 4, and 5, we obtain the numbers 7, 10, 16, 28, 52, 100. Now these numbers, leaving out the fourth, are nearly proportional to the distances of Venus, the earth, Mars, Jupiter, and Saturn respectively from the sun, as may be seen from the following table, in which are included three planets unknown when Bode drew attention to this relation :

| Names of Planets. | True relative distances from Sun. Earth's distance=10. | Relative distances from Sun according to Bode's Law. |
|---|---|---|
| Venus . . . | 7·23 | 7·00 |
| Earth . . . | 10·00 | 10·00 |
| Mars . . . | 15·23 | 16·00 |
| *Ceres* . . . | 27·66 | 28·00 |
| Jupiter . . . | 52·03 | 52·00 |
| Saturn . . . | 95·39 | 100·00 |
| *Uranus* . . . | 191·82 | 196·00 |
| *Neptune* . . . | 300·37 | 388·00 |

When Uranus was discovered, its distance from the sun was held to conform to Bode's law; but the deficiency of a planet between Mars and Jupiter constituted a gap in the series which Bode conjectured would one day be filled up by the discovery of an intermediate planet. Some German astronomers were confident enough to calculate from analogy what would be the elements of the missing planet, and it is said that these surmises guided subsequent observations to some extent at least. PIAZZI (1746—1826), Astronomer Royal at Palermo, being occupied with a revision of the heavens for the purpose of compiling a new and more complete catalogue of fixed stars, observed on the 1st of January, 1801, in the constellation Taurus, a small star which, having changed its position in the course of a few days, he supposed to be a comet. As such he in fact drew the attention of other astronomers to it; but it was soon found that the orbit of the new body was nearly circular, and that it was in reality a planet, whose elements resembled closely those calculated for the imaginary planet which Bode's law required. This coincidence, after that afforded by Uranus, made this curious relation announced by Bode deserving of the attention of astronomers, although the law is entirely empirical, and not referable to any known cause. The German astronomers were delighted at a discovery which confirmed so strikingly the conjectures of their countryman, and the articles in their journals announcing the discovery of *Ceres* were headed by such titles as "The long-expected Planet between Mars and Jupiter."

The new planet *Ceres* being supposed to complete our system, there was no reason to look for more planets. Yet on the 28th of March, 1802, Dr. Olbers of Bremen noticed a small star, which he soon discovered was not a fixed star; but he found it impossible to make its observed positions agree with the supposition of a circular orbit, and its appearance was unlike that of a comet. After observing it for three weeks, Gauss undertook to determine the orbit without making any preliminary hypothesis, and he found that the new body was a planet (Pallas), having an elliptical orbit, very close to that of Ceres, but distinguished by a great degree of eccentricity, and inclined to the plane of the ecliptic at no less an angle than 34°. So struck was Olbers with the nearness with which the orbit of Ceres and of Pallas approached each other at the intersection of their planes, that he started the lucky hypothesis that these planets were fragments of a larger one which had been shattered by some internal convulsion, or by collision with some other body. Two consequences were immediately deducible from this hypothesis, namely, that other fragments probably existed, and if so, their orbits must pass near the points in which the orbits of Ceres and Pallas intersect. The parts of the heavens corresponding to the two intersections were accordingly closely observed by the German astronomers. On the 2nd of September, 1804, Harding discovered at Lilienthal a third planet, which has received the name of Juno. The elements of this planet agreed very fairly with Olbers' hypothesis, if allowance be made for the perturbations caused by Jupiter. Olbers himself followed out his idea by every month carefully scrutinizing certain parts of the heavens; and after having carried on these observations for three years, his perseverance was rewarded by the discovery of Vesta on the 29th of March, 1807. This planet is similar in all essential particulars to the three previously discovered. The same system of examination was long kept up, and Olbers has declared that so regularly did he examine the parts of the heavens already referred to, he was certain that no new planet had passed over them between 1808 and 1816.

After an interval of thirty-eight years, a fifth member of the asteroid group was discovered by Hencke at Driesen on December 8th, 1845; a sixth by the same observer in 1847; and two more in the same year by Hind in London. Since 1847 further additions have every year been made to the planets between Mars and Jupiter. By the end of the year 1865, eighty-five asteroids had been observed and their elements computed. The whole number now known (1880) exceeds two hundred. These planets have been discerned by many different observers in various parts of the world, in England, Italy, France, the United States, Germany, and Denmark; but the elements have in most cases been calculated by Germans. It will of course be unnecessary to give here particulars of the several members of this numerous group, but the following remarks may have some interest. Their mean distances from the sun are from 200,000,000 to more than 300,000,000 of miles, and their orbits have various degrees of inclinations up to that of Pallas, which is inclined to the elliptic nearly 35°, while No. 33 (Polyhymnia) has an orbit the longer axis of which is to the shorter as 100 is to 94. The largest of the group is Pallas, with a diameter of 670 miles, while others have been calculated to be less than 20 miles in diameter. Vesta may be seen by the naked eye as a star of the sixth magnitude, and Ceres has also been seen under favourable circumstances without optical aid. The hypothesis of Olbers is considered to give a satisfactory explanation of the relation between these minor planets, whose orbits are so entangled, that if they were represented by material rings, it would be possible to lift them all up by taking hold of any one of them.

At first the members of the asteroid group discovered after Ceres, Pallas, Juno, and Vesta, received only names taken as usual from classical mythology, such as Astræa, Hebe, Iris, Flora, etc.; but when discoveries began rapidly to succeed each other, it was found desirable to distinguish each new planet of the group by a number as well as a name. The cause of the many discoveries of these small planets in recent times is not so much improvements in telescopes as the increased skill and systematic diligence of observers, and especially the greater detail of the more recent star-maps, in which stars of very low magnitudes are set down. By the aid of these maps a moving point is readily distinguished from the neighbouring fixed stars, and its path followed. It is probable that these miniature planets exist in indefinite numbers, and that many more await discovery. At present hardly a month passes in which the astronomical journals have not to record at least one addition to the asteroid group.

Encke, on attempting to determine the orbit of a comet which had been observed by Pons at Marseilles at the end of the year 1818, found that the observations could not be represented by any other than an elliptical orbit, in which the comet had a period of about $3\frac{1}{3}$ years. It was soon noticed that this comet was similar to one which had appeared in 1805, with regard to which Bessel had remarked that a parabolic orbit could not represent the observations, and it was found on investigation that a series of observations extending over thirty-three years showed ten returns of the same comet. Encke correctly predicted the return of the comet in 1822, when it was observed at Paramatta. Encke observed that the period of this comet appeared to be diminishing as it seemed to return to the point of its orbit nearest the sun (perihelion) $2\frac{1}{2}$ hours earlier at each revolution. The orbit of the comet lies entirely within the orbit of Jupiter, and nearly in the same plane. The perturbations to which the motion of the comet is liable are, from the enormous mass of Jupiter, excessive. Between 1819 and 1822 the perihelion passage was thus retarded for nine days. When all allowance had been made for the perturbations produced by the planets, Encke found that the diminution of the periodic time from 1,213 in 1789 to 1,211 days in 1838 remained unaccounted for, and he conjectured that it might be due to the resistance of some medium pervading space, and too attentuated to affect the movements of a planet, but capable of retarding the motion of a body composed of material so thin as that of which there is reason to believe a comet is constituted. The announcement of a comet revolving regularly in a short period was a discovery which excited very great interest among astronomers, and each return of Encke's comet has been regularly observed.

Another periodical comet of much interest is named after BIELA, a Bohemian military officer, who first observed it on February the 27th, 1826. After eight weeks' observation it was found that the orbit was elliptical, and that the comet was identical with one observed in 1772 and again in 1805. Its period was calculated to be 2,455 days. In the year 1828 Dr. Olbers announced that in 1832 this comet would be within 20,000 miles of the orbit of the earth, and this announcement was the cause of no small degree of popular alarm; for people in general were not sufficiently scientific to consider that the earth's orbit was a different thing from the earth itself, and it was feared that between our planet and the filmy comet there might occur a collision which would be disastrous to the former. Some persons, who thought themselves more foreseeing than their neighbours, sold their lands and houses, perceiving that property of this kind would certainly become much depreciated in value as the day approached on which the whole planet was expected to be shattered into ruin. They evidently considered that on that last day portable possessions would be more advantageous. In order to allay in some degree the alarm of the multitude, some astronomers endeavoured to explain to them that though the comet would on a certain day be not more than 20,000 miles distant from a particular part of the earth's *orbit*, the earth itself would not reach that point of her orbit until a month after the dreaded comet had passed, and that the two bodies would at no time be nearer to each other than the orbit of Mercury is to that of the earth. Biela's comet escaped observation at its next return to perihelion, on account of its then occupying a part of the heavens near the sun. At the end of 1845 it was seen again nearly at the calculated time, but with a strange difference in its configuration, for it had divided into two distinct comets which pursued their course together. The space by which the two parts of the comet were separated was calculated to be 157,000 miles; but it was subject to variation. In 1852, when the two comets returned, the space between them was found to have increased to 1,250,000 miles. The next perihelion passage of these comets would occur in 1859, but in a position unfavourable for observation. They were not seen in 1859, and although they have been expected and sought for at the periodic times, viz., in 1866 and 1872, they have not been seen.

The times of several other comets of short periods have also been determined. Their orbits, like those of Encke's and Biela's comets, all lie within the orbit of Jupiter, and they are liable to great perturbations caused by the attraction of that huge planet. The periods of a few other comets, varying from sixty-seven to seventy-seven years, have also been determined. The best known of these is the comet named after Halley, because this eminent astronomer, having made elaborate calculations from the records of remarkable comets which had appeared from time to time, with a view of discovering whether the comet he himself observed in 1682 had previously appeared in the same path, thought probably that it and certain comets seen in 1531 and 1607 were the same body. There were, however, discrepancies in the circumstances of the successive periods not being equal, and the inclination of the comet's orbit to the ecliptic being somewhat different in each case. Halley attributed these changes to the perturbing action of Jupiter, and by calculation he found that between 1607 and 1682 his comet must have passed so near to the planet that its motion would be accelerated and its period shortened. He predicted its return in 1758. The last return of Halley's comet was in 1835, when it passed through perihelion within four days of the time which had been assigned. The orbit of this comet was studied by Damoiseau in 1817, and by Pontécoulant in 1829. Records of its successive appearances have been recognized by Hind in the accounts of great comets from B.C. 11 downwards. The orbits of a great number of other comets have been calculated from the records of observations, but with little of interest in the results. It may here be mentioned that the method of deducing the most probable true value from a number of observations liable to be affected by errors, called the method of *least squares*, was first applied to calculations for comets, and has always since been in use for such calculations.

Several remarkable comets have been observed in the present century. In the autumn of 1811 a great comet was conspicuous in the heavens. Its tail was 25° in length, and about 6° in breadth. Its orbit was calculated by a German astronomer, and was pronounced to be elliptical, and to the comet the period of revolution was assigned of 3,065 years. A very brilliant comet, with a tail 40° in length, was seen in February and March, 1843. The orbit of this comet had an unusually small perihelion distance, for it approached to within 538,000 miles of the sun; and the comet swept through the part of its course near the sun with inconceivable velocity. In one day it passed through an arc which, measured from the sun, would include 290° of the whole circle of 360°. The year 1858 was distinguished by the appearance of a small comet with an intensely bright nucleus, which has a calculated period of about 2,000 years. It was first observed by Dr. G. B. Donati at Florence, and it is known as Donati's Comet. The great comet of 1861 was first discovered by an amateur astronomer in New South Wales on the 13th of May. It formed a splendid object in the summer evening skies. Its tail at one time was more than 72° in length.

The history of science can hardly boast of a more brilliant discovery than that which added a new planet to our system in 1845. The honour of the discovery belongs equally to two men, both young at the time,—the one a Frenchman, the other an Englishman. This discovery was the consequent of another, which has been already mentioned, namely, that of Uranus by Sir W. Herschel (page 260). The motion of Uranus was calculated by Delambre in 1790, but observation soon showed that the tables of the planet's positions which he drew up were inexact to such an extent that it became necessary to recalculate them upon better data. This was undertaken by a French astronomer named Bouvard. He based his investigations only on the recent observations of Uranus, rejecting those which were recorded previous to 1781, when it had been seen and registered as a star. He found it, in fact, impossible to reconcile these different observations with any assignable orbit. In 1821 he produced tables which for a short time fairly represented the motion of the planet. But Uranus soon began to depart from the course which had been traced out for him; and his irregularities increased at length so much, that astronomers despaired of representing his movements by any simple formula. Many hypotheses were put forward to explain the discrepancies between theory and observation. One suggested that the ether, or some such interstellar medium, was retarding the planet's motion; another, that possibly Uranus might be yoked to some unwieldy but hitherto unobserved satellite; another, that possibly at so remote a distance from the sun the law of gravitation might be in some

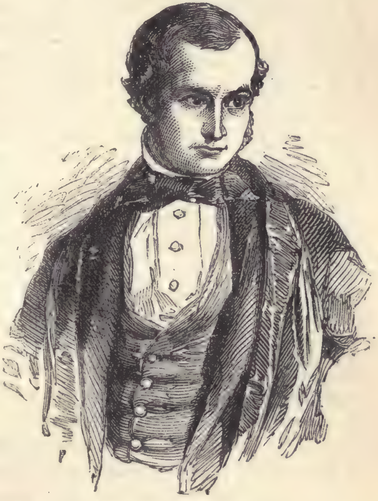


FIG. 188.—JOHN C. ADAMS.

degree modified; another, that some comet might have interfered with the regular movement of the planet; and so on. Bouvard himself was inclined to believe that an exterior planet was the sole cause of the irregularities of the motion of Uranus, and some other astronomers of eminence shared this conviction. The question was in this state when, in 1841, MR. JOHN COUCH ADAMS, then a student of St. John's College, Cambridge, resolved to investigate the question; but it was not until 1843, after he had taken his degree, that he found leisure to carry this resolution into effect. He worked at the problem for nearly two years, adopting the hypothesis of an exterior planet, and in October, 1845, he sent to Mr. Airy, the Astronomer Royal, the provisional elements of a planet revolving round the sun in an orbit outside of that of Uranus, such as would account for the perturbations of Uranus. Adams' results were unfortunately not published at the time; and although he had indicated in a communication to the astronomers of the Royal Observatory at Greenwich the approximate position of the planet, with the hope that it would be sought for, it appears that this was not done until the announcement of the results arrived at by the independent calculations of the Frenchman LE VERRIER. It was in 1845 that Le Verrier began his calculations, and on the 1st of June, 1846, he was able to announce positively to the Academy of Sciences that an exterior planet exercising a perturbing action on Uranus actually existed, and that on the 1st of January, 1847, its longitude would be 325°, without the possibility, in assigning this position, of an error greater than 10°. But in order that this planet might be observed, it was necessary to define its position within narrower limits, and Le Verrier accordingly undertook this further determination. In three months he accomplished a most laborious calculation, and on the 31st of August, 1846, he presented to the Academy a second paper, in which he gave the approximate values of the elements of the as yet hypothetical planet. He fixed its longitude more definitely, and stated that it must be sought for at so many degrees from a certain fixed star. On the 18th of September, 1846, Le Verrier communicated the result of his latest calculations to the astronomers of the Berlin Observatory. The letter reached its destination on the 23rd of September, and by a curious coincidence Galle, the Berlin astronomer, had been examining a new and very complete map of the stars of the very region of the sky in which Le Verrier had indicated. In the evening the telescope was directed towards the position named by Le Verrier, and there was at once seen a small star, of an aspect different from the surrounding stars, and not shown in the map. The next evening the position of the star was found to have changed precisely as the theory of the French astronomer required. Information of these observations was at once sent to Le Verrier, who on the 5th of October had the gratification of announcing to the French Academy of Sciences the complete verification of his conclusions.

A controversy arose between the English and French astronomers as to the right of Adams to participate in the honour of this memorable discovery. It will be unprofitable to reproduce here any of the contentions on either side. Suffice it to say that Le Verrier's results were indisputably first made public, but Airy clearly proved that Adams had completed his calculations eight months before Le Verrier had announced his results to the French Academy. It was only when Airy perused the memoir of Le Verrier's communication that he recognized the importance of Adams' communication by the coincidence between his results and those of the French astronomer. Airy immediately requested Challis of Cambridge to search for the planet in the region indicated. Challis had not the advantage of the possession of the Berlin map of the stars for this region, but commenced a systematic search on July 11th, 1846. It was, however, not until the 29th of September that the planetary nature of an object which had been observed as a star on the 4th and 12th of August was made out. The British astronomers were therefore a few days later than the German in recognizing the new planet; upon which it was agreed, after some discussion, to confer the name of Neptune.

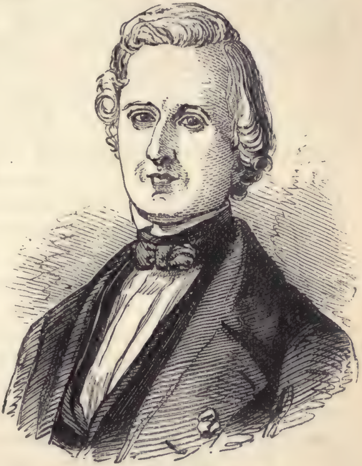


FIG. 189.—LE VERRIER.

The data for the deduction were deviations between the observed and calculated places of Uranus, which scarcely exceeded one minute of a degree. Bode's law (page 418) would serve to fix at least approximately the distance of the supposed planet from the sun; but the estimation of the mass and position of the unknown planet from the perturbations of Uranus requires methods of profound mathematical analysis and laborious calculations, into the details of which it would here be impossible to enter. It will readily be understood that the elements of Neptune, as determined from actual observations, could not be expected to conform exactly to those calculated *à priori* by Adams and Le Verrier. But the observed position of the planet when discovered did not differ 2½° from that indicated by Adams, while Le Verrier's determination was within 1° of the truth.

The diagram, Fig, 190, shows the position of Uranus and of Neptune in their orbits at certain epochs between the time of the discovery of Uranus and that of Neptune. The dotted lines indicate the direction of the attractive forces between the planets; and it will readily be observed that, until the year 1822, the action of Neptune on Uranus accelerated the movement of that planet, and that after 1822 the action changed into a retarding one. Neptune has been found to have a diameter of about 36,000 miles, and to revolve round the sun in 164·6 years at a distance of about 2,746 millions of miles. This distance is notably less than that required by the so-called law of Bode in the proportion of 300 to 388, and it is by far the most marked deviation from it. On the 10th of October, 1846, Lassell discovered a satellite of Neptune.

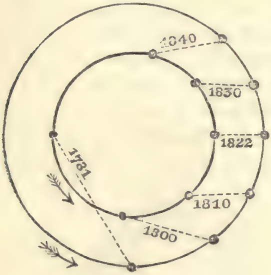


FIG. 190.

The orbit in which Neptune was actually found to revolve is, therefore, very much smaller than that which had been assigned by Le Verrier and Adams; and the other elements which had been hypothetically determined were consequently very different from the truth. There were in the problem four unknown quantities to be determined, but these might be modified in a variety of ways, so as to compensate and correct each other. Thus it happened that, the distance having been assumed too great, as it was afterwards found, the error was compensated by adopting a mass greater than the reality. This was possible, because the observations of Uranus only embraced a few years before and after the conjunctions of the planets. Had observations of Uranus over a very extended space of time been accessible, the inadequacy of the false assumptions to explain the whole of the phenomena would have been noticed, and a nearer approach to the truth obtained. After Neptune had been seen, and the elements deduced from observation, some astronomers affirmed that this was not the theoretical planet of Le Verrier and Adams; and that the agreement of Neptune's position, when discovered, with that of the hypothetical planet, was only a happy coincidence. The final conclusion is that Adams and Le Verrier did at about the same time investigate the irregularities of Uranus, on the supposition that they were due to an exterior planet, and that independently of each other they arrived at a very approximate determination of the *position* of the perturbing planet. Each is, therefore, entitled to be considered the independent discoverer of Neptune.

Lassell discovered also a satellite of Uranus revolving at a distance nearer to the planet than the innermost of the six observed by Sir W. Herschel. The number of Uranian satellites was again added to in 1847, when O. Struve discovered one which is the eighth in the order of discovery, and takes the second place in order of distance from its primary. On September 16th, 1847, an additional satellite to Saturn, the eighth in order of discovery, was seen in America by W. Bond; and, independently, by Lassell two days afterwards in England, who recognized it as a Saturnian satellite on the following day. It is one of the smallest in the Saturnian system, and its place in order from the planet is the seventh.

The above-named are the principal discoveries which have added new bodies to the solar system during the past portion of the nineteenth century. Many important investigations of the theories of various planets and of their secondary systems have been undertaken. Elaborate tables, by which the position of each planet may be found for any given epoch of time, have been calculated by several astronomers, who have introduced successively more and more corrections and refinements. Examples of such tables are Lindenau's "Tables of Mars," published 1811; his "Tables of Mercury," 1813; Delambre's "Tables of Jupiter's Satellites," 1817; Bouvard's "Tables of Jupiter, Saturn, and Uranus," 1821; Damoiseau's "Lunar Tables;" Lubbock's "Theory of the Moon," 1834 and 1838; Damoiseau's "Tables of Jupiter's Satellites," 1836; Hausen's "Lunar Tables," 1857.

Astronomers have industriously laboured at forming catalogues of the fixed stars, in which the position of each star shall be registered with the greatest accuracy. As the value and importance of such registers will hardly be understood by those who have given no special attention to astronomy, the words of a late eminent astronomer may be quoted: "Every well-determined star from the moment its place is registered, becomes to the astronomer, the geographer, the navigator, the surveyor, a point of departure which can never deceive or fail him; the same for ever and in all places; of a delicacy so extreme as to be a test for every instrument yet invented, yet adapted for the most ordinary purposes; as available for regulating a town clock as for conducting an army." The formation of star catalogues dates from at least the time of Hipparchus, and such catalogues will ultimately be the means of revealing changes in the stars themselves of which we have as yet scarcely any conception. Though the fixed stars serve admirably for what we may metaphorically call the landmarks of the heavens, their fixity is, after all, but relative; and other purposes which will be served by star catalogues are the determination of the motions which belong to each star (*i.e.*, its proper motion); the change in the apparent places of the stars due to the motion of the solar system itself; and various physical changes in the heavens themselves, such as the disappearance of stars and apparition of new ones. The amount of labour devoted by the astronomers of the present century in observations on the fixed stars may be inferred from the following tabular view of some of the star catalogues, which have been compiled during the present century and published in England and elsewhere.

| Year of publication. | By whom compiled. | Number of Stars in the catalogue. | Description of Stars. |
|---|---|---|---|
| 1800 | Rev. Francis Wollaston | —— | Circumpolar stars. |
| 1803 | Piazzi | 6,748 | Stars. |
| 1806 | De Zach | 1,830 | Zodiacal stars. |
| 1807 | Piazzi | 120 | |
| 1814 | ,, | 7,646 | |
| 1824 | Sir J. Herschel and South | —— | Double stars. |
| 1827 | The Royal Ast. Soc. | 2,881 | Stars. |
| 1829 | Pond | 720 | |
| 1830 | Sir J. Herschel | 1,236 | Double stars. |
| 1831 | | 364 | ,, ,, |
| 1832 | | 2,017 | ,, ,, |
| 1834 | Davies | 121 | ,, ,, |
| 1835 | Sir T. M. Brisbane | 7,385 | |
| ,, | Sir J. Herschel | 286 | Double stars. |
| ,, | Johnson | 606 | Southern stars. |
| 1837 | W. Struve | 3,112 | |
| ,, | Hon. J. Wrottesley | 1,318 | |
| 1838 | Airy | 726 | |
| ,, | British Association | 9,766 | Southern stars (La Caille's catalogue). |
| 1840 | Santini | 1,677 | |
| 1842 | Pearson | 520 | |
| ,, | Greenwich Observatory | 1,439 | |
| 1844 | Taylor | 10,015 | |
| 1845 | British Association | 8,377 | |
| 1847 | ,, | 47,390 | Catalogue from Lalande's "Hist Celeste." |
| 1848 | | | Double stars. |
| ,, | Greenwich Observatory | 2,156 | |
| 1850 | Main | 877 | Catalogue of Proper Motions. |
| 1852 | W. Struve | 192 | Stars. |
| | | 2,682 | Double stars. |
| ,, | Rümker | 1,200 | |
| 1854 | Jacob | 1,440 | |
| 1855 | Greenwich Observatory | 1,576 | |
| ,, | Bond | 5,500 | Small stars near the equator. |
| 1857 | | —— | Circumpolar stars. |
| 1859 | Robinson | 5,345 | *Observed at Armagh.* |
| ,, | | —— | Berlin star charts, begun 1830, completed. |
| ,, | Argelander | 110,982 | Zones, Part I. |
| 1861 | | 105,075 | ,, ,, II. |
| 1862 | | 108,129 | ,, ,, III. |

The list given above introduces among other eminent names that of an illustrious English man of science, whose researches have advanced not only astronomy, but many branches of physics, and who is besides to be remembered as the author of the most valuable treatise we possess on the logic of the sciences, namely, "An Introductory Discourse on the Study of Natural Philosophy." A few brief notes of the scientific labours of its author, SIR JOHN HERSCHEL (1792—1871), may be not inappropriately placed before the reader in this place. John Frederick William Herschel, the only son of Sir William

Herschel, was born at Slough in 1792. His academical education was completed at St. John's College, Cambridge, and in 1813 he was Senior Wrangler and also Smith's Prizeman. His first scientific paper, published in 1816, was an account of observations of multiple stars; and in the period between 1823 and 1828 he published several catalogues, comprising nearly 1,000 such stars. Again, in 1830 he compiled catalogues of the positions of 1,500 stars. In 1834 he proceeded to the Cape of Good Hope, and remained there for four years for the purpose of studying the heavens of the Southern Hemisphere, himself defraying all the expenses. In 1836 the gold medal of the Royal Astronomical Society was awarded to him for his catalogue of nebulæ. At the same time that he was carrying on these astronomical labours, he engaged also in elaborate investigations in several departments of physical science, as the papers attest that from time to time appeared in the scientific journals. He was also the author of systematic treatises on Sound and on Light published in the "Encyclopædia Metropolitana," and these are esteemed as among the best treatises on the subjects. In 1830 appeared from his pen "A Preliminary Discourse on the Study of Natural Philosophy," which formed the first volume of Lardner's "Cabinet Cyclopædia." This work unites in an extraordinary degree elegance of style and clearness of exposition, with power of logic and range of thought. A small octavo of 360 pages, in every respect adapted for popular reading, it is yet the best introduction to the study of physical science that can be put into the hands of an intelligent student. "A Treatise on Astronomy" was also contributed to Lardner's "Cyclopædia" by Sir John Herschel, who wrote besides a larger work on the same subject entitled "The Outlines of Astronomy." These works and the more recent publication, "Familiar Lectures on Scientific Subjects," are admirable examples of scientific literature. Sir John Herschel was made Master of the Mint in 1850, and died full of years and honours in 1871.

The study of double stars, in which Sir William Herschel first aroused the interest of astronomers, received, as we have seen, much attention from Sir John Herschel. The number of nebulæ with which Halley was acquainted in 1714 was but six, but by the beginning of the present century the number had increased to several thousands. Sir W. Herschel furnished catalogues of no fewer than 2,500, which he had observed. These all belonged to the northern heavens, for at the beginning of the present century astronomers were acquainted with no other nebulæ of the southern hemisphere than those observed by Halley and Lacaille (page 261). In 1828 Mr. Dunlop communicated to the Royal Society a list of 629 nebulæ and star clusters which he had observed with a Newtonian reflector of 9 inches diameter. In 1833 Sir John Herschel gave the results of his observations of nebulæ in the northern heavens. These observations extended over eight years, and were made with a 20-feet reflector. His catalogue embraces

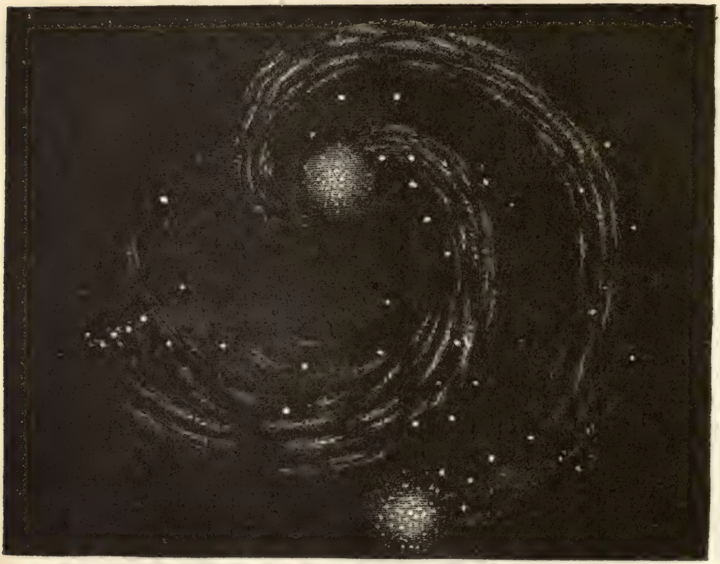


FIG. 191.—THE SPIRAL NEBULA.

2,306 objects. The results of the observations which the same eminent astronomer carried on at the Cape of Good Hope were published in 1847, and contained among other objects 1,708 nebulæ. The most valuable contributions that have since been made to our knowledge of nebulæ are due to Lord Rosse, whose observations have been made by the great 6-feet reflector, which will be described on a subsequent page. Many nebulæ, which in telescopes of less power presented a wholly cloudy appearance, were by Lord Rosse's powerful instrument resolved into close clusters of stars. Other nebulæ, not thus resolvable, nevertheless present with the 6-feet reflector a very different aspect to what they do with an 18-inch reflector. Nebulæ are almost the least luminous of celestial objects, and it will be readily understood that details of their structure, invisible with the 18-inch reflector, are revealed with the 6-feet instrument, which admits sixteen times as much light as the other. Some of the most interesting observations made by Lord Rosse are those of spiral nebulæ. One of the most remarkable of these is in the constellation *Comes Venatici.* It presented to Sir J. Herschel the appearance of a bright globular cluster surrounded by a ring, which he thought to be divided through about half its circumference, while near it was a small, bright, round nebula. Lord Rosse's telescope shows this object as a series of spirals of nebulous matter, and the smaller nebula near is found to be connected

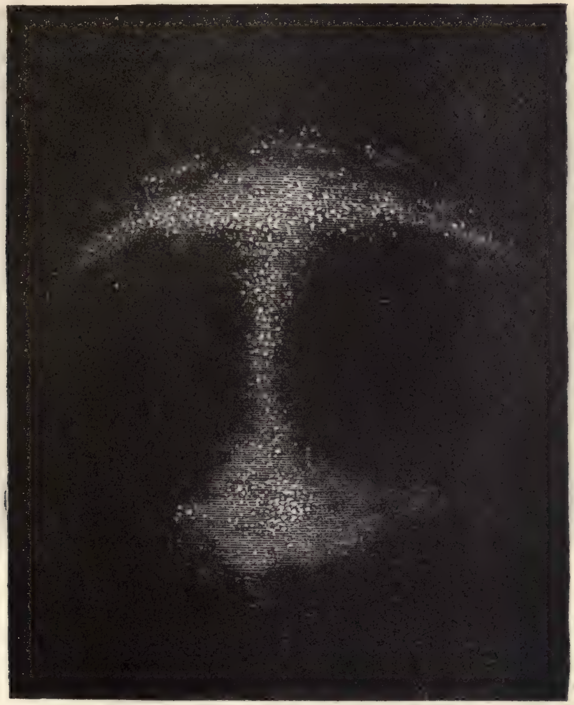


FIG. 192.—THE "DUMB-BELL" NEBULA.

with the larger, also by means of spiral convolutions. Fig. 191 will serve to give some idea of the aspect which this nebula presents as seen by Lord Rosse, who has also detected unmistakable indications of a spiral structure in at least fourteen nebulæ. In some other nebulæ, which had previously been described as possessing uniform round discs, Lord Rosse found distinct appearances of an annular arrangement. Fig. 192 represents the so-called "dumb-bell" nebula in the constellation *Vulpecula*, as it is figured by Lord Rosse. It usually happens that in very large telescopes the aspect of a nebula is different from that which it presents in smaller instruments. In this case, for instance, the name, which correctly enough expresses the form of the object as seen in an ordinary telescope, ceases to be appropriate to the appearance of the nebula when examined by the great reflector at Parsonstown. The nebula represented in Fig. 193 is known as the Great Nebula in *Orion*, and is one of the largest and most curious in its configuration. It was discovered by Huyghens in 1656, and was

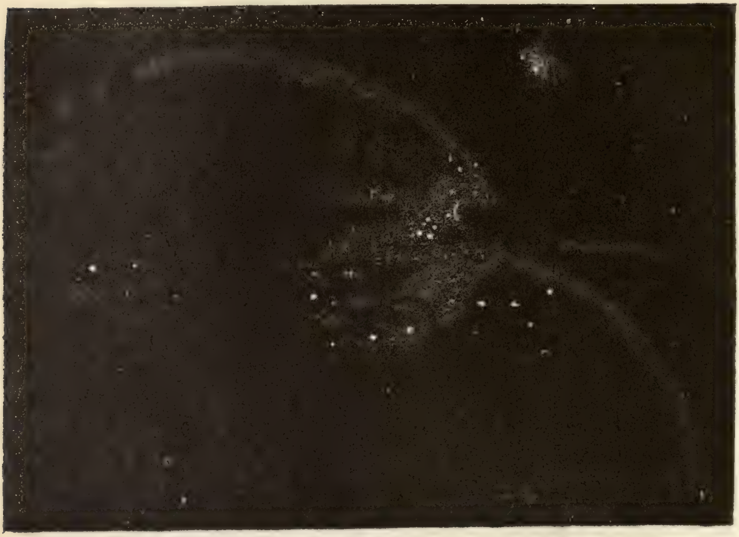


FIG. 193.—THE GREAT NEBULA IN ORION.

examined by the elder Herschel under his highest telescopic powers without its showing any appearance of being capable of resolution into stars. But Lord Rosse's reflector and the great refractor of the Cambridge (U.S.) Observatory have to a considerable extent resolved this nebula into a multitude of stars. The spiral arrangements of such nebulæ as that in *Comes Venatici* would appear to afford some support to the hypothesis of Laplace; but astronomers must scan these singular bodies with close attention for many ages before they can hope to recognize signs of such changes as the hypothesis requires. Granting, however, the truth of the hypothesis and a sufficiently long duration for the human race and human records, it is not too much to expect that these records may yet describe the phenomena which present themselves while a nebula is being transformed into a solar system.

The rapid progress of astronomy in modern times has been largely due to improvements of the instruments of observation and of measurement. The delicacy with which angular magnitudes can now be estimated would excite the astonishment of an ancient astronomer were one "to revisit the glimpses of the moon." The division of graduated instruments which had, in the previous century, undergone successive improvements in the hands of Sharp, Graham, Bird, and Ramsden, was carried to thegreatest pitch of perfection by Troughton, the celebrated instrument maker. EDWARD TROUGHTON (1753—

1835) was a native of Cumberland, and was originally brought up to farming, but at seventeen years of age he went to London, and under his brother John learned the optician's art. The brothers commenced in London a business which soon proved a success, and, upon John Troughton's death, Edward continued to carry it on for many years, and at length acquired a world-wide reputation as a maker of accurately-divided astronomical instruments. Troughton constructed several magnificent instruments for the Royal Observatory at Greenwich, among which may be mentioned a mural circle and a transit instrument with a 10-feet telescope. Among improvements in instruments may be named the method of measuring reflected angles devised by John Pound. This method depends upon the law of the equality of incidence and reflection, and was put into practice by observing the angular points of an object by direct vision, and also by reflection from the surface of a trough of mercury. The position of the bisection of the angle between the two directions is that of the horizon. In transits Pound found it necessary to have these observations made simultaneously with two different instruments. The present Astronomer Royal has devised a plan of obtaining the required observation with only one circle. Mr. Simms, the successor of Troughton, has constructed more recently for the Greenwich Observatory some fine instruments of large dimensions; but as a detailed description of gnomiometrical instruments would necessarily be of too intricate a nature to interest the general reader, we will pass on to mention some of the recent improvements of astronomical telescopes.

In producing specula of a very perfect figure, Mr. Lassell of Liverpool has especially distinguished himself. It was with a reflector of 2 feet diameter made by his own hands that he discovered the satellite of Neptune and the eighth satellite of Saturn, and made some important observations of the planets. But all other reflecting telescopes yet constructed have been surpassed by the gigantic instruments made by the Earl of Rosse at his seat, Birr Castle, at Parsonstown, about fifty miles from Dublin. In 1840 Lord Rosse completed a reflecting telescope having a speculum of 3 feet diameter, which had been ground to a true form by machinery which he had devised for that purpose. The focal length of this telescope is 26 feet. In 1845 Lord Rosse had finished and erected the largest telescope in the world. This has a speculum of 6 feet diameter, and 54 feet focal length. The speculum weighs 4 tons, and in its casting and mounting special and ingenious arrangements were required to overcome the difficulties entailed by its magnitude, and the nicety of the operations involved in shaping to a mathematically perfect parabolic curve a piece of metal weighing 4 tons, or seven times as much as the 4-feet speculum of Sir W. Herschel, may be inferred from the fact that between the spherical and parabolic figures the difference is so small, that if two surfaces having sections coincided at the centre of the 6-feet speculum, separation at the edge would not amount to one 10,000th part of an inch. Lord Rosse's great telescope is mounted between two piers 60 feet high. The speculum is mounted at one end of a wooden tube formed of staves, which are hooped with iron. This tube is about 50 feet long and 8 feet in diameter in the middle. The speculum, the tube, and the parts connected with them, weigh about 12 tons, and this heavy mass is so mounted and counterpoised that it can be moved with the greatest ease and steadiness. The telescope commands the southern sky from the horizon to the zenith, and a star on the equator can be followed for an hour. The observer is stationed in a gallery which is so arranged that it can be moved in altitude and azimuth with great readiness. The cut Fig. 197, on page 441, gives a distant view of both of Lord Rosse's great telescopes. The more remote one is the 3-feet reflector, the nearer one on the right is the 6-feet reflector between its two piers of massive masonry.

The advantages possessed by a reflecting telescope over a refracting telescope are that the former will bear a much greater magnifying power in proportion to its length, and that it gives images free from colour; for, as we have already seen (page 283), the best achromatic lenses give necessarily some outstanding uncorrected coloration. Against these advantages are to be set off the loss of light occasioned by the two reflections, the liability of the surface of the speculum to become tarnished, and the inconvenience arising from the weight of the mass of metal if the speculum is of large size, and lastly the difficulty of giving a parabolic curvature to the surface. From the time of the invention of the achromatic lens by Dollond, down to quite recent times, reflecting telescopes have not been much in use, always excepting those famous large ones made by W. Herschel, Lassell, and Lord Rosse, and others of large size. But during the last few years the use of reflecting telescopes has been greatly extended by the happy application of an easy chemical process by which a film of pure lustrous silver may be deposited on the surface of polished glass. The mirrors for the new reflecting telescopes are made not of speculum metal, but of glass. A disc of glass is, in the first instance, ground spherically concave on one side, and the spherically curved surface is then converted by careful and skilful manipulation into a parabolic figure. Foucault of Paris, Steinheil of Munich, and With of Hereford, have all acquired high reputations for the excellent performance of their silvered glass specula. With's specula are mounted in various ways by Browning of London. Fig. 194 represents a 12-inch reflector mounted on an equatorial stand of the most complete kind as constructed by Browning. The advantages claimed for silvered glass specula over those made of speculum-metal are their cheapness, lightness, greater reflecting power of the silver, and the ease with which the brilliant surface can be renewed by re-silvering when necessary.

The cheapening and improvement of reflectors attending the adop-

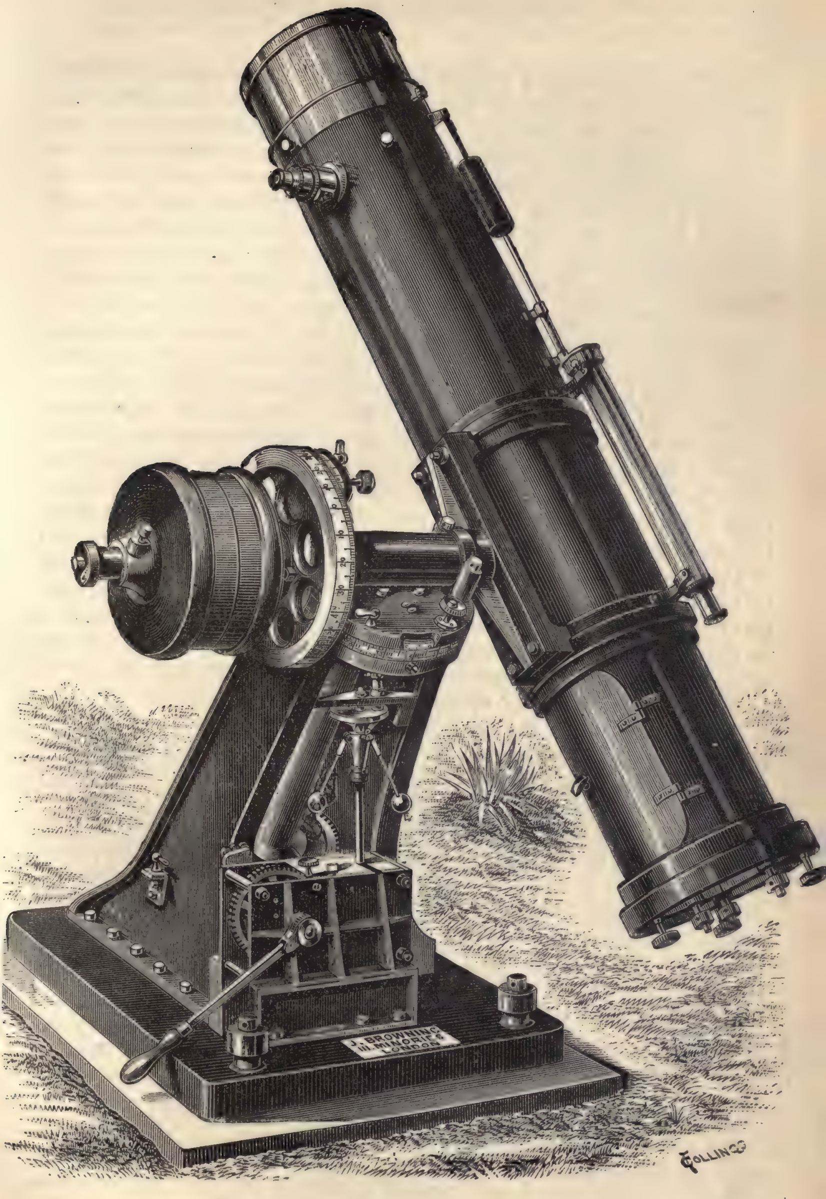

J. BROWNING
111 MINORIES
LONDON
COLLINGS

FIG. 194.

tion of the silvered glass specula has stimulated the makers of achromatic lenses to give them increased dimensions. The difficulties connected with the construction of lenses of larger diameter are very great. It is no easy matter to obtain glass discs of considerable size perfectly uniform throughout in their refracting power, and free from *striæ* and other defects. At the end of the last century a Swiss mechanic named Guinand succeeded, after long-continued attempts, in producing flint-glass suitable for the construction of achromatic object-glasses. The manufacture of flint-glass had, before this, been carried on in England only, and this country consequently enjoyed almost a monopoly in the making of achromatic lenses. But when intelligence of the success of the Swiss artizan reached Munich, FRAUNHOFER (1787—1826), the famous optical instrument maker, induced Guinand to remove to Bavaria about the year 1805. Fraunhofer then began to construct achromatic telescopes of diameters far exceeding anything hitherto attempted. With a telescope of Fraunhofer's construction of 10 inches aperture, Struve made his famous observations on double stars between 1824 and 1837; and, with another achromatic of 12 inches, Lamont of Munich made a series of interesting observations of the satellites of Uranus. Guinand returned to his native country in 1814, and subsequently supplied some of his now celebrated flint-glass to certain eminent French opticians, who most skilfully used it in the construction of achromatic lenses. Achromatic object-glasses of 2 feet diameter and upwards have been since constructed by Cooke, Clarke, Grubb, and other optical artists.

Stellar parallax was the subject of several investigations in the earlier part of the present century, but it was only about 1843 that the parallax of certain fixed stars was established and its amount ascertained. There is a star registered by astronomers as 61 *Cygni*, which is remarkable for the relatively large amount of its *proper motion*. That is, the position of the star in the heavens is changing so that it annually displaced 5″ of arc, or in about 360 years it will occupy a position separated from its present by an interval equal to the breadth of the sun or moon. Bessel concluded that as the movement of the star is greater than that observed in other stars, probably 61 *Cygni* was comparatively nearer to us. The star is one of the doubles having two components, and the angular distances of these from two very small stars near them was carefully determined at different seasons of the year. After all allowances and corrections had been made, Bessel's final result was that the annual parallax of 61 *Cygni* amounts to 0·3483″. A subsequent and independent investigation by M. Peters at the Pulkowa Observatory gave 0·349″ as the parallax of the same star. It will be noticed that the correspondence of the results is very close. Another star, having a large proper motion, α *Centauri*, was observed for parallax by Henderson at the Cape of Good Hope in 1832-3, and the result deduced was 1·16″. Maclean, who succeeded Henderson at the Cape of Good Hope, verified in 1840 the existence of a sensible parallax in this star, fixing it at a somewhat less value—namely, 0·9128″, which more recent observations have raised to 0·9187″. The parallax of *α Centauri* corresponds with a distance of about 20 billions of miles. Or, to represent this immense distance in another way, it may be stated that light, which travels with the velocity of 190,000 miles per second, takes no less than 3¼ years in reaching us from *α Centauri*. The distance of 61 *Cygni* is about 60 billions of miles, and if this star were suddenly extinguished, its image would, after that event, continue to shine in our skies with steady radiance for ten years, so long does its light take to reach us. The proper motion of 61 *Cygni*, which has been spoken of above, corresponds with a motion of translation of that star through space at the rate of at least 1,333 millions of miles per annum,—a velocity nearly three times as great as that of the earth in its orbit round the sun.

At the commencement of the nineteenth century the data in existence upon which was founded our knowledge of the earth's figure were the following arcs:—

| Date when completed. | Place. | Measured by |
|---|---|---|
| 1736 | Lapland . . . | Clairaut, Maupertuis. |
| 1743 | Peru . . . | Bouguer, La Condamine, etc. |
| 1752 | Cape of Good Hope. | Lacaille. |
| 1752 | Italy . . . . | Boscovich. |
| 1764 | America . . . | Mason, Dixon, etc. |
| 1768 | Austria . . . | Liesganig. |
| 1768 | Piedmont . . . | Beccaria. |
| 1794 | France . . . | Delambre and Mechain. |
|  | India . . . | Burrows. |

The earth's density was deduced from various pendulum experiments, especially the series instituted at Paris by Borda in 1790; the Schehallien observations of Maskelyne (p. 256); and Cavendish's experiments of the leaden balls (p. 257). In the years 1801, 1802, 1803, the arc in Lapland—which, according to the calculations of Clairaut and Maupertuis, presented some unaccountable anomalies—was measured over again, and extended so as to embrace more than 1½°. This was done by Ofverbon, Svanberg, and others, and the results obtained were more in accordance with those of other measurements. About the same time an arc was measured very accurately in England from the Isle of Wight to Clifton in Yorkshire. Two arcs were measured in India under the direction of Colonel Lambton. One of these embraced nearly 10°, and this was several years afterwards extended to 16°. Biot and Arago extended the French arc to a small island in the Mediterranean near Iviza, so that its whole length was now nearly 12½°. Short arcs were measured by the Geveva astronomers, and the distance between Dover and Falmouth having been obtained in the trigonometrical survey of England, the length of a degree of the parallel in that longitude was found by the difference of time between the two places, and this was determined by carrying chronometers between them. Another and much longer arc of longitude was measured by the French and Italians on the Continent. Captain Sabine conducted a valuable series of pendulum observations in various latitudes, and these permitted the relative intensity of gravity at different places to be accurately determined. The results of all these measures of arcs is to give to the earth an ellipticity of about $\frac{1}{300}$, some persons fixing on a number a little greater, others on one a little less. The pendulum experiments give, by a theorem of Clairaut's, an ellipticity somewhat greater than this.

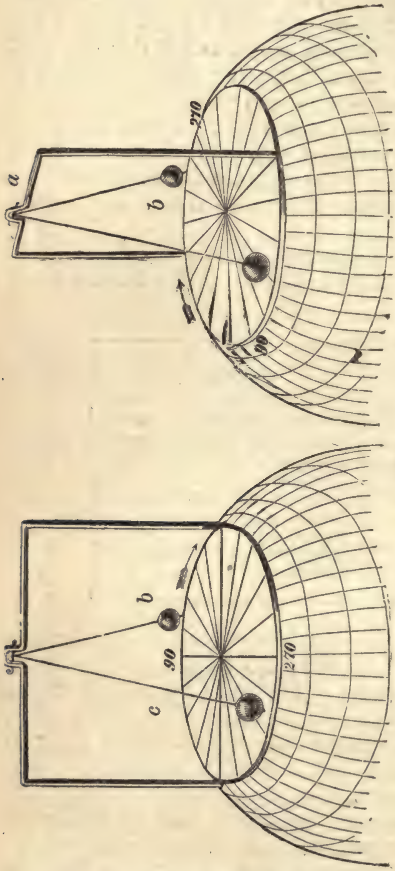


FIG. 195.

In this place we may describe the pendulum experiments devised by Foucault in 1850, by which the earth's rotation on its axis was in a manner made visible to the eye. The experiment depends on the fact that a freely suspended body when made to oscillate will, in the absence of any disturbing force, continue to perform its oscillations in one plane. The application of this principle to the exhibition of the earth's rotation will be readily seen from Fig. 195, showing a model in which a body is suspended at a point *a* in the line of the axis of a globe representing the earth. The pendulum will continue to make its vibrations in one plane when the globe is turned on its axis as shown in the figures. If such a pendulum could actually be erected at the earth's pole, and set to oscillate in a certain plane, that

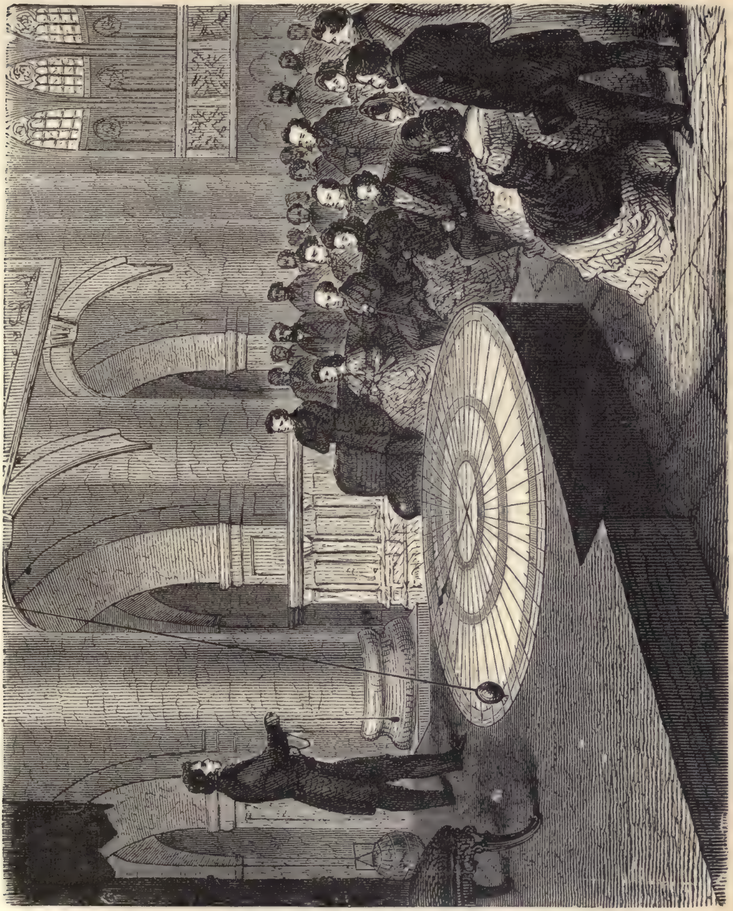


FIG. 196.—FOUCAULT'S PENDULUM EXPERIMENT.

plane, preserving as it would a fixed direction in space, would appear to an observer to turn slowly in the direction of the hands of a watch at the rate of 15° per hour, in consequence of everything but the plane of oscillation being carried round by the earth in its rotation. If such a pendulum were set in motion at other places on the surface, the like apparent change in the plane of rotation would be observed; but its rate would be less and less as the plane was more distant from the pole and nearer to the equator. Thus, while at the pole the plane of rotation appears to turn through a complete circle in twenty-four hours, in latitude 54° twenty-eight hours nearly would be required;

in latitude 41°, thirty-five hours; in latitude 19°, seventy-one hours; in latitude 5°, eleven days would be the times approximately. The experiment is usually performed by suspending an iron ball of at least 14 lbs. weight by a very fine and long steel wire. The length of the suspending wire should be at least 20 feet, though with even a height of 12 feet the effect is observable. The experiment has occasionally been very well exhibited in very lofty buildings, such as churches. Fig. 196, from the roof of which a very long wire could be suspended. The change in the direction of the plane of oscillation in the space of an hour can be seen by a large audience, and its rate may be followed by means of a graduated horizontal circle. It will of course be understood that a properly suspended pendulum of this kind will continue its oscillations for several hours.

The progress of astronomy is indicated by the great increase in the number of observatories. At the opening of the present century many national observatories had been established. Englishmen have occasion to regard with satisfaction the work done at the Royal Observatory of Greenwich, for the observations made there have been conducted with the utmost regularity, and their correctness has never admitted of a doubt. It is from the materials furnished by these excellent observations that astronomers have succeeded in bringing their tables of the sun, moon, and planets to the present state of perfection. For the complete knowledge of the moon's motions especially, the world is indebted to the Greenwich establishment; and mainly to data derived from observations made here are due the precise determinations of aberration (p. 250), refractions, etc., which we now possess. Delambre remarks in his "History of Astronomy in the Eighteenth Century," that were all other records of the science destroyed, the Greenwich observations alone would furnish materials sufficient to reconstruct the whole body of astronomy. In more recent times our national observatory enlarged its scope by the addition of appliances for the study of meteorological and magnetical phenomena, and the records kept in these departments have been published with the same regularity as the astronomical observations. The value of these observations, so trustworthy, regular, and prolonged, has been incalculable.

Among the most notable of the observatories erected during the present century may be mentioned the new one established by the Russian Government in 1839 at Pulkowa, a small town about ten miles from St. Petersburg. This establishment has been designed on a magnificent scale, and provided with the finest instruments from the hands of the best German and English artists, and altogether it may be considered as one of the most complete observatories in the world The cost of its erection and equipment amounted to £100,000, and more than £10,000 per annum are supplied by the imperial treasury for its maintenance.

In the United States astronomical pursuits have been encouraged by the establishment of several well-equipped observatories with funds supplied by the State Governments, or provided by the munificence of private individuals. At Washington there is a central observatory; but one of the most famous observatories in the States is that of Harvard University, Cambridge, Massachusetts, which possesses one of the finest refracting telescopes in the world. It was by this instrument that Bond discovered the eighth satellite of Saturn. Several ingenious applications of the principle of the electric telegraph to time-signalling, and to the recording of the moment at which astronomical phenomena occur, are due to American men of science, and such applications have enabled astronomers to observe the time of these occurrences with a marvellous degree of precision.

Among other influences promoting the improvement of astronomy may be named the regular publication of observations. Several Ephemerides were, it is true, published periodically during the preceding century, but their utility was restricted by the absence of sufficient detail in the records. In 1800 monthly astronomical journals were first published in Germany, and this system has been followed up in various quarters with excellent results. The foundation in 1820 of the Royal Astronomical Society of London marks another epoch in the recent history of the science.

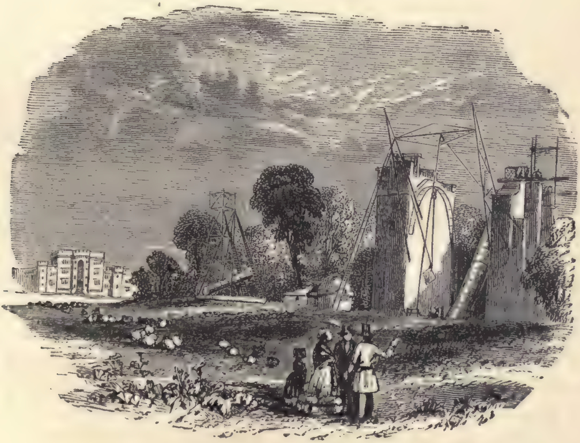


FIG. 197.—LORD ROSSE'S TELESCOPES.

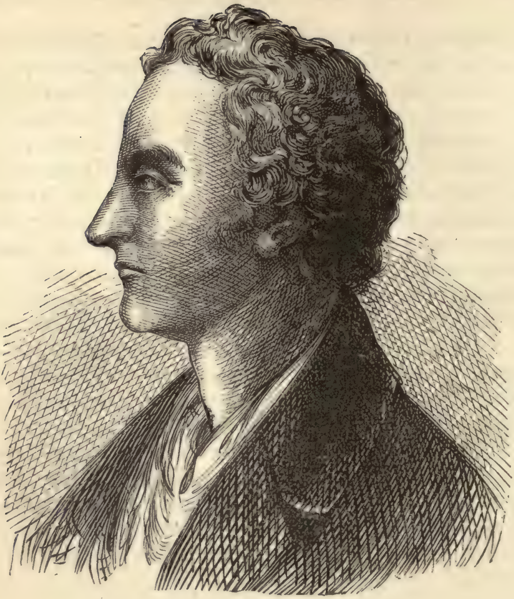


FIG. 198.—THOMAS YOUNG.

# CHAPTER XVII.

## PHYSICS OF THE NINETEENTH CENTURY—LIGHT.

THOMAS YOUNG (1773—1829) was born at Milverton, in Somersetshire. His earliest years were marked by the display of talents quite extraordinary for his age; and his maturity fulfilled the promise of his infancy. At two years of age he was able to read fluently, and even at this time his extraordinary memory excited the astonishment of his elders. By four years of age he had learnt by heart many English poems, and he could recite several Latin ones, although as yet he did not understand the language. Young was sent to school when six years old, his teacher being a person of mediocre attainments, who perhaps did not do much directly to develop the brilliant abilities of his pupil. The boy's true intellectual vocation was revealed by his accidental acquaintance with a land-surveyor, who took a great fancy for him, and sometimes carried him into the country, where Young was greatly interested by the operations of the surveyor. Those operations especially by which the distances and heights of inaccessible objects were determined excited his wonder, until he began to see through the mystery by the aid of an encyclopædia of mathematics. To use the theodolite now became the boy's outdoor recreation; to calculate heights and distances from the observed angles his indoor amusement. Between the ages of nine and fourteen Young attended a school at Compton, in Dorsetshire, kept by a Mr. Thompson. Here Greek and Latin were, as usual, subjects to which much time was devoted; but Young had the opportunity of acquiring from the usher, Josiah Jeffery, some insight into experimental science in his leisure hours. The linguistic acquirements which Young of his own accord added to all the ordinary school duties are perfectly amazing. They comprised French, Italian, Hebrew, Persian, and Arabic. His mathematical abilities were no less extraordinary, as may be gathered from the following circumstances. He had taken a great liking to the study of botany, and soon found that, in order to examine the minute organs of plants, a microscope was necessary. He set about constructing one for himself from the mere description of the instrument; but having met with some mathematical formulæ relating to the optical arrangements, in which formulæ there occurred certain symbols that he did not understand, he actually entered upon the study of the differential calculus, to which these symbols belonged.

The boy's excessive devotion to severe studies is said to have affected his health, as he was threatened with symptoms of serious disease; but by medical skill and the affectionate care of his friends his health was shortly afterwards restored. Young after this pursued his studies at Youngerbury, in Hertfordshire, under the direction of Mr. David Barclay, who afterwards made himself a reputation in classical literature. Dr. Brocklesby, the brother of Young's mother, was at this time one of the most successful physicians in London, and this gentleman had the means of placing his nephew in communication with some of the most influential men of the day. Young's brilliant talents were recognized, and had he chosen to enter the arena of public or forensic life, under the patronage of some of the great politicians or statesmen of that time, he would no doubt have had an easy road to the honours which reward success in such a career. His choice was otherwise, for he preferred intellectual freedom to the obligations of political partizanship, even though the fetters of the latter might have been forged with golden links. He selected medicine as his profession, and pursued his studies first in London, afterwards at Edinburgh, under Black, Munro, and Gregory. He graduated in medicine at Cambridge University also, where he was familiarly known as "Phenomenon Young," and took the degree of M.D. at Gottingen in 1795. Before this Young had become known by his scientific papers on various subjects. A paper by him was published in the "Transactions of the Royal Society" when its author was little more than twenty years of age. It was a subject of some difficulty, namely, the means by which the eye is adapted for perceiving objects whether distant or near, so that the image is brought to a proper focus on the retina. Some philosophers had supposed that the ball of the eye, as a whole, was capable of changing its form, so as to become more or less flattened as occasion required; others had contended that the crystalline lens was capable of the same kind of movement as the lens of the *camera obscura*, others, that the curvature of the external part of the eye, called the *cornea*, changed, in order to give the necessary optical adjustment; others, again, that a change took place in the curvature of the crystalline lens. This last explanation had been the subject of discussion before Young advocated it; but Sir Everard Home, the eminent anatomist, and Ramsden, the celebrated instrument maker, conjointly decided against this view, and declared that the crystalline lens was unalterable; and Young himself, deferring to authorities so great, thought that he must have been mistaken. But a few years afterwards he withdrew this retractation, and in a paper published in 1800 he enters very fully into numerous observations and direct experiments in support of his views.

Young tells us that in 1801, when reflecting on the beautiful optical experiments of Sir Isaac Newton, he discovered a law which appeared to him to account for a greater variety of interesting phenomena than any principle that had then been made known. What this principle was he explains by the following comparison: "Suppose a number of equal waves of water to move upon the surface of a stagnant lake with a certain constant velocity, and to enter a narrow channel leading out of the lake; suppose, then, another similar cause to have excited another equal series of waves, which arrive at the same channel with the same velocity and at the same time with the first. One series of waves will not destroy the other, but their effects will be combined. If they enter the channel in such a manner that the elevations of the one series coincide with those of the other, they must together produce a series of greater joint elevations; but if the elevations of one series are so situated as to correspond to the depressions of the other, they must exactly fill up those depressions, and the surface of the water must remain smooth,—at least, I can discover no alternative, either from theory or from experiment. Now, I maintain that similar effects take place whenever two portions of light are thus mixed, and this I call the general law of the *interference of light*." It is in these terms that Young first unfolded his beautiful discovery of the *interference of light*, concerning which we shall have more to say presently.

About 1801 Young became connected, as Professor of Natural Philosophy, with the Royal Institution, which had then been recently established, and has since been made famous by the distinguished men who have filled its chairs—by Young and Davy, Faraday and Tyndall. No man of the present century has displayed a wider range of acquirements than Young. Look over a list of his writings, and you will see medicine and engineering, mathematics and fine art, astronomy and philology, mechanics and zoology, represented by turns. Though he was brought up as a member of the Society of Friends, and though his acquisitions would have been supposed compatible only with the life of a retired student, he was, on the contrary, a man of the world, witty, elegant in manners, and mixing in the best society of the capital. He was well versed in the fine arts; there was scarcely a musical instrument which he could not play; and he acquired a competent knowledge of painting. His discovery of the key to the mysterious Egyptian hieroglyphics, whose interpretation had baffled the learned for so many ages, added greatly to his fame. It may appear singular that a man of Young's extraordinary intellectual power should have achieved but a very indifferent success in the practice of medicine, which was the career he had originally chosen. Perhaps it was the amount of his knowledge which made him less confident in prescribing medicines of which he could not calculate all the effects, for the tendency of his mind was essentially towards the study of truths admitting of deductive demonstration. In 1818, when he was appointed Secretary to the Board of Longitude, he relinquished the practice of medicine altogether, and devoted himself to the superintendence of the "Nautical Almanac," while the "Journal of the Royal Institution" gave evidence of his capabilities of dealing with the most difficult problems of astronomy and navigation. Young died in 1829 at the age of fifty-six.

It was in the "Bakerian Lecture" before the Royal Society in 1800 that Young, in announcing that the more recent observations made by himself and others had induced him to revert to his former opinion, noticed also other interesting particulars relating to the "mechanism of the eye." The records of the many observations and discoveries made from time to time concerning the eye constitute of themselves quite an extensive literature. Indeed, the extent of the bibliography of this one subject would certainly astonish a reader unversed in scientific inquiries. The human eye has been studied in many different relations — anatomical, physiological, optical, medical, and psychological, and the results form a series of discoveries of the highest interest and beauty. Our knowledge of the eye, however, unlike many of the great epoch-marking discoveries in science, cannot be referred especially to any individual investigation. It has been a growth, and the result of the general advancement in the several branches of sciences, and of the researches made by a host of inquirers, especially during the seventeenth, eighteenth, and nineteenth centuries. Young's contributions to the theory of vision, which were laid before the Royal Society in the Bakerian Lecture for 1800, were important additions to our knowledge of the subject; and though these researches form by no means the chief title of Young to scientific fame, a few observations relative to the "Mechanism of the Eye" (the title of the Bakerian Lecture for 1800) may here be made. And as certain discoveries relative to vision may also be referred to in subsequent pages, the diagram in Fig. 199 will serve to illustrate not only Young's observations, but others to which allusion may be made hereafter.

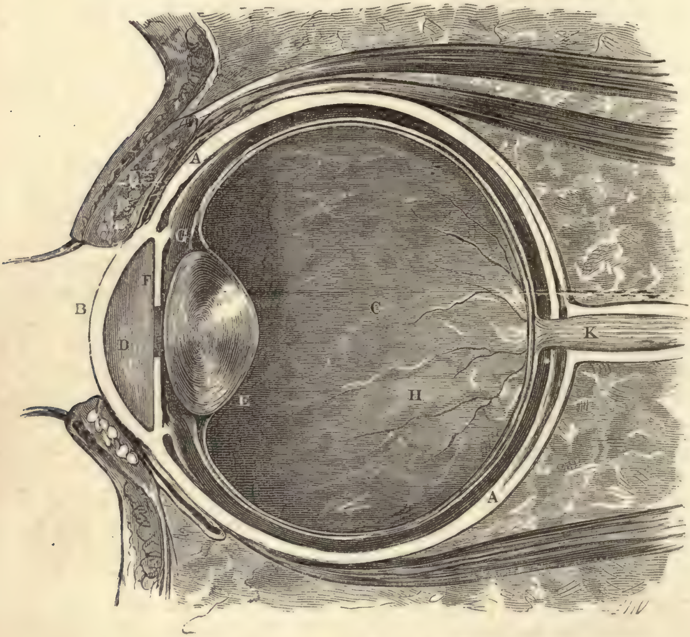


FIG. 199.—VERTICAL SECTION OF THE EYE.

Fig. 199 represents in section the eyeball, which is nearly globular in form, its outer covering being a firm tough case. This covering, called the *sclerotic coat*, A, is white and opaque, except in the front, where it becomes transparent, and forms the *cornea*, B. The first refraction of the rays of light entering the eye occurs at the outer surface of the cornea, but the two surfaces of this coat being nearly parallel, its refractive power has little or no effect, and the cornea serves only to give a proper form to the *aqueous humour*, D, which fills and distends its concavity. The aqueous humour is partly divided by the *iris*, F (this is the part which gives different colours to the eyes of different individuals). This is pierced in the centre by the circular opening called the *pupil*, and immediately behind this is the *crystalline lens*, G E. The substance of the crystalline lens is much more refractive than the aqueous humour, and it increases in density towards its centre. The larger cavity of the eyeball C is completely filled with a watery fluid called the *vitreous humour*, and its inner surface on the back part is lined by the *retina*—a delicate network of nerves supported by a dark-coloured layer called the *choroid*. The pencils of rays which enter the pupil of the eye are rendered more convergent by the refraction of the crystalline lens, and are collected into foci on the retina, where they form an inverted image according to the law of refraction by lenses (page 157).

Young applied the mathematical formulæ of optics to calculating the refractions of the various humours of the eye, and he determined by ingenious methods the necessary experimental data. He noticed that a small luminous spot, such as a star or the image of a candle in a small concave mirror, never appears perfect in form, but always presents itself with the radiated appearance. These radiating lines he attributed to inequalities in the refraction, occasioned by the fibrous structure of the crystalline lens. Fig. 200 displays the arrangement of the superficial layer of fibres in the crystalline lens of the human eye, and the six-rayed figure, divided into branches, corresponds with the appearances presented by luminous points. Young mentions an elegant and very simple experiment by which Dr. Wollaston proved that the eye is not truly achromatic. He looks at a luminous point (*e.g.*, a brgiht star) through a prism, which then gives, of course, a spectrum (page 217) having no apparent breadth. Nevertheless, the eye not being able to bring at one time the several rays of this linear spectrum to a focus, it happens that when the red extremity is seen distinctly as a point, the blue rays will, by the same adjustment of the eye, be too much refracted, and will appear expanded into a surface; when, on the other hand, the focus is adapted for the blue extremity, the red end appears expanded: in either case the *line will appear as a narrow triangle*. That the accommodation of the eye to view objects at different distances is effected by a change in the curvature of the crystalline lens Young had now satisfied himself by his own observations and those of others.

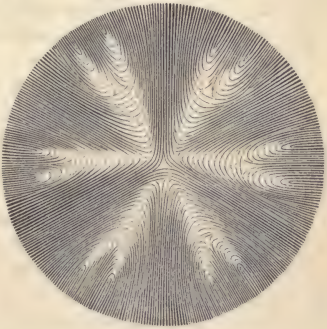


FIG. 200.

In the Bakerian Lecture for 1801 Young drew attention to the theory of undulations as explaining the phenomena of light. He did not so much propound a new theory as support by additional evidence one which had been already proposed; and he applied it to the explanation of a number of facts of very diversified character, which had previously been scarcely noticed. In advocating anew the doctrine of undulations, Young is at great pains to invoke, whenever he can, the great authority of Newton,—*clarum et venerabile nomen*,—adducing on each occasion from the writings of his illustrious predecessor all passages which appear to favour the admissibility of his own doctrines. The passages quoted would seem to show that Newton himself had suggested the hypothesis of undulations in a rare medium; but they are chiefly from the Queries where Newton, in his "Optics," advances a variety of speculations. The citation of these speculations of Newton's certainly caused the doctrines now definitely enunciated by Young to be more attentively considered, particularly by English men of science, among whom any opinion supported by Newton's approval had the highest authority in its favour. Newton's suggestions include the hypothesis of an "ethereal medium" constituted like atmospheric air, but far rarer, subtler, and more strongly elastic; that by this medium heat may be conveyed through spaces deprived of air; that by its vibrations light is "put into fits of easy reflection and easy transmission." This medium may pervade all bodies, and by its elastic force expand through all the heavens, where it need not necessarily retard the motions of planets and comets in any sensible degree, for its resistance may be so small as to be inconsiderable. For instance, if the *ether* (for so Newton calls it) be supposed 700,000 times more elastic than our air (that is, requiring for the same amount of compression a force 700,000 times greater), and be also supposed 700,000 times rarer than air, the resistance it would offer to a body moving through it would be 600,000,000 times less than that offered by water; and so small a resistance would scarcely make any perceptible alteration in the motion of the planets in 10,000 years. The ether is further supposed to be, like air, capable of vibrations or tremors, only the vibrations are far more swift and minute, for while a vibration of the air set up by a man's voice may occupy a space of 8 or 12 inches, those of the ether may be reckoned at not more than $\frac{1}{100000}$th part of an inch in extent. Such are some of the suggestions thrown out by Newton in the passages whereon Young rests a claim to his predecessor's support. But that Newton's conception was something different from the fundamental postulate of the undulatory hypothesis appears from several of the phrases quoted, as, for instance, where he suggests that the *vibrations may overtake the rays of light*, etc. It would appear that the hypotheses concerning the nature of light in which Newton admitted vibrations, were really much more complicated than the undulatory doctrine. An objection which presented itself to Newton's mind against admitting mere undulations as the mode of action, is clearly stated by himself: "Are not all hypotheses erroneous," he asks, "in which light is supposed to consist in pression or motion propagated through a fluid medium? If it consisted in pression or motion, propagated either in an instant or in time, *it would bend into the shadow*, for pression or motion cannot be propagated in a fluid in right lines beyond an obstacle which stops part of the motion, but will bend and spread every way into the quiescent medium which lies beyond the obstacle. The waves on the surface of stagnant water, passing by the sides of a broad obstacle which stops part of them, bend afterwards and dilate themselves gradually into the quiet water behind the obstacle. The waves, pulses, or vibrations of the air, wherein sounds consist, bend manifestly, though *not so much as the waves of water*, for a bell or a cannon may be heard beyond a hill which intercepts the sounding body . . . but *light is never known to bend into the shadow*." Some expressions to which we would specially direct the reader's attention we have placed in italics, because the bending of light round an object was now experimentally proved to be precisely what occurred. The bending is less in air than in water, and extremely small in the case of light, because of the extreme minuteness and immense velocity of the undulations.

It is especially for his development of the *undulatory theory* of light that Young's name will always be remembered in the history of science. It has been already mentioned that Newton preferred what is called the *corpuscular theory* of light, which explains the phenomena by supposing that very minute particles are actually projected from luminous bodies, and that they continue to move with the velocity which light possesses. The alternative hypothesis, which was originally proposed by Hooke and Huyghens, attributed light to undulations of a highly elastic medium. It might, at first sight, appear that the corpuscular theory had the advantage of its rival in simplicity. But this simplicity is only apparent, for when the facts are studied closely it is found that the emission theory requires special modifications to explain them; and, further, that the modifications required by the different classes of facts are independent of and unconnected with each other. On the other hand, when the undulatory theory came to be applied to newly-observed phenomena, the modifications it required had, in many cases, a connection with others, or were mutually confirmative. These remarks may be illustrated by Newton's proposed explanation of his "rings" (page 223) by "fits of easy reflection and transmission;" his explanation of diffraction by an attraction between the particles of light and the solid body near which they pass (page 226); while double refraction required that the particles of light should be assumed to possess yet another property unconnected with either of the former—namely, *polarity*, which purely arbitrary assumption has given a convenient name to certain conditions of light. The manner in which the undulatory hypothesis lends itself to the explanation of very different phenomena will be seen in the accounts of certain experiments proving the *interference*, *diffraction*, and *polarization* of light, which will shortly be brought under notice.

The undulatory theory was suggested to Hooke by the colours of thin films, of which "Newton's Rings" form a striking example; for, seven years before Newton's experiments were made, Hooke had published in his "Micrographia" an account of these phenomena and an explanation. He described the coloured films that are obtained by dividing a piece of transparent and colourless talc into flakes sufficiently thin, or by blowing glass to the greatest possible tenuity. He remarks that any transparent substance made sufficiently thin will show colours, and he shows that the particular tint depends upon the thinness. In his explanation of these effects he comes so near to the undulatory theory, that Young states that he would himself have earlier been led to that theory if he had previously seen the passage in the "Micrographia" where Hooke thus speaks of the cause of the colours of thin plates: "It is most evident that the reflection from the under or farther side of the body is the principal cause of the production of these colours. Let the ray fall obliquely on the thin plate, part therefore is reflected back by the first superficies, part refracted to the second surface, where it is reflected and refracted again; so that after two refractions and one reflection there is propagated a kind of fainter ray, and by reason of the time spent in passing and repassing, this fainter pulse comes behind the former reflected pulse; so that hereby (the surfaces being so near together that the eye cannot discriminate the two pulses) this confused or duplicated pulse, whose strongest part precedes and whose weakest follows, does produce on the retina the sensation of a yellow. If these surfaces are farther removed asunder the weaker pulse may become coincident with the reflection of the second or next following pulse from the first surface, and lie behind that also, and be coincident with the third, fourth, fifth, sixth, seventh, or eighth; so that if there be a thin transparent body, that from the greatest thinness necessary to produce colours does by degrees grow to the greatest thickness, the colours shall be so often repeated, as the weaker pulse does lose paces with its primary or first pulse, and is coincident with a subsequent pulse."

Young affirmed that Newton's rings and the colours of thin plates are due to the *interference* of undulations reflected from both surfaces of the film. Each ray is in part reflected from the first surface, and in part transmitted with refraction, so that this part reaches the second surface, where again it is in part reflected, and this part passing out with another refraction through the first surface, emerges in a direction perfectly parallel with the portion of the original ray reflected from the first surface. Thus in Fig. 201, if A B C D represent a thin transpa-

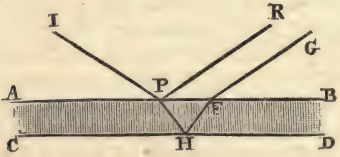


FIG. 201.

rent film, and I P be a ray of light falling upon it at P, part of that ray is reflected along P R and part enters the film in the direction P H, and this arriving at H, is (wholly or in part) reflected in a ray along H F, which emerges from the film in the direction F G parallel to P R. Now, it is plain that the light which follows the course I P H F G has to pursue a longer path than that which follows the course I P R, and therefore it may happen that the undulations which have followed the longer course shall be half a wave-length, or some *odd* multiple of the half-wave-length, behind the undulations which have followed the shorter course; in such a case the rays would destroy each other, and the result would be darkness. In this way the black rings seen in Newton's experiment with monochromatic light are accounted for. Ordinary light causes a transparent film to appear coloured when the thickness of the film is such that the set of undulations on which some particular colours depend are thus made to destroy each other by interference. The film then appears tinted with the complementary colour. Thus if it be the *green* rays which are destroyed, the film will appear red, for this is the colour which predominates in the rays of the spectrum remaining after abstraction of the green. Young gives a very simple construction to show the conditions required in order that a film may exhibit one and the same colour throughout, the result being that the thickness of the film must vary as the secant (page 61) of the angle of reflection, and this agrees exactly with Newton's experiments. Another case of the interference of light was pointed out by Young in the effect fine fibres have upon light. Coloured fringes are visible when a candle is looked at across a single thread as spun by the silkworm. The cause of these coloured fringes, said Young, must be sought in the interference of the portions of light bending round each side of the fibre.

The next great discovery regarding light was made by a young French mathematician, STEPHEN LOUIS MALUS (1775—1812). Malus was one of the first pupils of the celebrated Ecole Polytechnique, where he soon acquired the friendship of Monge, the mathematician. He entered the engineering corps of the French army at the age of twenty-one, and was immediately engaged on active service in Flanders, Germany, Egypt, and Syria. His experiences in the East included marches, battles, sieges, sackings of towns, and an attack of the plague. In 1801 he married, and in 1802 we find him engaged in military engineering duties at Lisle, and he was subsequently employed in constructing fortifications at Antwerp, Strasbourg, and Kehl. In 1807 Malus presented his first paper to the Academy of Sciences, to the membership of which he was elected in 1810. He died in 1812 at the early age of thirty-seven. The discovery with which the name of Malus will ever be associated is that of the *polarization of light by reflection.* The phenomena of polarization are not only among the most curious and brilliant in the whole range of optical science, but their study has proved most fruitful of results as regards the theories, and the most fertile as opening the way to new discoveries. In 1805 Malus was one day viewing through a doubly-refracting crystal of Iceland spar the rays of the sun reflected from the windows of the Luxembourg Palace. Instead of two images, which with ordinary light are formed by Iceland spar, he was astonished to find that the crystal exhibited but one. In whatever position he held the crystal, but one image of the sun-illumined window could be seen; the only difference being, that in one position he perceived the image due to the "*ordinary*" ray, and in the transverse position that due to the "*extraordinary*" ray. In the evening of the same day on which he made this observation, he instituted some experiments in order to determine if possible the conditions under which the phenomena were produced. He found that the light of a candle reflected from a glass plate at one particular angle (about 35°) was as completely polarized as the rays emerging from crystals of Iceland spar. A ray reflected from water at the incidence of 36° he found also to be polarized; and, in general, the light reflected from all *transparent* bodies is always more or less polarized, and that reflected at a certain angle which varies from one body to another is completely polarized. Here, then, was a curious and but recently discovered property of light, hitherto known only as an exceptional and rare phenomenon connected with only a very few kinds of mineral crystals, at once proved to be as common as daylight and as ancient as the world. At first Malus supposed that, besides the polarization by doubly-refracting crystals, reflection was the only means of polarizing light. But further investigations showed that the light which had passed through transparent bodies was, to some extent, polarized. The fact is, that if a ray of ordinary light is incident on a plate of glass at the angle of 35°, a portion of the light is reflected, and the whole of this portion is polarized; but the greater part is transmitted, and of this transmitted beam a part is polarized equal to that which is polarized by reflection. By increasing the number of successive reflections, we can obtain a ray almost completely polarized by its passage through a sufficient number of parallel transparent plates. Hence a bundle of thin plates of glass is often used as the means of obtaining polarized light.

Malus himself was an adherent of the Newtonian or corpuscular theory of light, and his study of polarization phenomena led him to conclude that neither the corpuscular nor the undulatory theory could explain these phenomena. Even Young declared that their explanation was a problem which "would probably long remain to mortify the vanity of an ambitious philosophy, completely unresolved by any theory." But though admitting the insufficiency of his own hypotheses respecting the nature of light to explain the phenomena of polarization, yet he did not consider that these phenomena had proved the undulatory theory to be false. These admissions were welcome intelligence to

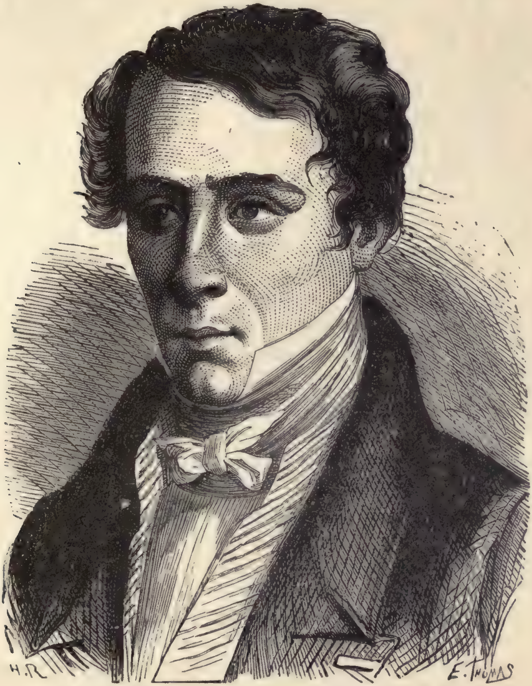


FIG. 202.—AUGUSTE FRESNEL.

Malus, who was a declared partizan of the emission theory. A few years afterwards (in 1817) we find Young taking a more hopeful view of the capabilities of the undulatory theory, and a passage from one of his letters to Arago, in which the subject is referred to, is highly interesting as giving the first hint of the *transverse vibrations*, and showing that his previous conception of the undulations of the ethereal medium corresponded with that entertained regarding the undulations by which sound is propagated. "I have been reflecting," says Young, "upon the possibility of giving an imperfect explanation of the affection of light which constitutes polarization, without departing from the genuine doctrine of undulations. It is a principle of the theory that all undulations are simply propagated through homogeneous mediums, in concentric spherical surfaces, like the undulations of sound, consisting simply of the direct and retrograde motions of their particles in the direction of the radius, with their concomitant condensations and rarefactions. And yet it is possible to explain in this theory a *transverse* *vibration*, progated also in the direction of the radius, and with equal velocity, the motions of the particle bearing a certain constant direction with respect to that radius; and this is polarization."

The decisive adoption of the undulatory hypothesis is due to the genius of another illustrious Frenchman, AUGUSTE JEAN FRESNEL (1782—1827). His early childhood forms a curious contrast to that of Young, for we are told that at eight years of age Fresnel could scarcely read. No one would have predicted Fresnel's future intellectual greatness from his place in the school classes. But even at school he had shown a turn for experiments, and by his schoolfellows was dubbed with the name of "the genius." From his home at Caen, in the heart of Normandy, Fresnel proceeded at the age of sixteen to become a pupil of the Ecole Polytechnique. His health had always been delicate, and yet he supported the fatigue of seven years' tuition.

On completing the required course of instruction, Fresnel was passed into the department of "*Ponts et Chaussées*" (bridges and roads), and when he had obtained the rank of engineer he was sent by the Government to repair the destruction which the recent civil war had effected in La Vendée. His duties there, and the manner of their performance, are thus sketched by Arago in his *éloge* of Fresnel (*Smyth*, *Powell*, *and Grant's Translation*): "To level small portions of roads; to seek, in the countries placed under his superintendence, for beds of flint; to preside over the extraction of the materials; to see to their deposition on the road or in the wheel-ruts; to execute, here and there, a bridge over the irrigation drains; to re-establish some metres of bank which the torrent had carried away; to exercise principally an active *surveillance* over the contractors; to verify their accounts; to estimate scrupulously their works,—such were the duties, very useful though not very lofty, not very scientific, which Fresnel had to fulfil during eight or nine years in La Vendée, in Drôme, and in Ile et Vilaine. How heavily must a mind of such power have been affected, when he compared the use which he might have made of those hours, which pass so quickly away, with the manner in which they were being spent! But with Fresnel conscientiousness was the foremost part of his character, and he constantly performed his duties as an engineer with the most rigorous scrupulousness." The political and dynastic troubles of which France was the scene in 1814 and 1815 were the occasion of Fresnel's being deprived of his appointment. This gave him leisure for study and research. In 1814 he writes from Nyons to Paris, asking to have sent to him works from which he may gather information on the "polarization of light," of which phrase he says he does not know the meaning. Eight months after this his own researches had raised him to the highest rank as a physicist. In 1815 he was reinstated in Government employment as an engineer of the pavements of Paris, and as secretary to a commission for lighthouses. In 1819 he obtained the prize offered by the Academy of Sciences for the best essay on *diffraction.* In 1823 he was unanimously elected a member of that academy, and two years afterwards the Royal Society of London appointed him one of their foreign associates. In 1827 the same body awarded him the Rumford Gold Medal. A few days after he had received this scientific distinction he passed away, at Ville d'Avray.

It is the glory of Fresnel that he established the undulatory theory on the sure basis of quantitative determinations. His investigations conducted him to mathematical formulæ which expressed the laws of the phenomena in all their generality, and the consequences deduced from these mathematical expressions led the way to new and unexpected discoveries. Fresnel's experimental arrangements were as effective as his mathematical analysis was far-reaching. His famous experiment of the inclined mirrors is perhaps the most direct method of producing the phenomena of interference, and of studying their significance. In this experiment a light falling upon a screen, say, of white paper, is turned to darkness, not by interposing an opaque body, or turning aside the rays of light, but actually by the addition of more light-rays. That is, light added to light is capable, under certain conditions, of producing darkness. In order that the reader may with the more certainty conceive the manner in which this apparently paradoxical

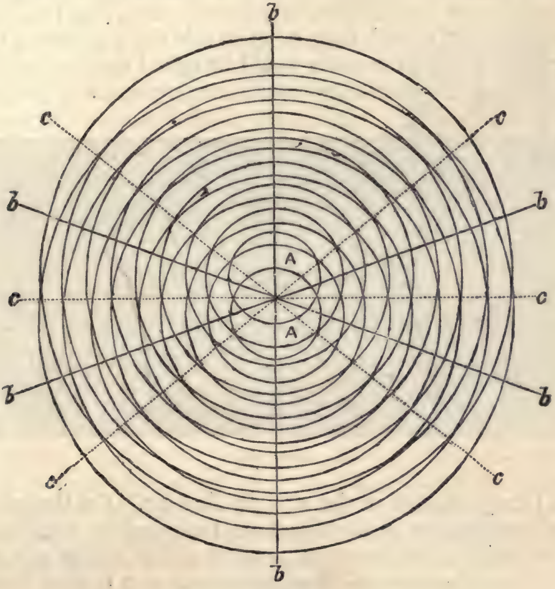


FIG. 203.

effect is explained by the undulatory theory of light, he may here again be reminded by the diagram in Fig. 203 of the effect of two systems of interfering waves. We shall suppose the diagram to represent the waves raised on a quiet pool of water by simultaneously dropping in two stones at the points A A. Waves will spread in circles from each point of disturbance as a centre, and the circles in the diagram may be taken as representing the *crests* of the waves at some one instant of time. The deepest part of the hollows between the waves would of course be half-way between these circles. Now, the two systems of waves would interfere with each other in this way: wherever the crests of two waves coincide, the water would be raised to double the height; and this, it may be observed, occurs at certain series of points, as, for instance, those included in the straight line C C. On the other hand, it will be noticed that in the directions indicated by *b b* the *crest* of a wave always occupies the same position as the hollow of a wave belonging to the other system. The consequence is that at those points the two sets of waves annul each other, and then the water is (for the instant) at its ordinary level.

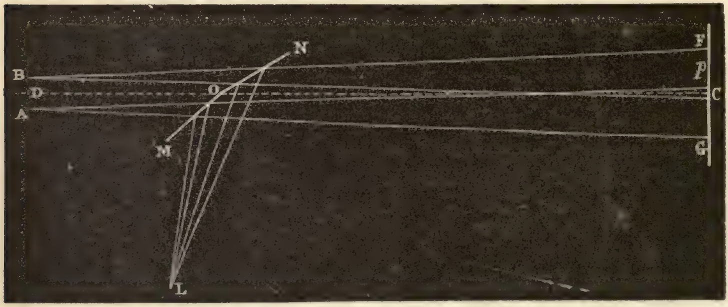


FIG. 204.

In Fig. 204 we have a diagram representing by the lines O M, O N, the edges of two upright metallic plane mirrors, which at O form a very small angle with each other. At L, rays of sunshine, enter the dark room through a very narrow vertical slit, and falling upon the two mirrors, are reflected to a white screen placed at F G. By the laws of ordinary reflection the effects will be precisely the same as if the virtual images of the slit A B were real sources of light. It is a great advantage in this arrangement that by merely decreasing the inclination between the mirrors, the two images or light-sources, as we may now consider them, may be made to approach as near to each other as may be desired. Let us now consider *p* a point on the screen which receives rays from both sources. It will be obvious that *p* is less distant from B than from A, and that the difference will be less as *p* is nearer to C, where of course it would become *nil.* It will be seen that the distances C O, O L, can be easily measured, and also the angle which A B subtends at C. When this has been done, the accurate calculation of the distances $p$ A and $p$ B will present no difficulty. When homogeneous light, *i.e.*, light of one colour and refrangibility, is admitted through the slit L, the screen exhibits a number of alternate light and dark stripes as in Fig. 205. The centre C (which is equally distant from A and B) is *always* light, but at a certain interval right and left of it dark bands, 1,1′; 2,2′, etc., are seen. When the distances of the black band 1 from A and B are calculated, they are found to differ by some extremely small distance, which we may call $d$. The same calculation for the second dark band 2 shows a difference in the paths of the rays from A and B of just *three* times $d$; the third dark band 3 is placed where the difference of length amounts to 5 $d$, and so on of the odd multiples of $d$. Then if the differences for the bright bands be calculated, they are at C = 0; at $a = 2\,d$; at $b = 4\,d$; and so on, following the even multiples of $d$. What is this small difference $d$ thus continually reappearing in these measurements? It is perfectly explained by supposing that light is a wave motion, and that the distance $d$ is *half the length of a wave.* The waves, which enter the slit at L in a certain phase, having to pursue different *lengths* of paths in order to reach the screen F G, arrive there in different phases. Where the lengths of path by which the waves reach the same part of the screen differ by an *odd* number of half wavelengths, the undulations arrive in opposite phases, and neutralize or extinguish each other; hence the dark bands. When this experiment is made with light of the various colours, it is found that the dark and bright bands are closer as the rays are more refrangible, that is, the value of $d$ is less as we pass from red to violet. The following table shows the lengths of the waves corresponding to the various colours, and the number of vibrations or undulations that must take place in a second.

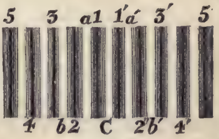


FIG. 205.

| Colours. | Number of waves in the length of one inch. | Number of undulations in one second. |
|---|---|---|
| Red . . . . | 40,960 | 514,000,000,000,000 |
| Orange . . . | 43,560 | 557,000,000,000,000 |
| Yellow . . . | 46,090 | 578,000,000,000,000 |
| Green . . . | 49,600 | 621,000,000,000,000 |
| Blue . . . | 53,470 | 670,000,000,000,000 |
| Indigo . . . | 56,560 | 709,000,000,000,000 |
| Violet . . . | 60,040 | 750,000,000,000,000 |

Here we have figures according with those which Newton deduced from the dimensions of his "rings" (page 224), although the conditions of experiment and methods of measurement are entirely different in the two cases. It is evident that the values of a periodicity of some kind belonging to light has really been ascertained by these measurements. In Newton's theory it was the periodicity of the *fits* of easy transmission and reflection; in the undulatory theory it is the periodicity of the *waves*, which is represented by the amazingly minute intervals. About the reality of the periodicity itself it is impossible to entertain any doubt, and when the known range of optical phenomena had not yet included polarization by reflection, and certain very curious effects discovered by Fresnel and by Arago, it might seem that the undulatory theory could not claim any very superior position. Both theories, it should be observed, are formed from the behaviour of *sensible masses of matter*, which is transferred to the supersensuous or ideal particles or molecules of light. In the emission theory these particles were supposed to be projected in straight lines with the velocity of light. Reflection was explained by the rebound of the light-particles, according to the same law that operates when a stone rebounds after striking a wall, or when the billiard-ball has the direction of its motion changed by impact with the cushion. Refraction was explained by the illustrious expounder of the corpuscular or emission theory by the aid of the conception of universal attraction, which had so largely occupied his mind. Newton, in fact, extended to the inconceivably minute particles of light the ideas of attractive forces which he had gathered from the motion of the planets. A ray of light passing through the air and falling obliquely upon the surface of water, is refracted, that is, the beam of light changes its direction, because in entering the liquid the atoms of the latter attract the light-particles: it can be shown by mathematical analysis that this would occasion the reflection of the ray. Or, to be more accurate in statement, the force which operates is the excess of the attraction between the light and water-particles over the attraction between the light and air-particles. A mathematical consequence of these suppositions is, that the light-particles must move *more rapidly* through the water than through the air. The undulatory theory, on the other hand, requires the light to pass through water *less rapidly* than through air. To any pair of media having the same relation in their refractive powers as the air and water here specially mentioned as examples, these considerations, of course, equally apply. Here, then, was a point upon which the rival theories were distinctly at issue; and a direct determination of the relative velocities of light in air and water would constitute a "*crucial instance.*" This crucial experiment was subsequently made, and the result is decisively in favour of the undulatory theory.

Huyghens, who was the first to propound the wave theory in a definite form, laid down certain general mathematical principles regard-

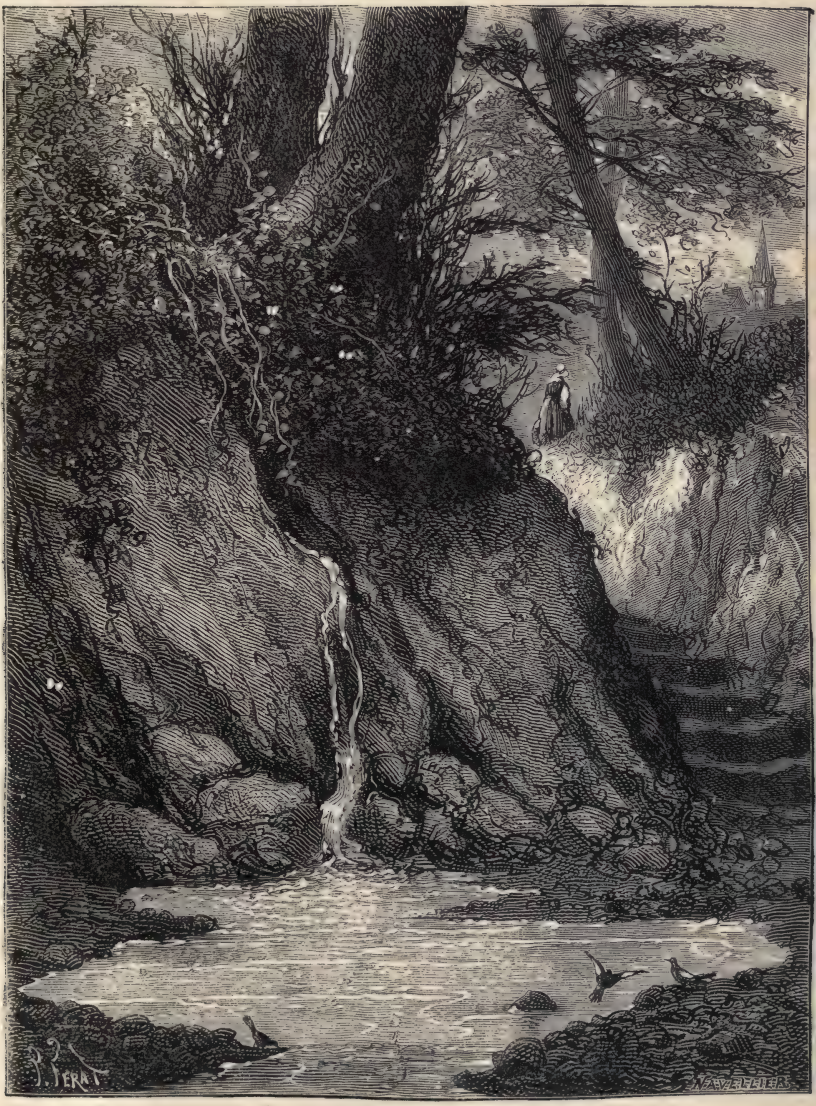

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PLATE XI.—THE SPRING.

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ing the propagation of undulations. He showed that in an elastic medium, uniform in all parts and directions, a disturbance set up at any point will expand in concentric spherical waves. To put the matter in another form and introduce a conception due to Huyghens, let us suppose that light emanates from a point in such a medium as that just spoken of. Taking any one point within that medium, the particles at that point will pass through all the successive phases in a certain very small portion of time. But let us consider any one instant of time, or suppose that the motion of the particles of the medium is arrested absolutely and instantaneously while we leisurely make in idea the following construction: taking any point of the medium, the particles are in some one phase, and there are other immediately contiguous particles also in that phase; and again, others contiguous to them in the same phase, and so on; a continuous surface might be drawn so as to contain all the series of these particles. In the case supposed, these surfaces would be spherical, and in all cases such surfaces are called *wave-fronts*. If we consider the wave-fronts at increasing distances from the centre of disturbance, it will be obvious that each very small portion of the wave-front becomes more nearly a plane. Each wave-front advances with the velocity of light, and every point of it may be considered as a centre from which secondary disturbances of the medium are being continually propagated. But these secondary disturbances are relatively very small, and from the manner of their production they tend to destroy each other's effects. Let us see Huyghens' principles applied in the construction by which reflection and refraction may be explained. Let A B, Fig. 206, represent a reflecting surface; C D is a wave-front, which at the point C we will suppose has at this instant reached A B. While the wave-front at D is advancing to G, a new wave-front has been advancing from C as a new

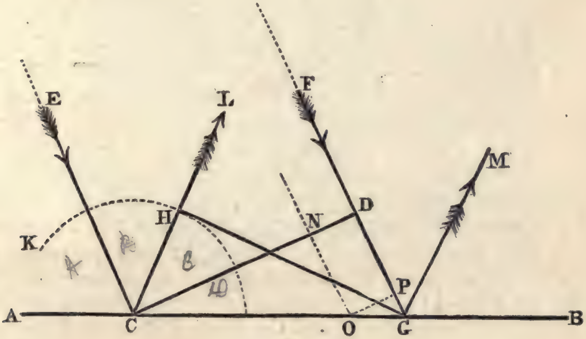

FIG. 206.

centre of disturbance; therefore draw from C as a centre, with radius = G D, the semicircle K H; and from G draw the tangent G H. The line G H is the position of the wave-front after reflection. Join C H and produce towards L, and from G draw G M at right angles to H G. C L and G M are the direction of the reflected ray, and very little geometry is required to prove that this result agrees with the experimental law of reflection—*i.e.*, the angles of incidence and reflection are equal. To show that this construction applies to every point in the wave-front, take any point in C D as N, draw N O parallel to F G, and O P parallel to N D. Then a circle drawn from O with radius = G P will just touch the line H G, and the reasoning which applies to points D and C will equally apply to P and O. Imagine that the same construction is made for every point in C D as for N, then will the semicircles all have H G for their common tangent; that is, the waves they represent will all concur by arriving at G H in one and the same phase, and G H is the only locus in which *all* the reflected waves will so concur.* The front of the reflected wave can therefore be no other than G H. We have here spoken of drawing semicircles, but properly speaking it is by *hemispherical surfaces* that the reflected wave-fronts would be defined. The semicircles in the diagram may be considered as sections of the hemispheres by a plane containing their centres, and there will be little difficulty in conceiving that in this series spherical surfaces can concur only in the plane H C G to produce a wave-front.

For the case in which undulations are supposed to pass from one medium to another, Huyghens' principle requires a construction similar to that given for refraction on page 227, to which the reader is requested to refer. Let us suppose that the shaded portion of Fig. 111 represents water, and that the space above C D is air. In the case supposed, the wave-surfaces are spherical; but in the majority of cases where light is refracted by a solid body, the velocity of propagation is *not uniform in all directions* within the body, and the form of the wave-front is determined accordingly. Such, at least, is the theory by which Huyghens was able to explain the phenomena of refraction presented by Iceland spar—phenomena since found, in fact, to obtain to some degree in all except one class of crystals. It will be readily seen on inspection, that, in Fig. 111, the angle B A B′ is equal to the angle of incidence ($i$), since each of these is the complement of angle B B′ A. Similarly the angle of refraction, or ($r$), = angle A B′$f$. Now (page 61) $\frac{BB'}{AB'} = \text{sine } i$, and $\frac{Af}{AB'} = \text{sine } r$; therefore, $\frac{\text{sine } i}{\text{sine } r} = \frac{BB'}{Af}$; but B B′ : A$f$ = velocity of light in air : velocity of light in water. It follows, therefore, that the index of refraction (page 156) between two media is the ratio of the velocities of light in these media. For instance, the index of refraction from air to water being as a matter of experimental fact $\frac{4}{3}$, or 1·333, it follows, if the undulatory theory be correct, that light moves with less velocity in water than in air in proportion as 3 is less than 4. Here we see that the undulatory theory perfectly accounts for the law of the sines in refraction.

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* If the reader has any difficulty in realizing this mentally, he is recommended to take a ruler and compasses, and actually draw on a piece of paper for a considerable number of points the construction indicated in the text.

We have postponed to this place an account of the discovery of *double refraction*, in order that the subject might be considered in the present connection. The facts are briefly these: A Danish physician published in 1669 an account of certain crystals found in Iceland, which possessed the property of causing all objects viewed through them to appear double. Fig. 207 shows the effect produced when a single line of print is placed behind a crystal of Iceland spar. It became evident when this property of Iceland spar was discovered, that the laws of refraction as stated by Snell (page 155), were not applicable to Iceland spar. Huyghens investigated the new case, as we have already stated on page 228, and he found that by a certain geometrical conception he was able to arrive at a very satisfactory explanation of the phenomena of *double refraction*, and to apply to them the formulæ of mathematics as rigorously as in the case of ordinary refraction. The hypothesis by which Huyghens explained the peculiarities of refraction in Iceland spar, supposed that a ray of light passing into the crystal is divided into two parts, one of which is capable of propagating its undulations in the crystal with equal velocity in every direction, so that the wave-fronts to which it gives rise are *spherical* at

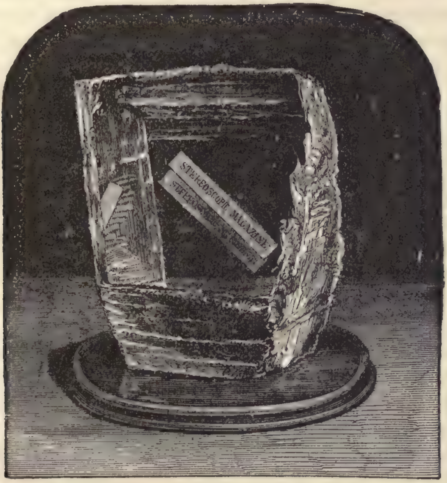

FIG. 207.—ICELAND SPAR SHOWING DOUBLE REFRACTION.

what point or in what direction soever it enters the crystal. The other or "*extraordinary*" portion of the ray, on the contrary, propagates undulations which move with less velocity in a certain direction than in any other. This direction is parallel to the line joining those two corners of an *equilateral* rhomb of Iceland spar which are formed by the union of *three obtuse* angles of the surface, and which, from the properties we are considering, is called its *optic axis*. The velocity of propagation in all directions perpendicular to the axis is uniform and at its maximum. It will, however, be quite unnecessary to here pursue further the geometry of double refraction in Iceland spar, which is not without a certain difficulty. Enough has been said to enable the reader to understand the importance of a confirmation of all its conclusions, which was effected by Wollaston when he determined the values of the indices of reflection in Iceland spar by an ingenious method of his own invention. What renders this confirmation the more striking is the fact that at the time Wollaston made his discovery he was unacquainted with Huyghens' hypothesis. We may infer from this how little ground the undulatory theory had gained among men of science in 1802.

The discovery by Malus of polarization by reflection and by transmission was a surprise to the scientific world, for polarization had until then been considered as merely the special property of two or three minerals. The generality of this property which the discovery of Malus revealed, and the certain allied phenomena first observed in the early years of the present century by Fresnel and Arago, demanded a theoretical explanation of a more comprehensive kind than the advocates of the Newtonian hypothesis were able to bring forward. Young, who had been attracted to the general undulatory theory of light by analogies he had observed between light and sound, had entertained the idea of *transverse* vibrations in rays of light, but hesitated to advance that hypothesis on account of some dynamical difficulty. This had reference to the mode in which a force could be conceived to produce, in such a medium as the supposed ether, vibrations transverse to the direction in which the ray is propagated. Fresnel, however, came to the conclusion that only by the supposition of transverse vibrations could certain phenomena, which he examined in conjunction with Arago, be explained in terms of the undulatory theory. Although Arago had been associated with Fresnel in the experimental investigations, the account of which was published under their joint names, the second part of the paper, where the hypothesis of transverse vibrations is first definitely proposed and justified, stands in the name of Fresnel alone, Arago shrinking from joining in the publication of so bold a conception. The difficulty of reconciling lateral vibrations with the force determining the direction of the ray will doubtless occur to most readers. It would hardly be possible to explain here the mechanical and mathematical principles on which the hypothesis is justified; but one or two familiar illustrations may serve both to fix the conception and to lessen its difficulty. A rope fixed at one end and held in the hand at the other, may by a jerk of the hand be thrown into a series of waves, which are the result of lateral vibrations of its parts, and these waves travel along the rope. Another illustration of lateral disturbance lineally transmitted is sometimes shown at lectures. A series of magnetic needles are arranged in the magnetic meridian, and when the pole of a permanent magnet of the same name as the outer pole of the last needle is brought near to the latter, the repulsion causes the needle to turn aside, and this movement is followed by the rest of the needles one after the other, on account of their mutual action. The action is progressive by passing along the line, and the movements of each needle are lateral only.

The undulatory hypothesis explains the phenomena of interference, refraction, etc., equally well whether the vibrations are conceived as taking place in the direction of the ray or transversely. Polarization is satisfactorily accounted for only on the latter hypothesis, but a certain relation between interference and polarization was discovered by Fresnel and Arago, and admitting undulations at all, it would appear impossible to explain the fact we here allude to otherwise than by the conception of *transverse* vibrations.

Fresnel applied his great mathematical power to the development of Young's theory of interferences, and to the more complete investigation of the phenomena of diffraction. When light enters a dark room through a *very small* aperture, the shadows of bodies in the room so lighted, instead of being bounded with a sharp edge, are surrounded by variously coloured bands. Also if the opaque body is narrow enough and its shadow is received at some distance, the interior of the shadow, where complete darkness would be expected, may be observed divided into equal spaces by alternate dark and bright bands, which last are, like those that surround the shadow, coloured. Fresnel fixed in his window-shutter a piece of copper foil, in which a very small hole was made, and upon this hole the rays of the sun were concentrated by a large lens placed outside of the window. Finding this arrangement inconvenient on account of the sun's motion, he replaced the lens by a mirror which reflected the solar rays in the required direction, and instead of the very small hole in the copper foil, a lens of a short focal length, half an inch or less, was placed in an opening in the shutter. In the focus of this lens the rays were concentrated in a point which became a source of light equivalent to a very small aperture. In his first trials Fresnel, not having at hand a glass lens of sufficiently short focus, improvised one by placing a drop of honey in a round hole in a thin plate of copper. A piece of iron wire illuminated by the light which passed through the honey-drop gave well-defined fringes in its shadow, permitting excellent observations and measurements to be made. At first Fresnel studied the fringes by receiving the shadow on a piece of ground glass, and when looking at these through a magnifying-glass, he observed that fringes were equally visible beyond the area of the piece of ground glass, and that, in fact, the interposition of this screen was useless, as the coloured fringes could be directly viewed through the magnifying-glass. The first use he made of this mode of direct observation was to trace the fringes to their origin: that is, as in approaching the wire the fringes and the intervals between them continually narrowed, the question was from what part of the opaque body did they originate. Fresnel traced the *exterior* bands quite up to the very margin of the opaque body, or, at least, the most powerful magnifying-glass failed to detect any interval. Newton's theory, which sought to explain these fringes by repulsive action exercised on the ray of light in passing near the opaque body, required an appreciable interval between the origin of the bands and the margin of the opaque body. That for the production of the bands *within* the shadow rays emanating from *both* margins were necessary, Fresnel proved conclusively by simply attaching a little piece of black paper to one or the other side of the wire, when the interior bands opposite to that part of the wire disappeared. It may easily be understood that by carefully measuring the actual intervals between the fringes in places at various known distances from the wire, the shape of the paths of the fringes could be determined. The intervals, small as they are, could be very accurately measured by a micrometer constructed on the same principle as the instrument used in telescopes (page 212).

In order to place clearly before the reader the manner in which Fresnel conceived the undulations to operate in producing diffractive phenomena, we reproduce in Fig. 208 the diagram which accompanies his paper. S is the radiant point, A and B the extreme points of the body producing the shadow. From the points S, A, and B as centres, series of circles are described, with radii increasing successively by the same amount, which is supposed to represent half a wave-length. The circles traced in plain lines may be taken to represent the *crests*, and those in dotted lines the *hollows* of the waves. [We use here the words *crests* and *hollows*, but the original has the terms which are applied to waves of sound.] The intersections of the two different kinds of circles are points where the undulations arrive in opposite phases, and are therefore points in the darkest bands. Lines traced through these points are *hyperbolas* (page 42), and the position of the dark bands is determined by the places in which the screen intersects these hyperbolas, F$'$ and $f'$, $gg'$. Again, the intersection of circles of the same kind will give the positions of bright bands, and these occupy the places of the hyperbolas L L$'$ and $ll'$. An inspection of the figure will explain one of the most notable facts in the experiment, namely, that the number of bands *within* the shadow is greater as the shadow is examined at positions nearer to the wire. It is also easy to explain by the undulatory theory the coloration of the fringes when ordinary light is used in this experiment; the rays of different colours being produced by luminous undulations of different lengths, the points of coincidence and of opposition are more or less separated

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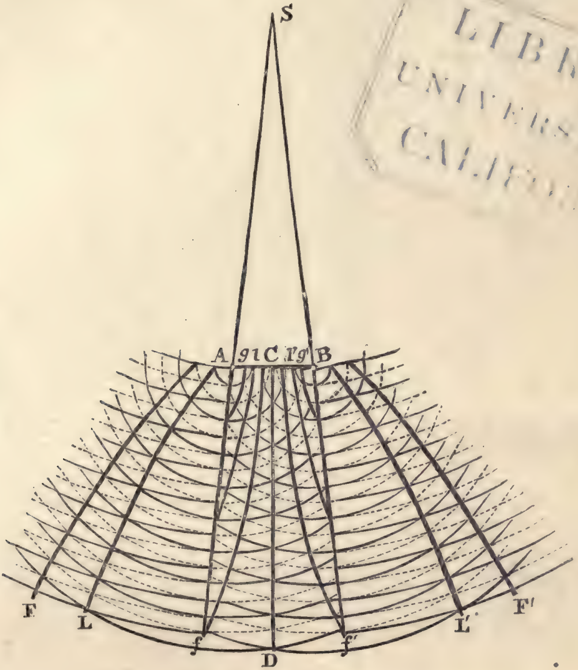

FIG. 208.

according to the length of the undulations. Thus if the distances between the circles in the diagram (Fig. 208) be taken as corresponding to the wave-lengths for violet light, other series of circles more widely separated will represent red light, and the hyperbolas F *f* would be found more widely separated.

The actual shadow of a body projected by a luminous point extends beyond the "geometrical shadow," that is, beyond the space which is bounded by lines drawn through the point tangential to the body. From this Fresnel concluded that the secondary waves arising at the margins of the body were retarded by half a wave-length, because if they started with the same phase as the original waves, there would be a perfect agreement between the phases of the direct and of those of the secondary undulations in the tangent plane, which, on the contrary, is in fact the position of the darkest part of a dark band. Such being the theory, it is perfectly easy for a geometrician to calculate the intervals which ought to be found between the bands at any given distances from the wire. It is indeed obvious by simple inspection of Fig. 208 that the position of any of the intersections of the circles is found when we know the radii of the circles, as their centres S, A, and B are fixed points. Now, for calculating the positions of the bands according to this theory, it is only necessary to measure the distances between the fixed points and to know the wave-length of the rays of light employed. The radii of the circles will be always multiples of the half-wave-length. Adopting the measures deduced by Newton from the phenomena of his rings (page 224) as representing the wave-length, Fresnel calculated what ought to be the distance of the dark bands within and without the geometrical shadow of the wire at certain positions where he had measured the actual distances between the bands. The close agreement between the calculated and the observed distances will be best shown by the tables below from Fresnel's paper. The light used was homogeneous, that is, of one colour, and was obtained by permitting sun-light to first pass through a particular kind of red glass. Had white light been admitted, the effects of interference would have manifested themselves not in the changes of mere intensity, which give rise to the dark and light bands, but in certain phenomena of coloration. It is obvious that only red rays can interfere with red rays, and that, for instance, green rays could never interfere with red, but only with green. The colours which arise from interference when white light is used, are due to the extinction of the complementary colour. Thus, for example, white light gives on a screen a green band in the place where red light would produce a dark band. The wave-length corresponding to the light transmitted by the red glass, deduced from Newton's results, was ·000623 millimetres. As the distances in Fresnel's table are given in terms of the metric system, we shall here insert a scale of the actual dimensions of a *decimetre* or one-tenth of a *metre*, with its division into *centimetres* and *millimetres*, Fig. 209.

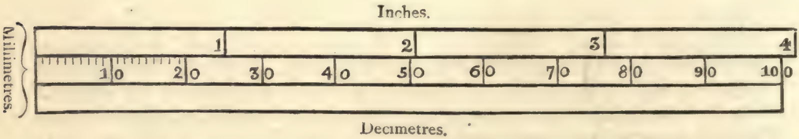

FIG. 209.

*Exterior Fringes produced by Homogeneous Red Light.*

| Distance of the wire from the luminous point. | Distance of the wire from the micrometer. | Order of the dark band. | Double the distance of the band from the boundary of the geometrical shadow. | | Differences. |
|---|---|---|---|---|---|
| | | | By measurement. | By calculation. | |
| *Metres.* | *Metres.* | | *Millimetres.* | *Millimetres.* | *Millimetres.* |
| 0·201 | 1·000 | 1st | 5·34 | 5·46 | −0·12 |
| ,, | ,, | 2nd | 7·69 | 7·72 | 0·03 |
| 0·997 | ,, | 1st | 2·99 | 3·16 | 0·17 |
| ,, | ,, | 2nd | 4·37 | 4·47 | 0·10 |
| 1·991 | ,, | 1st | 2·79 | 2·74 | +0·05 |
| ,, | ,, | 2nd | 3·83 | 3·87 | 0·04 |
| 3·971 | ,, | 1st | 2·38 | 2·50 | 0·12 |
| ,, | ,, | 2nd | 3·47 | 3·53 | 0·06 |
| 3·828 | 0·313 | 1st | 1·23 | 1·30 | 0·07 |
| ,, | ,, | 2nd | 1·83 | 1·84 | 0·01 |
| ,, | 1·192 | 1st | 2·64 | 2·79 | 0·15 |
| ,, | ,, | 2nd | 3·89 | 3·95 | 0·06 |
| 3·860 | 0·294 | 1st | 1·26 | 1·26 | 0·00 |
| ,, | ,, | 2nd | 1·77 | 1·78 | 0·01 |
| ,, | 1·125 | 1st | 2·59 | 2·69 | 0·10 |
| ,, | ,, | 2nd | 3·85 | 3·81 | +0·04 |
| 5·935 | 1·015 | 1st | 2·47 | 2·43 | 0·04 |
| ,, | ,, | 2nd | 3·45 | 3·44 | 0·01 |
| ,, | ,, | 3rd | 4·16 | 4·21 | −0·05 |
| ,, | ,, | 4th | 4·85 | 4·86 | 0·01 |

*Interior Bands produced by Homogeneous Red Light.*

| Distance of the luminous point from the wire. | Distance of the wire from the micrometer. | Diameter of the wire. | Number of intervals included in each measure. | Widths measured. | Widths calculated. | Differences. |
|---|---|---|---|---|---|---|
| *Metres.* | *Metres.* | *Millimetrs.* | | *Millimetrs.* | *Millimetrs.* | *Millimetrs.* |
| 1·430 | 0·546 | 0·76 | 1 | 0·45 | 0·45 | 0·00 |
| 1·430 | 0·546 | 1·01 | 3 | 0·98 | 1·01 | −0·03 |
| 5·950 | 0·546 | 1·01 | 3 | 0·98 | 1·01 | 0·03 |
| 1·447 | 1·093 | 1·56 | 3 | 1·30 | 1·31 | 0·01 |
| 1·447 | 1·093 | 2·56 | 7 | 1·90 | 1·86 | +0·04 |

The reader has now been made acquainted with one of the chief experimental bases of the undulatory theory of light, and a few of the actual measurements of the phenomena and of the results deduced from the theory alone have been been placed side by side, that the degree of correspondence may be seen. The measured positions in the first table differ from those calculated but very little, the greatest difference being with regard to the bands of the first order, that is, for the first bands F, F′, outside of the shadow. The reason is that these bands are broader than the others, etc., and therefore it is more difficult, in taking the measurement, to fix precisely upon the centre of the darkest part. A reason for the greater width of the first bands will be suggested by an inspection of the diagram, Fig. 208. With the same admirable clearness that characterizes his account of these experiments and deductions, Fresnel in the same paper applies the wave theory to explain how it happens that a small hole or the focus of a lens becomes practically a centre of luminous undulations. He even traces the undulations through the lens by which the light and dark fringes are viewed, and follows the waves in their progress into the eyeball itself, until they impress the retina.

In order that the reader may understand the reasons which led Fresnel to make *transverse* vibrations a part of the undulatory theory of light, it will be necessary to here describe particularly some of the phenomena of polarized light. There is a crystallized mineral called *tourmaline* found in various localities. Chemically it consists essentially of silica and alumina, together with small quantities of lime and other bases, including iron and manganese. Tourmalines occur of various colours, and in some kinds the polarizing property is more effective than in others. The mineral was first discovered in Ceylon in the sixteenth century, and has since been found in many other places. Fig. 210 shows at A and B the prismatic shapes of the tourmaline crystals. C represents such a crystal of tourmaline cut by means of a lapidary's wheel into slices parallel to the axis of the prism. The two inner slices, which may have each a thickness of about $\frac{1}{20}$th of an inch, form, when polished, a convenient means of exhibiting the simplest phenomena of polarized light. Indeed, the tourmaline plates were for a long time the only means at the command of experimentalists. If one of these plates be held vertically between the eye and a source of ordinary light—the flame of a candle, for instance—the light will be distinctly seen through it, tinged however with the colour of the tourmaline, which is usually greenish. In this respect it will make no difference whether the slice be held with the axis of the crystal upright, horizontal, or inclined,—the light will be seen equally well. Let us suppose, however, that this slice is fixed with the *axis vertical* between the spectator's eye and the light. Then let a second slice of tourmaline, either from the same or from another crystal, be held

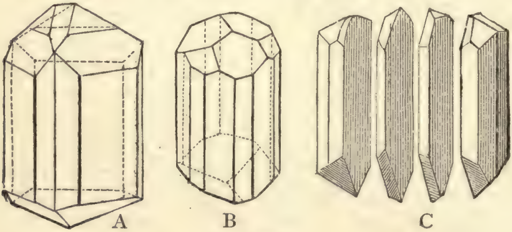

FIG. 210.

between the first slice and the eye. It is now no longer a matter of indifference as to the position of the axial direction. When the axes of the crystalline plates are both upright the light is seen through both crystals. If, however, the axis of the second piece is horizontal, the light which has passed through the first plate refuses altogether to pass through the second. In intermediate positions, more or less light will pass according as the axial directions of the crystals are more parallel or perpendicular to each other. It will be obvious that the light which has passed through the first tourmaline is no longer in the same condition as the light from the candle, for the rays have now different properties in *different directions.* It is impossible to conceive any theory of undulations consisting of movements in the direction of the ray only which will explain these phenomena ; and the hypothesis of Newton's, which attributed by a strained analogy polar attractions and repulsions to the particles of light, was entirely unsatisfactory. On the other hand, the theory of transverse vibration lends a ready explanation of the phenomena exhibited by the tourmaline plates. We must conceive the vibrations as taking place transversely to the direction in which the waves are propagated. This is visibly the case with the waves on water, but here the particles vibrate in vertical planes only, whereas in the undulations of the luminiferous ether the vibrations occur in planes passing in every direction through the ray. Thus, suppose a ray of light to enter a room and descend vertically : the vibrations are executed not only in planes north and south, or east and west, but in both those directions, and to the same degree in every other direction. Thus some particles will move north and south, others east and west, others north-east and south-west, and so on. Let us consider for the present only two planes of vibrations perpendicular to each other—as, for instance, those directed north-south and east-west. The motions of the ethereal particles which take place in other planes can, by the well-known propositions of the resolution of velocity and of forces, be referred to the two planes. Let N S and W E in Fig. 211 represent the directions of these planes, and A B the vibration of a particle in a plane inclined to N S and W E. From A draw the perpendiculars A*n*, A*e*, and from B the perpendiculars B*w*, B*s*. Then *n s* and *w e* will represent the mechanical effects of that vibration in the planes N S and W E. Now, as the ordinary principles of mechanics are applied to the motions of the ether particles, it follows that, if from some cause the particles should at some point in a ray of light be compelled to confine their vibrations to only the two perpendicular planes N S and W E, the vibrations before executed in planes inclined to these would exercise their mechanical effects by the *components n s*, *w e*, resolved in these two

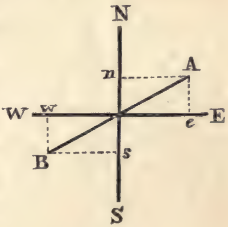

FIG. 211.

directions. Now, the fact of the (plane) polarization of light is regarded by the theory as consisting simply of an actual resolution of all the transversal vibrations into two perpendicular planes. It is easy to rudely represent by models and diagrams how such effects occur in polarization; and, as we are about to place before the reader a representation of a model intended to illustrate the experiment of the tourmalines, we must guard him against supposing that in *ordinary* light the vibrations are confined to any particular planes. On the contrary, it is precisely in this circumstance that polarization consists. Fig. 212 therefore, as a model of a ray of light, would properly represent a *polarized* ray; but, if we can suppose that by a kind of anticipation the vibrations in the ordinary ray are already referred to the two perpendicular planes into which the polarization really separates them, we may illustrate by models some of the effects of polarization. Fig. 212 represents a model of the effects of the tourmaline plates. A is the first plate upon which falls the beam of ordinary light B A. Only the vibrations executed in the vertical plane pass on; those in the horizontal plane are destroyed—that is, their effect is in some way quenched or absorbed by the tourmaline. When the ray which has passed the first plate falls upon the second plate, it passes through that also if the axes of the crystalline plates are parallel; if they are perpendicular to each other, as represented by the model, the ray does not pass. This effect is indicated in the model by representing the plates as *gratings*, which, according as they are placed in a vertical or in a horizontal position, would allow or prevent one plane or the other to pass through.

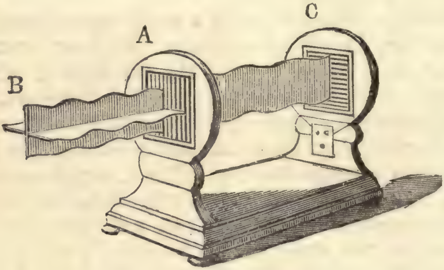

FIG. 212.

The discovery of double refraction in Iceland spar has already been mentioned, and the theory of that effect explained (page 461). When the two emergent rays into which a beam of ordinary light is divided by Iceland spar are examined by a slice of tourmaline, it is at once found that these two rays are polarized in perpendicular directions. When the tourmaline is in a certain position the ordinary ray will not pass through it, while the extraordinary ray is transmitted with maximum brightness. When the tourmaline is turned 90° from this position the results are reversed,—the extraordinary ray is completely quenched, and the ordinary ray passes freely. In the hands of an ingenious optician named Nicol, the polarizing property of Iceland spar was made available as a far better means of studying the phenomena than that afforded by the tourmaline. By availing himself of the difference of the refractive indices of the two rays in the spar, he was able to turn one of these rays out of the field, so that the properties of polarized light might be studied in the other without intermixture of effects. He accomplished this by taking a rhomb of the spar, and cutting it obliquely in a certain direction, and after polishing the two cut surfaces, cementing them together by a layer of *Canada balsam*. The refractive index of Iceland spar for the *ordinary* ray is greater than that of Canada balsam, but for the *extraordinary* ray it is less. Hence, if the ordinary ray meets the Canada balsam at a sufficiently great angle of incidence, total reflection occurs. The section of the crystal being made so as to obtain this incidence, the ordinary ray is reflected to one side, as shown in Fig. 213, where it is seen emerging from the crystal at O. But the extraordinary ray, passing from a less to a more refractive substance, cannot be totally reflected, and it is in great part transmitted, emerging from the rhomb, as shown at E, completely polarized. The rhomb of Iceland spar thus treated is known as "Nicol's Prism," and its invention enabled many beautiful chromatic effects of polarized light to be fully studied, as Iceland spar is itself colourless and perfectly transparent.

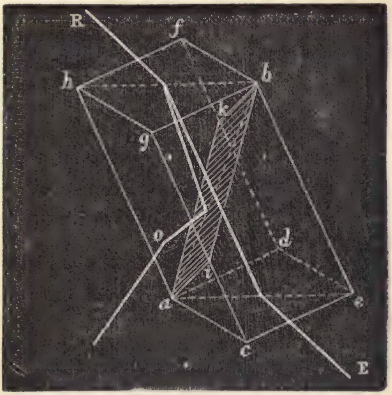

FIG. 213.

When the light polarized by reflection from transparent substances is examined by Iceland spar (Malus' discovery, p. 452), or by a tourmaline, or by Nicol's prism, the phenomena observed correspond with those already described. For instance, if we place a plate of glass in an upright position, and so that a beam of light falls upon it at an angle of 32° with the surface, on interposing a Nichol's prism in the path of the reflected beam between the eye and the glass plate we shall find that in certain positions of the crystals the reflected beam is wholly quenched, while in positions 90° to the former the light is wholly transmitted. In the case supposed the beam will pass the tourmaline when the axis of the crystal is vertical; but when the axis is horizontal it will be completely extinguished. Conversely, when the tourmaline is viewed by reflection from the glass, it will be seen of its true colour when held vertically, but held horizontally it will appear as opaque as a piece of metal. If we examine the light which passes through the plate of glass, it will be seen through the tourmaline in all positions, but brightest when the axis is horizontal, darkest when it is vertical. This shows that some of the light is polarized, and, be it particularly observed, this polarization is in a *plane perpendicular to that in which the reflected beam is polarized.* By placing several plates of glass in contact with the first we can, as already observed, obtain the polarization of practically the whole of the transmitted light. The reader will presently see the use made of these facts by Fresnel in an experiment which may be termed crucial, for it decided the adoption of transverse vibration into the theory of light. Models have been constructed to illustrate the perpendicularity of the two planes of polarization in the refracted and transmitted beams. In Fig. 214 B may be supposed (with the differences already pointed out) to represent a beam of ordinary light incident upon a bundle of upright glass plates. The shaded vertical plane C indicates the direction of the vibrations in the reflected beam; the unshaded plane D, in which the vibrations are horizontal, represents the transmitted beam. Before describing those experiments of Fresnel and Arago which determined the adoption of the theory of *transverse* vibrations, we place before the reader a diagram (Fig. 215) to further illustrate the application of that theory to the discovery of Malus. Let A B C D be a rectangular plate of glass, and S O a beam falling upon it at the angle of 32°. We shall suppose the edges A B and C D to be horizontal, and directed north-south, and the plane of incidence—*i.e.*, the plane containing the incident and reflected rays—to be east-west, the reflected ray O O′ being vertical. The vibrations in the reflected ray will then be confined to a vertical plane directed north-south, that is, the motions of the ethereal particles, as shown by the small arrows, will now be parallel to the surface of the glass. Let this reflected polarized ray be received upon another rectangular plate of glass E F G H, the edges of which, E F and G H, must

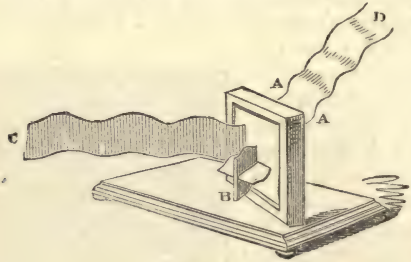

FIG. 214.

be supposed always horizontal. So long as the edges E F and G H are directed north-south the polarized beam will be reflected from the upper glass plate, even when the beam falls upon it at the polarizing angle. When, however, the upper plate forms an angle of 32° with the beam, and is so turned that its upper and lower edges, E F and G H, are directed east-west, it absolutely refuses to reflect the polarized beam. On turning the upper plate round O O′ as an axis, while its inclination to O O′ is always 32°, there are two positions in which the reflected light is at its maximum, namely, E F G H and E″ F″ G″ H″, and two at which it is totally abolished, namely, E′ F′ G′ H′ and E‴ F‴ G‴ H‴. At intermediate positions the amount of light reflected varies from one to the other extreme.

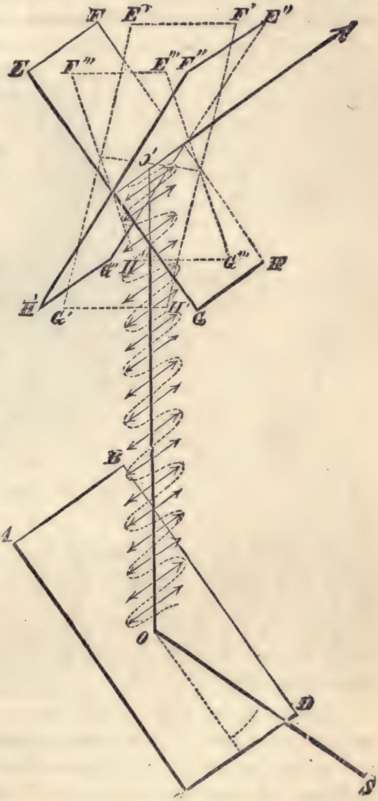

FIG. 215.

The experiments of Arago and Fresnel were designed to test the properties of polarized light as regards interference. We have already seen the genius of Fresnel in the application of the undulatory theory to the facts of diffraction and interference phenomena (pages 455 and 464). We select for description an experiment contrived by Arago, because it is the most direct and convincing. Two very fine parallel slits were made in a thin sheet of copper close to each other. A bundle of fifteen thin laminæ of mica was cut in two, and of these it is obvious that in the parts which were contiguous before separation the thickness must have been very nearly the same. These bundles polarized almost completely light which fell upon them at an angle of 30° to the surface, and one of them was placed at this angle of inclination before each of the slits in the copper plate, these slits being illuminated by light from a point or single slit. When the two bundles of plates were inclined in the same direction, the bands formed by the interference of the polarized rays were distinctly seen, exactly as when two rays of ordinary light act upon each other. But if one of the bundles was turned round the incident ray so that the planes of polarization of the light from the two slits were perpendicular to each other—as, for example, when one bundle was inclined from above downwards, and the other from right to left—then no interference bands whatever were produced. These following results were thus conclusively established:

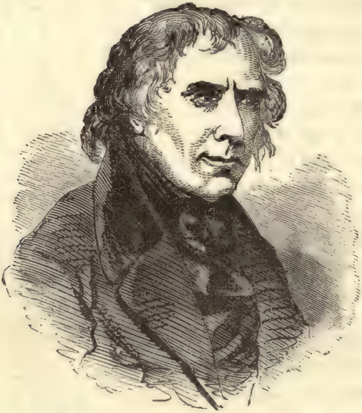

FIG. 216.—FRANÇOIS ARAGO.

1st. *Two rays of light polarized in the same direction act upon each other like rays of ordinary light, the phenomena being absolutely the same in both cases.*

2nd. *In the same circumstances under which two rays of ordinary light appear to destroy each other, two rays polarized in perpendicular directions have on each other no appreciable action.*

FRANÇOIS ARAGO (1786—1853), the distinguished French *savant* whose name has been so often mentioned in this chapter, was another of those pupils of the Ecole Polytechnique who made that college famous by their brilliant scientific careers. Arago succeeded Monge in the professorship of geometrical analysis, and he became eminent not only as a mathematician and astronomer, but also as a physicist. Many of his discoveries and researches in connection with light and with electricity are of great interest and importance. He published a popular treatise on astronomy, and other writings which are highly esteemed.

We have now described at length some of the phenomena upon which the undulatory theory of light depends, and throughout we have been more anxious that the reader should clearly realize a few of the more important facts and the inferences from them, than that he should be presented with necessarily imperfect accounts of every discovery made by Fresnel, Arago, Biot, Brewster, and others. The phenomena presented by polarized light are extremely interesting and varied, and it is the glory of the undulatory theory to supply a consistent explanation of facts so numerous and diverse. To fully exhibit the correspondence between these facts and the theory would require the use of mathematics of an order far beyond the due range of a popular work. The power and fertility of the undulatory theory cannot be more strikingly illustrated than by the verification of an *à priori* and highly elaborate deduction from the theory, by direct observation of the corresponding previously unsuspected fact. Professor Tyndall, in his "Notes on Light," has so felicitously compared the discovery alluded to with the discovery of Neptune (page 423), that we take the liberty of quoting the passage.

"You regard, and justly so, the discovery of Neptune as a triumph of theory. Guided by it, Adams and Leverrier calculated the position of a planetary mass competent to produce the disturbances of Uranus. Leverrier communicated the result of his calculation to Galle of Berlin, and that same night Galle pointed the telescope of the Berlin Observatory to the portion of the heavens indicated by Leverrier, and found there a planet 36,000 miles in diameter. It so happens that the undulatory theory has also its Neptune. Fresnel had determined the mathematical expression for the wave-surface in crystals possessing two optic axes; but he did not appear to have an idea of any refraction in such crystals other than double refraction. While the subject was in this condition, the late Sir William Hamilton, a profound mathematician, took it up, and proved the theory to lead to the conclusion that at four special points of the wave-surface the ray was divided not into *two* parts but into an *infinite number of parts*, forming at those points a continuous *conical envelope* instead of two images. No human eye had ever seen this envelope when Sir William Hamilton inferred its existence. If the theory of gravitation be true, said Leverrier in effect to Dr. Galle, a planet ought to be there; if the theory of undulation be true, said Sir William Hamilton to Dr. Lloyd, my luminous envelope ought to be there. Lloyd took a crystal of arragonite, and following with the most scrupulous exactness the indications of theory, discovered the envelope which had previously been an idea in the mind of the mathematician. Whatever may be the strength which the theory of gravitation derives from the discovery of Neptune, it is matched by the strength which the undulatory theory derives from the discovery of *conical refraction*."

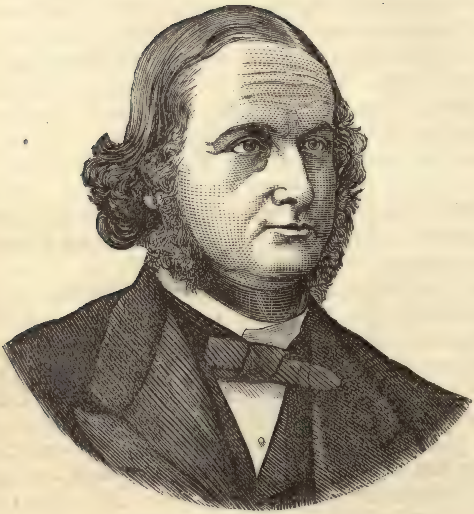

FIG. 217.—PROFESSOR KIRCHOFF.

# CHAPTER XVIII.

## PHYSICS OF THE NINETEENTH CENTURY—SPECTROSCOPY.

THE starting-point of the history of our present subject may be found in the memorable experiment in which Newton resolved ordinary sunlight into its constituent coloured rays. The experiment which belongs to the seventeenth century, has been described in a previous chapter (page 217), and to this the reader is recommended to refer. The next step in the course of discovery was not taken until the opening of the present century, when Wollaston, instead of allowing the light to enter the dark chamber through a round hole, made use of a narrow slit parallel to which the refracting edge of the prism was placed, as shown in Fig. 218. Newton had, in fact, viewed, not one spectrum, but an indefinite series of spectra partially overlapping but not coinciding with each other. It will be observed that as an extremely narrow beam of light, such as will pass through a slit not more than $\frac{1}{40}$th of an inch wide, suffices to produce a brilliant spectrum, the round hole in the shutter, a quarter of an inch in diameter, as used by Newton, may be considered as made up of a series of such slits, each of which would give rise to its own spectrum. Hence Wollaston was the first to observe certain phenomena which were lost in Newton's form of the experiment. Wollaston had no idea of the important results which were afterwards to be deduced from the phenomena he was the first to discover; indeed, the discovery seems to be mentioned only incidentally in a paper of his relating to another subject. We shall give *verbatim* those passages in the paper which belong to our present subject; but before doing so we devote a short paragraph to the personal history of a man whose name deserves to be remembered in connection with this subject and for his eminent services to science generally.

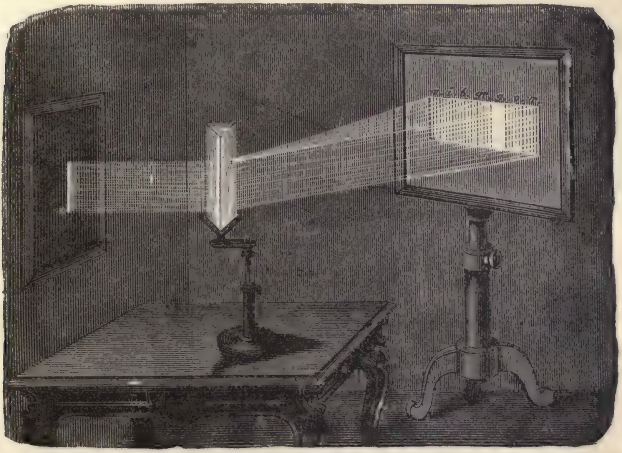

FIG. 218.

WILLIAM HYDE WOLLASTON (1787—1826) was educated for the medical profession, and took his M.D. degree at Cambridge. He endeavoured to establish himself in medical practice—first at Bury St. Edmunds, afterwards in London—but meeting with less success than he expected, he abandoned the profession altogether, and devoted himself to the study of chemistry and natural philosophy. He directed much attention to the application of science in the arts, and some patents he had secured for his inventions proved so lucrative a source of income that he amassed a large fortune. He will be remembered as the first to isolate the metal platinum from its native mineral, and to devise a method of rendering it malleable. Wollaston as an experimenter was remarkable for working with very small quantities of his materials. He became Secretary to the Royal Society in 1806, and shortly before his death he received the gold medal of the Society for his platinum process. He bequeathed his wealth to objects calculated to promote science.

The paper which brings Wollaston's name into our present chapter appeared in the "Philosophical Transactions" for 1802. Its title imports that it describes a new method for measuring refractive powers by determining the angle of total reflection, and the following passages are therefore merely incidental to its main subject.

"I cannot conclude these observations on dispersion without remarking that the colours into which a beam of white light is separable by refraction, appear to me to be neither seven, as they are usually seen in the rainbow, nor reducible by any means (that I can find) to three, as some persons have conceived; but that by employing a very narrow pencil of light, four primary divisions of the prismatic spectrum may be seen with a degree of distinctness that, I believe, has not been described or observed before.

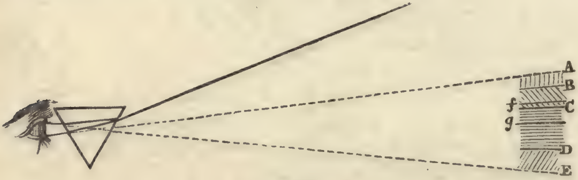

FIG. 219.

"If a beam of daylight be admitted into a dark room by a crevice $\frac{1}{20}$th of an inch broad, and received by the eye at the distance of ten or twelve feet through a prism of flint-glass, free from veins, held near the eye, the beam is seen to be separated into the four following colours only: red, yellowish green, blue, and violet, in the proportions represented in Fig. 219.

"The line A that bounds the red side of the spectrum is somewhat confused, which seems in part owing to want of power in the eye to converge red light. The line B, between red and green, in a certain position of the prism is perfectly distinct; so also are D and E, the two limits of violet. But C, the limit of green and blue, is not so clearly marked as the rest; and there are also, on each side of this limit, other distinct dark lines, *f* and *g*, either of which in an imperfect experiment might be mistaken for the boundary of these colours.

"The position of the prism in which the colours are most clearly divided is when the incident light makes about equal angles with its two sides. I then found that the spaces A B, B C, C D, D E, occupied by them, were nearly as the numbers 16, 23, 36, 25. . . . .

"By candlelight a different set of appearances may be distinguished.

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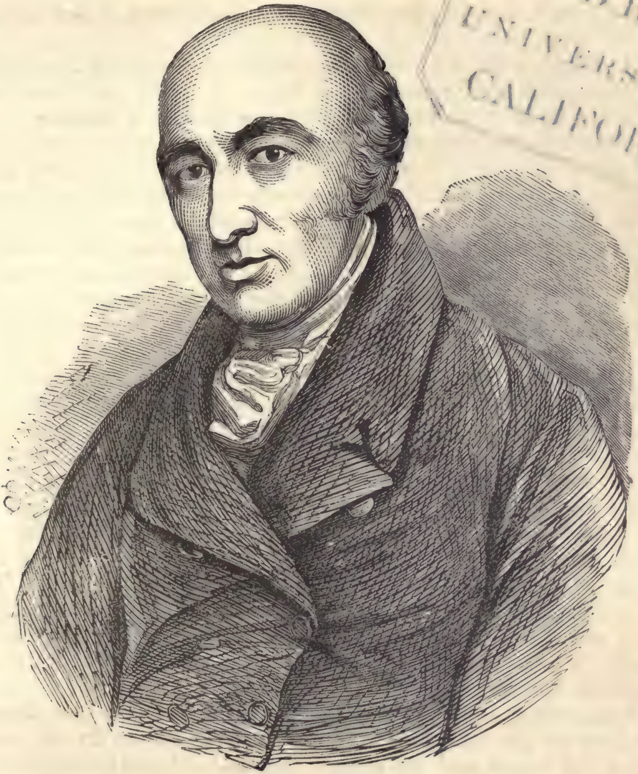

FIG. 220.—WILLIAM WOLLASTON.

When a very narrow line of the blue light at the lower part of the flame is examined alone, in the same manner, through a prism, the spectrum, instead of appearing a series of lights of different hues contiguous, may be seen divided into five images at a distance from each other. The first is broad red, terminated by a bright line of yellow; the second and third are both green; the fourth and fifth are blue, the last of which appears to correspond with the divisions of blue and violet in the solar spectrum, or the line D of Fig. 219.

"When the object viewed is a blue line of electric light, I have found the spectrum to be also separated into several images; but the phenomena are somewhat different from the preceding. It is, however, needless to describe minutely appearances which vary according to the brilliancy of the light, and which I cannot undertake to explain."

In a note to a passage relating to the proportions of the colours in the spectrum, he adds:

"Although what I have above described comprises the whole of the prismatic spectrum that can be rendered visible, there also passes on each side of it other rays whereof the eye is not sensible. From Dr. Herschel's experiments we learn that on one side there are invisible rays occasioning heat that are less refrangible than red light, and on the other I have myself observed, and the same remark has been made by Mr. Ritter, that there are likewise invisible rays of another kind that are more refracted than the violet. It is by their chemical effects alone that the existence of these can be discovered, and by far the most delicate test of their presence is the white muriate of silver. To Scheele, among many valuable discoveries, we are indebted for having first duly distinguished between radiant heat and light, and to him also we owe the observation, that when muriate of silver is exposed to the common prismatic spectrum, it is blackened more in the violet than in any other kind of light. In repeating this experiment, I have found that the blackness extended not only beyond the space occupied by the violet, but to an equal degree and to about an equal distance beyond the visible spectrum; and that by narrowing the pencil of light received on the prism, the discoloration may be made to fall almost entirely beyond the violet. It would appear, therefore, that this and other effects usually attributed to light are not owing to any of the rays usually perceived, but to invisible rays that accompany them; and that if we include the two kinds that are invisible, we may distinguish upon the whole six species of rays into which a sunbeam is divisible by refraction."

These extracts show that Wollaston was the first who saw either the dark lines in the solar spectrum, or the bright lines in the spectra yielded by flames and by electric sparks. He did not, however, pursue the subject, nor indeed does any notice appear to have been taken of his observations. Some years afterwards, however, the dark lines of the solar spectrum were observed under better conditions by the German optician Fraunhofer, a native of Bavaria (1787—1826). Fraunhofer appears to have been unacquainted with Wollaston's descriptions of the dark and light lines of spectra; and as he therefore not only discovered the dark solar lines for himself, but was the first to study them carefully, they have been very properly called *Fraunhofer's Lines*. His method of observation was somewhat different from that of Wollaston; for, instead of looking through the prism with the naked eye, he made use of a telescope, which, with the prism, was placed at a distance of 24 feet from a very narrow slit. Not only was his method of observation better than that of his predecessor, but his appliances were of an excellence which could not previously have been equalled. His prism, for example, was not only wrought with great nicety, but was made of the famous glass specially prepared for optical purposes at Munich. He thus was able to observe a very great number of fine dark lines crossing the spectrum transversely, and in 1814 he published an account of these lines with a carefully drawn map, in which the positions of 354 lines were laid down; but he counted altogether no fewer than 576 in the solar spectrum. These lines are of different intensities and breadths, some of them appearing as delicate as a spider's line does to the unassisted eye, while others are comparatively strongly marked. The most conspicuous lines (or groups of lines) are indicated in Fraunhofer's map by the letters A, B, C, D, E, F, G, H, and by these letters the lines are still designated, as they constitute as many fixed points, to which it is often convenient to refer the position in the spectrum of other lines; but for accurate purposes, any given line is now generally indicated by a number representing the *wave-length* (page 457) of the corresponding ray.

Fraunhofer ascertained that these lines were always produced in identically the same position by sunlight, whether by the rays direct or reflected from the moon or the planets. But when he examined the light of fixed stars, he found that though these gave spectra containing dark lines, the lines were very different in their number, order, and position from those in the solar spectrum. Hence he concluded that the dark lines, whatever their cause, did not depend upon our atmosphere, but upon something special in the light of our own and other suns. This observation may be considered the starting-point of perhaps one of the most interesting and extraordinary series of researches that belong to the present century.

The course of discovery leads us now to the mention of certain researches in coloured flames by Brewster, Fox-Talbot, and Sir John Herschel. In 1822 Brewster proposed the use of spirit-lamps with salted wicks as a means of obtaining monochromatic light for optical experiments. The fact of compounds of sodium yielding a light containing only yellow rays had, in fact, been announced long before Brewster's time. About the same time Sir John Herschel examined by the prism the lights given off by flames containing severally chloride of strontium, chloride of copper, nitrate of copper, boracic acid, and chloride of potassium. The first of these substances introduced into the flame of a spirit-lamp causes an intense red coloration, the next three tinge the flame green, and the last one makes it violet coloured. In the article on Light in the "*Encyclopedia Metropolitana*" in 1827, Herschel mentions the colours imparted to flames by compounds of lime, strontia, lithia, baryta, and copper; and he says that the colours may readily be produced by placing salts of these substances, but preferably the chlorides, in powder in the wick of a spirit-lamp. He offers a distinct suggestion of the employment of flame colorations in chemical analysis: "The colours thus communicated by the different bases to flame afford in many cases a ready and neat way of detecting extremely minute quantities of them."

Fox-Talbot, whose name will always be remembered in connection with the beautiful art of photography, in a paper published in 1826 records the results of prismatic examinations of flames, and proposes the use of the prism in detecting the presence of certain substances. "The flame of sulphur and nitre," he says, "contains a red ray of a definite refrangibility, and apparently characteristic of potash salts, as the yellow ray is of soda salts. The red light, however, is not visible in the flame viewed by the naked eye, on account of feeble illuminating power, but is detected by the prism." He suggests this principle: whenever the prism shows a homogeneous ray of any colour to exist in a flame, the ray indicates the presence of a definite chemical compound. Then, after describing the spectrum of a flame coloured by strontia, he makes the observation (which has since been completely realized): "If this opinion should be correct, a glance at the prismatic spectrum of a flame may show it to contain substances which it would otherwise require a laborious chemical analysis to detect." Some years afterwards (1834), speaking of the spectra of lithium and of strontium, which impart to flames intense red colours not to be distinguished from one another by the unaided eye, he is more positive: "Hence I hesitate not to say that optical analysis can distinguish the minutest portions of these substances from each other with as much certainty as any other known method, if not with more."

By this we find that the principle of spectral analysis was clearly enunciated as regards certain substances. The extreme minuteness of the quantity of sodium compounds which suffices to give the intense yellow line was unknown to these early observers, and the constant intrusion of the sodium line in cases where no sodium was thought to be present, was a source of much perplexity. Fox-Talbot was inclined to attribute this line to the presence of water, which he supposed to be the only substance that could possibly be present in the various salts, all of which yielded spectra containing the yellow ray, whatever other lines were also produced. But he found the yellow line showing itself in the spectrum of burning sulphur, which could not be supposed to contain water. We now know that sodium compounds are most widely diffused, and that a trace of them, which would be beyond the ordinary means of detection, even if increased a thousandfold, suffices for the production of the yellow ray.

Wollaston, as we have already seen, observed spectra in which lines of three classes were represented: he saw first the dark lines in the sun's light, which are often called after Fraunhofer; second, the bright lines produced by a coloured flame; and third, the bright lines of the electric spark. All these classes of lines have since been the subjects of much research, and the germ of some of the most important discoveries which have been made in spectroscopy was contained in an observation of Sir David Brewster's, by which a fourth class of lines was brought to notice in 1832. Brewster found that the brownish vapours of nitrous acid had a remarkable effect on the spectrum. When the sun's light was passed in its course to the prism through a glass vessel containing these vapours, the spectrum was seen to be crossed by a great number of dark bands, which were quite independent of the Fraunhofer lines, for as was afterwards shown by W. H. Miller and Professor Daniell, they were also produced with other sources of light; for instance, with the bright part of a candle-flame, which, but for the interposition of the nitrous acid vapours, would have given a *continuous* spectrum. By the *continuous spectrum* we must understand one without any lines, either dark or bright, but showing the various prismatic colours graduating one into the other. The appearance presented by the spectrum of these nitrous acid vapours is re-

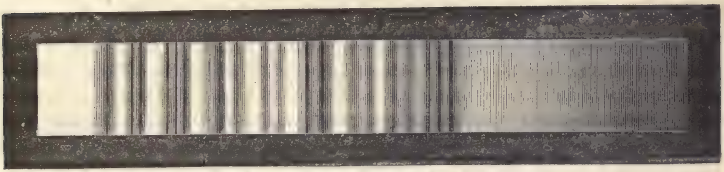

FIG. 221.

presented in Fig. 221. In the course of his investigations Brewster observed that at times certain dark lines were visible in this spectrum which on other occasions ceased to appear. He soon found that these lines appeared when the observation was made with the sun low or near the horizon, and that they ceased to be visible when the sun had attained a certain elevation. He was therefore led to attribute them to the action of our atmosphere upon the solar rays, an action which appeared to bear the closest analogy with that which was exercised by the brown-red vapour of the nitrous acid, namely, a power of stopping or absorbing certain of the solar rays. The dark lines which result from this property of gases or vapour were henceforth known as *absorption bands* or *absorption lines*, and we shall presently see the important part they play in the development of the science.

Brewster's experiments were followed up by others instituted by W. H. Miller and Daniell, who very shortly afterwards showed that other coloured vapours also give absorption bands. They ascertained that this was the case with the vapours of bromine and iodine, and with a certain yellow-coloured gaseous compound of chlorine and oxygen. The lines, however, given by each substance are entirely different. They appear to have no relation to the *colour* of the gas or vapour. Thus bromine and nitrous acid vapours, although they have almost identically the same colour, give quite different sets of absorption bands in their spectra; and again, the red vapours of chloride of tungsten, which have the same appearance, give, as W. H. Miller discovered in 1845, no absorption lines whatever. The absorption lines are seen in the vapour of some chemical elements and not in that of others. Compound bodies in the state of vapour may exhibit absorption lines, while their constituents do not, or *vice versâ.*

Among the other observations made by Fraunhofer in 1814 was one the significance of which was not understood until many years afterwards. He found that the conspicuous dark line of the solar spectrum, designated by the letter D, was in reality formed by *two* lines very close to each other; and he noticed—and this is the fact which was afterwards to become the starting-point of a new path of inquiry—that these two dark D lines were identical in their position in the spectrum with two bright lines which were seen in ordinary flames. These lines having since been proved to be due to the presence of *sodium*, are now often called the sodium lines. Again, in 1849, Foucault, while viewing these bright sodium lines as displayed in the rays from the electric light, caused the concentrated rays of the sun to traverse the electric arc, and he observed that with this compound light the dark sodium line of the solar spectrum appeared more intense than usual. He drew no general inference from this fact, and although Professors Stokes and W. Thompson had in the meantime pointed out that these and other facts pointed to certain general principles, it was reserved for two eminent German men of science to investigate the relations between the bright and the dark lines, and to propound a general theory of these phenomena. Professor Kirchhoff, of Heidelberg, desired to put to the most direct test the coincidence of the bright sodium lines with the dark Fraunhofer D lines of the solar spectrum. When he had obtained a moderately bright spectrum without admitting the sunlight directly into his spectroscope, he brought a flame coloured by a sodium compound in front of the slit of the spectroscope. He then saw the bright sodium lines due to the flame appear in the very same places that had been occupied by the D lines. Next he allowed the direct rays of the sun to enter the slit after passing through the sodium-flame, and he was surprised at the great intensity of the black D line which then appeared. Continuing the inquiry, he now began to vary the condition of the experiment. The sunlight was replaced by the oxy-hydrogen limelight, which, like every incandescent solid, furnishes a spectrum perfectly continuous, that is, without either bright or dark lines. When the light from the incandescent lime was received into the spectroscope after passing through a flame coloured by common salt, two *dark lines* were seen in the positions of the sodium lines. In this last experiment it is plain that the rays which in the absence of the limelight produced the bright sodium lines, also entered the slit of the spectroscope along with the much more intensely luminous rays from the limelight, and the appearance of darkness arose from the more intense illumination of the limelight spectrum. Further, it is obvious that, in the light from the incandescent lime, those rays that would have otherwise have made the D spaces as luminous as the rest of the spectrum, were stopped or absorbed by the sodium-coloured flame. In other words, from the limelight there passed through the flame rays of every degree of refrangibility, except only rays corresponding with the two D lines, to which rays the sodium vapour was completely opaque. Kirchhoff likewise ascertained that when the flame was coloured with potassium compounds instead of sodium compounds, there appeared, in the spectrum of the limelight behind the flame, dark lines corresponding exactly with the bright lines which were seen when the flame alone was viewed by the spectroscope.

These and other facts of the like kind were generalized by Kirchhoff into a conclusion which may be thus stated: *When any substance rendered luminous by heat emits rays of a certain definite refrangibility, the substance has the power of absorbing at the same temperature rays of that identical refrangibility.* The like theoretical law applying to rays of heat had before this been proposed by Prévost, and by Provostaye and Desains, and this law was fully elucidated by Balfour Stewart very shortly before Kirchhoff announced that it holds good for light also. The importance of Kirchhoff's generalization will appear more clearly when we have shown the remarkable explanation of the Fraunhofer lines which he deduced from it. As the researches connected with spectroscopy became, from about the period we have now reached, spread over several ever-widening fields of inquiry, it will be more convenient to indicate the chief landmarks of discovery in each province of spectroscopic investigation, rather than to follow the chronological order and be passing continually from one branch of the subject to another. We have brought the reader to the period at which Kirchhoff clearly announced the relations that exist between the dark and the bright lines of spectra. But for the present we shall consider the progress of spectroscopy as a means of chemical analysis. The application of the spectroscope was suggested, as we have already seen, by Fox-Talbot and by Sir John Herschel, and some spectra of coloured flames were described by Professor W. A. Miller in 1845.

In 1857 Professor Swan, published an elaborate research "On the Spectra of the Flames of the Hydro-carbons." The first recorded observation of line spectra includes, as we have seen, a reference to a hydro-carbon spectrum (page 479). Swan had the advantage of using in his experiments the now well-known Bunsen gas-lamp, which provides the best means for the examination of flame spectra generally. He observed the constant occurrence of the yellow (D) line in the spectra, and he was the first who proved that this line is due solely to sodium compounds. He proved the extreme delicacy of the spectroscopic indication of the presence of sodium, by showing that quantities of sodium compounds, wholly imperceptible by any other method, and comparable in weight to a 200-millionth part of a grain, would readily indicate their presence by their spectrum.

To the German professors, Kirchhoff and Bunsen, with whom was also associated in these researches our eminent countryman, Professor Roscoe, science is indebted for the development of spectroscopic observation as a means of chemical analysis. The manner of viewing the spectra of flames devised by Bunsen will be understood by inspection of Fig. 222, on which is represented the arrangement of his first and simplest apparatus. The dark chamber is represented by the box here shown with the cover removed. Near the middle of the box is the prism *d*. The prism here shown is a hollow one, its sides being made of flat plates of glass cemented together at the proper angles, and containing, as in a bottle, some liquid of great dispersive power, such as sulphide of carbon. In one side of the box is fixed a tube, which carries at its outer extremity *c* an arrangement by which the very narrow vertical opening or slit placed there can be adjusted to any required width. The end of this tube next the prism carries a lens, and as the slit is placed near its principal focus, its action is to render parallel the rays which fall upon the prism, so that with the slit and the prism only a few inches apart, the same effect is obtained as if the slit were at a great distance. The rays, after their passage through the prism, are received into a telescope fixed on another side of the box, and the lines, seen on looking through the telescope, are in fact so many different images of the slit as there are rays of different refrangibilities to produce them. To obtain the spectra of many substances it suffices to place a drop of their solutions on the end of a platinum wire supported by the stand *b* in the flame of the

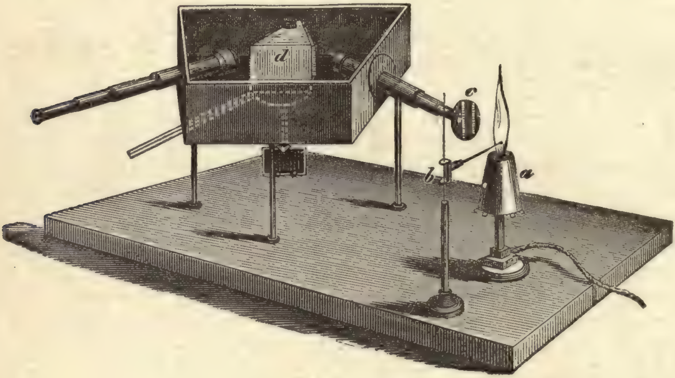

FIG. 222.

Bunsen gas-burner *a*. The apparatus included, as will be seen, an arrangement for turning the prism so that the lines of the spectra might be identified by the angular position of the prism when these lines were brought into coincidence with cross-wires in the telescope. But greatly improved forms of spectroscopes were soon afterwards contrived. For example, from the face of the prism next the telescope the image of a graduated scale was reflected into the telescope, and being seen simultaneously with the spectra, the graduation at which the lines appeared could be noted, and they could therefore be identified by their position. The box was dispensed with, because a black cloth thrown over the prism and the inner ends of the tubes cut off all extraneous rays. Fig. 223 shows the form of single-prism chemical spectroscope now constructed by Mr. Browning. The slit is on the left, the telescope on the right. The prism is supported on a little stage fixed in the centre of a horizontal disc, about whose centre moves an arm carrying the telescope. The limb of this disc is graduated, and the movable arm carries a *vernier* (page 212). Any line of a spectrum is identified by the angular reading when the line coincides with the cross-wires in the eye-piece of the telescope. In the front of the slit a small prism is so mounted that it can be either made to cover half the length of the slit, or can be turned aside altogether. The object of this arrangement is to reflect, when required, rays from a second Bunsen's burner or other source of light placed laterally, so that two spectra may be viewed side by side, and their lines compared.

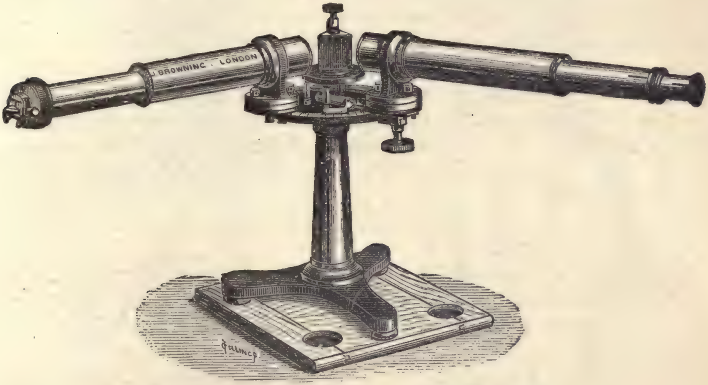

FIG. 223.—SPECTROSCOPE WITH ONE PRISM.

In the hands of Bunsen the new method of spectral analysis very soon led to some striking results, for in 1860 he had added by its means two new metals to the list of chemical elements. In examining spectroscopically the salts contained in the waters of a mineral spring at Dürkheim, he observed certain lines which he had never seen in spectra before. He was so convinced that these lines were due to minute quantities of some hitherto unknown chemical elements, that, in order to separate from the water a quantity of the substances sufficient to admit of their examination by ordinary methods, he had no less than 44 tons of the Dürkheim water evaporated. He was able to isolate from the residue salts of two hitherto unknown metals, of the same class as potassium, sodium, and lithium. To one of these metals he gave the name of *rubidium*, because among the lines of its spectrum were two very intense red lines. The other metal, on account of two splendid blue lines in the spectrum of its compounds, was called *cæsium*. The quantities of the salts of these two metals contained in the Dürkheim water are extremely small. One ton of water contained only 4 grains of chloride of rubidium and 3 grains of chloride of cæsium. These quantities are mere traces, which could never have been recognized by any ordinary method of examination of mineral waters. So great is the delicacy of the spectroscopic reactions, that one-millionth part of a grain of chloride of cæsium can be recognized by its characteristic lines. Singularly enough, rubidium, though existing in very small quantities, was found to be widely diffused in the vegetable world. It has been found in the ashes of tea, coffee, cocoa, beetroot, oak, and other plants.

Another metal which yields a very characteristic spectrum, and which was, before these researches, supposed to be of very rare occurrence, was found to be in reality one of the most widely diffused of the chemical elements. So far is lithium—the metal to which we allude—from being confined to half a dozen minerals, in which it was previously known; that it is found in almost every rock, in the ashes of most plants, in sea and river-water, and in animals. The spectrum of lithium is characterized by a splendid red line of great intensity. There is also a feeble yellow line, and these two are the only lines which the spectrum of this substance presents. As in the case of the other alkaline compounds, the spectrum reactions of lithium are of great delicacy, for so small a quantity as $\frac{1}{6000000}$th part of a grain can easily be detected. Indeed, the spectroscopic reactions of the compounds of all the alkalies and alkaline earths are remarkable for the extremely small quantities of the substance required to exhibit the characteristic lines. Whether their spectra exhibit few or many lines, these fall in each case in different parts of the spectrum; and when once the positions of the lines have been mapped, the spectrum and the substance producing it can be identified with absolute certainty.

The year following the discovery of the two new metals a third was found by Mr. W. Crookes by the spectrum indications in a mineral obtained from the Hartz. This substance imparted a bright green tint to flames, and Mr. Crookes found that the flames gave a spectrum consisting of a single line of a beautiful green colour. He isolated

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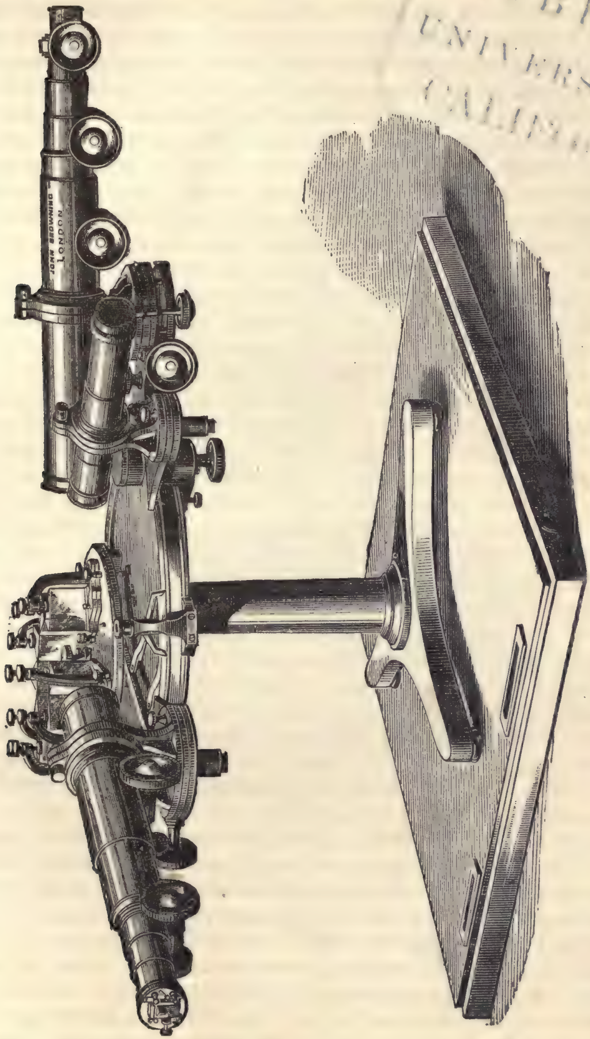

FIG. 224.—THE GASSIOT SPECTROSCOPE.

the new substance, which proved to be a metal having chemical properties intermediate between those of lead and the alkaline metals. For this metal Mr. Crookes proposed the appropriate name of *thallium*, the Greek word θαλλος, a green branch. Since Mr. Crooke's discovery of thallium, the spectroscope has been the means of adding several other new metals to our list of chemical elements. Thus, in 1864, Professors Reich and Richter, of Freiberg, in Saxony, found a metal, the spectrum of which is distinguished by a blue and a purple line.

The method of obtaining spectra which has just been described answers perfectly for the compounds of the alkalies and the earths, viz., for the detection of potassium, sodium, lithium, rubidium, cæsium, thallium, indium, calcium, barium, and strontium; but compounds of the other metals for the most part fail to exhibit any characteristic spectrum when their compounds are simply brought into the flame of a Bunsen's burner. It will be remembered, however, that the spectrum of the electric spark was examined for the first time by Wollaston (page 479), but the inquiries which his observation suggested were not pursued at the time. It was very naturally supposed that the luminous appearance was electricity itself, until Faraday conclusively proved that electric sparks are attended with a transference of some portion of metals between which they pass; and that, in fact, the luminous effect is due to the intense ignition of small portions of the metals or other substances. These deductions were completely confirmed by Wheatstone, who, in 1835, announced that the spectra of electric sparks were different as they were taken between different metals, and he considered that the spectra were due not to the combustion of the metals but to their volatilization. Indeed, Fox-Talbot had observed the previous year certain bright lines in spectra yielded by deflagrating gold and copper by the passage of a current from a powerful galvanic battery.

Twenty years later the spectrum lines yielded by electric sparks taken between different metals were undergoing very careful and laborious investigations in the hands of a number of eminent experimentalists. The greater part of the progress made in this direction is due to the labours of the Swedish physicist Angström, to those of Kirchhoff in Germany, and to those of Mr. Huggins in England. These observers found that the lines peculiar to the spark-spectrum of each metal were very numerous. Sets of maps and tables of the positions of the lines were prepared by each of the philosophers we have named and by others. The amount of labour incurred in these undertakings will be better understood when it is explained that instead of the comparatively few lines which a single-prism spectroscope exhibits, the spark-spectra visible with the instruments in which a train of prisms is used instead of a single one, show lines that in some cases may be numbered by hundreds. Fig. 224 shows the arrangement of a spectroscope with nine prisms, which was made for Mr. Gassiot by Mr. Browning. This number has been exceeded in other instruments, and the ingenious device has also been adopted of causing the rays to be reflected so as twice to traverse the train of prisms. Although some thousands of lines of spectra have been mapped, it is probable that the number of visible lines may increase with every increase in the power of the instrument.

It was soon observed that the spectrum lines given by any substance depend upon the temperature. Thus, while lithium compounds in the flame of the Bunsen burner give only the splendid red and the feeble yellow lines already referred to, these same compounds, volatilized in the intense heat of the electric arc between carbon points, yield an additional line of a beautiful blue colour. The spark-spectra of barium, of strontium, and of calcium exhibit many lines in addition to those which are visible in their flame-spectra. Thallium, for instance, in addition to the intense green line of its flame-spectrum, shows five other lines. In examining spark-spectra, the induction coil (the description of which will be found in Chapter XX.) furnishes a very convenient apparatus. The poles of the "secondary coil" are formed of slender rods of the metal to be examined, or sometimes the sparks are drawn from a solution containing the metal. In Fig. 225 is a simple and effective arrangement for obtaining spark-spectra, either with solutions or pieces of metal. The spectra of metals examined in this way are found to contain a very great number of lines. Thus the spark-spectrum of calcium exhibits in a powerful spectroscope at least 75 lines, while that of iron shows nearly 500.

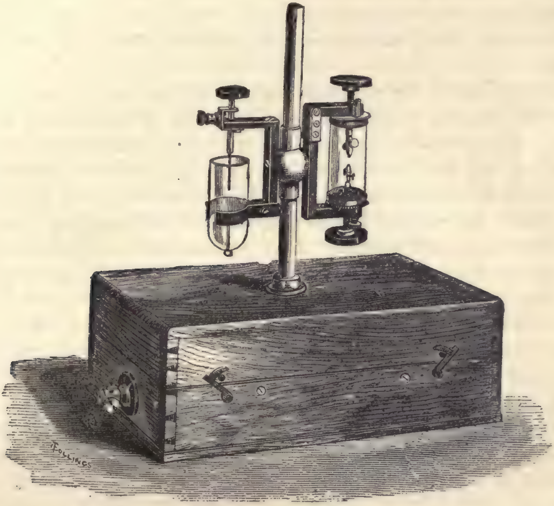

FIG. 225.—APPARATUS FOR SPARK SPECTRA.

It was soon observed that these spark-spectra exhibited two sets of bright lines; one set special to the particular metal of which the poles are found, and the other set belonging to the gases which surrounded the poles. Thus, if the sparks are taken in air between poles of platinum, some of the bright lines due to the oxygen and to the nitrogen of the atmosphere appear in the spectrum, as they do in the spectrum of each other metal under like circumstances. If hydrogen gas surrounds the poles, then only the lines of the metal, together with those of hydrogen, are visible, and so on. A small portion of the surrounding gaseous medium is, in fact, heated to incandescence by the electric discharge, and a small quantity of the metal of the poles is also volatilized, and in the state of glowing gas yields its characteristic lines. Hence spark-spectra and flame-spectra appear to depend upon this principle: Each chemical element when in the state of a glowing gas gives off a certain set of rays of definite refrangibilities. Further, it appears that some of these rays are given off only when the temperature of the vaporous substance is very high, and this is the condition obtained by the electric discharge.

Lines peculiar to the gases of the atmosphere surrounding the metallic poles become visible in the spectrum of the spark, as already remarked, when the discharge is intense. When the discharge is made to take place in a highly rarefied gas, as in the *Geissler Tubes* (Chapter XX.), the spectrum lines peculiar to the gas are obtained with great brilliancy. These tubes contain but extremely small quantities of gas; but when the sparks of an inductive coil are allowed to stream through them, they glow with light, which in many cases presents a characteristic colour even to the unaided eye. Thus a hydrogen tube seems filled with red light, a carbonic acid tube with blue light, and so on. This light, like that of a substance heated in the flame of a Bunsen's burner, is attributed to the incandescence of the minute quantity of gas in the tube, and if so, it represents a temperature far higher than any which can be obtained in flames. The extreme smallness of the quantity of the gas will explain the absence of the very marked heating effects which a larger mass at a temperature of a like order would produce.

When the light of the discharge in Geissler's tubes is examined by the spectroscope, the characteristic lines of the gas are very distinctly seen. Hydrogen gas, for example, gives a spectrum of three distinct lines,—one dark blue, one greenish-blue, and one red; the red line being much more intense than the others. Hence the red light which appears to fill the Geissler's hydrogen tube. The redness of the ordinary electric spark in air is due to the hydrogen contained in the aqueous vapour of the atmosphere. The spectra of nitrogen, oxygen, and other gases are much more complicated than that of hydrogen. Now, while the electric spark shows the spectra of the metals, the method of observation by the discharge in Geissler's tubes gives the spectroscopist the characteristic lines of all the gaseous elements. Thus the whole of the chemical elements may be identified by the spectroscope. Many chemical compounds give, under certain circumstances, lines which do not belong to the elements of which they are composed, and these have been supposed to belong to some compounds of the substances undecomposed at the temperature of the flame.

In 1865 Plücker and Hittorff showed that many of the elementary gaseous bodies will yield different spectra according to the conditions of pressure and temperature. For example, these observers found that nitrogen, when rarefied beyond a certain point, will not permit the passage of the induction-sparks; but under a tension of less than one millimetre of mercury, the current passes and the tube becomes luminous, giving off a yellow-coloured light, the spectrum of which consists of a large number of coloured bands, or narrow luminous spaces less sharply defined than the "*line*" spectra. A slight change either in the intensity of the spark or density of the gas causes the nitrogen to glow with a bluish instead of a yellowish light, which gives another band spectrum different from the former. If the temperature due to the electric discharge is still further increased by including a Leyden jar in the circuit, the light changes to white, and the spectrum which this yields is now one of bright *lines* on a dark ground. These changes were explained by attributing them to the existence of different *allotropic* states of nitrogen. Chemists are indeed not acquainted with nitrogen except in one state; but they are familiar with certain other elements—sulphur, for example—in states in which different physical properties are exhibited by the same (*i.e.*, *chemically* identical) substance states. These are also known to depend upon temperature, so that Plücker and Hittorff's explanation has much probability in its favour. The same observers have found that by increasing the temperatures sufficiently, oxygen and certain other elementary gases yield continuous (page 484) spectra.

It would be impossible within our limits to mention a tithe of the many interesting discoveries which have rewarded the labours of spectroscopists. A multitude of skilful experimentalists in every country of Europe and in North America have for the last twenty years been engaged in enlarging our knowledge of the spectra of the chemical elements and compounds. But as we have probably dwelt upon spectrum analysis in its application to terrestrial substances sufficiently to give the reader some notion of the kind, though not of the number, of facts upon which this new method of investigation is based, we now turn to another development of the subject, which is of the highest general and scientific interest, inasmuch as the discoveries which it includes have greatly influenced our views of the constitution of the universe.

The stars of heaven, ever as it seemed carried round the earth in the unhasting and unresting revolution of their sphere, must, even in the most remote period of human history, have attracted men's eyes and thoughts. How many minds have pondered over the mysteries of the midnight sky! How many eyes have anxiously scanned the constellations, to read, if might be, in their bright configurations the preordained destinies of men! Gliding on silently and unchangeably, far above the mutations and turmoil of the generations of mankind, was it any wonder that the sages of old deemed the orbs of the starry sphere to be eternal and incorruptible? But from the time when the telescope of the illustrious Florentine was pointed to the sky, men's conceptions of the heavenly bodies began to undergo modifications. When two of the greatest intellects of last century—Laplace, the French mathematician, and Kant, the German metaphysician—had simultaneously and independently conceived and propounded that bold speculation called the Nebular Hypothesis, the scientific conception of the universe had taken a distinct form. This hypothesis had its justification in the revelations which the improvement of the telescope afforded of the configurations of the sun, moon, planets, and nebulæ. Astronomers have had reason to be gratified with the great additions to our knowledge which the gigantic telescopes of Herschel and the great refractors of more recent times have been making. Yet it appeared as if all knowledge of the chemical nature of extra-terrestrial matter was completely and hopelessly denied. There was, indeed, the exception of the aërolites, those metallic masses which fall upon the earth from the realms of space. The chemist could of course take a fragment of an aërolite to his laboratory, analyse it into its constituent parts, and state the name and quantity of each element it contained. But could astronomer or chemist ever hope to find, with the same certainty, what chemical elements exist in the far-distant sun?

Kirchhoff and Bunsen not only developed spectroscopy as a method of detecting with great ease, certainty, and delicacy the presence of the various elements in substances which may be handled in the laboratory, but the principle by which the former philosopher explained the Fraunhofer lines laid the foundation for cosmical chemistry. Applying this principle to the sun, we can understand how the central part of our luminary would of itself yield a continuous spectrum; but the substances surrounding it as vapours, and forming an intensely heated atmosphere, intercept those radiations which they are themselves capable of emitting. Thus, in the experiment already mentioned the ignited lime would correspond with the solid or a liquid nucleus of the sun, while the sodium-flame would represent the sodium vapour contained in its atmosphere. Kirchhoff undertook the task of comparing the positions of the Fraunhofer or dark solar lines with the bright lines in the spark spectra of our several elements. In comparing the very numerous bright lines yielded by the spark-spectrum of iron with the Fraunhofer lines, he found that for every bright iron line there was, occupying exactly the same place in the spectrum, a dark Fraunhofer line. The coincidence in position was perfect for each of several hundred lines, and not only so, but the relative intensities and widths of the lines in the two spectra corresponded, line by line. Kirchhoff prepared with great care a map of the solar spectrum, in which the positions and intensities of many hundred lines were laid down. To those solar lines which he found identical with lines of one of our terrestrial elements, he attached the chemical symbol of that element. In this way the 460 bright lines which belong to the spark-spectrum of iron were found to agree with Fraunhofer lines. The conclusion, therefore, was inevitable that iron as vapour must exist somewhere between the nucleus of the sun and the spectroscope. That the atmosphere of the sun itself was the only place where such vapour could exist was apparent from several considerations. When the presence in the sun's atmosphere of some of our terrestrial elements had thus been demonstrated, it seemed reasonable to attribute the rest of the Fraunhofer lines to other elementary bodies. Kirchhoff accordingly examined the spectra of calcium and magnesium, and he found the most perfect coincidence in the position and intensity of the lines in these cases also. In short, his observations led him to conclude that in the sun's atmosphere the following metals certainly exist, viz., sodium, calcium, barium, strontium, magnesium, iron, nickel, cobalt, chromium, copper, zinc, cadmium, manganese, and hydrogen. The presence of these elements in the sun's atmosphere accounts for a certain number of the Fraunhofer lines, and another considerable number are known to be due to the earth's atmosphere, for this produces an effect analogous to the sun's atmosphere. There still remain the greater number of the Fraunhofer lines as yet not identified, and this fact suggests the existence in the sun of many substances with which we are not acquainted on the earth.

These very remarkable results were announced by Kirchhoff in 1861. They harmonized well with the cosmical theories of geologists and astronomers, as the following words of Kirchhoff himself will show:

"In order to explain the occurrence of the dark lines in the solar spectrum, we must assume that the solar atmosphere encloses a luminous nucleus, producing a continuous spectrum, the brightness of which exceeds a certain limit. The most probable supposition which can be made respecting the sun's constitution is that it consists of a solid or liquid nucleus heated to a temperature of the brightest whiteness, surrounded by an atmosphere of somewhat lower temperature. This supposition is in accordance with Laplace's celebrated molecular theory respecting the formation of our planetary system. If the matter now concentrated in the several heavenly bodies existed in former times as an extended and continuous mass of vapour, by the contraction of which sun, planets, and moons have been formed, all these bodies must necessarily possess mainly the same constitution. Geology teaches us that the earth once existed in a state of fusion, and we are compelled to admit that the same state of things has occurred in the other members of our solar system. The amount of cooling which the various heavenly bodies have undergone, in accordance with the laws of radiation of heat, differs greatly, owing mainly to the difference in their masses. Thus, whilst the moon has become cooler than the earth, the temperature of the surface of the sun has not yet sunk below a white heat. Our terrestrial atmosphere, in which now so few elements are found, must have possessed, when the earth was in a state of fusion, a much more complicated composition, as it then contained all those substances which are volatile at a white heat. The solar atmosphere at this time possesses a similar constitution."*

When the significance of the spectra of the heavenly bodies as revealing their chemical constitution had been established by the classical researches of Kirchhoff, the study of the spectra was entered upon with great eagerness by several devoted and enthusiastic observers, both in England and upon the Continent. Our countryman, Dr. Huggins, was one of the earliest and most successful labourers in the new field of discovery.

The spectroscopic observation of the stars, extending to the accurate measurement and comparison of the positions of their light or dark with those of known substances, may easily be understood to be a task of greater difficulty than the study of the sun lines. The image of a star being but a point, when such an image falls upon the slit of the spectroscope, the spectrum it produces is without appreciable breadth, that is, it is merely a variously-coloured line, in which the eye is unable to perceive the extremely minute breaks which would represent dark lines. It is necessary to widen out the image of the star upon the slit, so that instead of a mere point it shall form a little line of light parallel to the slit, and this is accomplished by making use of a cylindrical lens. Then the instrumental appliances require great nicety of construction, for the telescope must follow the apparent motions of the heavens in such a manner that the position of the image shall not be changed by a hair's breadth. Further, the apparatus must include the means of observing simultaneously the spark or flame-spectra of known terrestrial elements produced by the same train of prisms.

The earlier researches of Mr. Huggins on stellar spectra were undertaken in conjunction with Professor W. A. Miller, and a memoir published under their names appears in the "Philosophical Transactions" of 1864. In this paper the authors remark that the success of Kirchhoff in determining by spectrum analysis the nature of some of the constituents of the sun suggested to them the extension of the same method of investigation to the fixed stars. They began their experiments in January, 1862, the investigation upon which they entered being one that had never previously been attempted or even imagined. Nothing, indeed, previous to Kirchhoff's researches, could have appeared more impossible than that men should ever attain to any knowledge of the chemical constitution of the stars. A philosopher of the present century had even pronounced that other knowledge of the fixed stars than could be gathered from their motions was never to be expected. The only known facts regarding them were that some of them formed systems governed by the same laws of gravitation as obtain in the solar system, and that analogy rendered it probable that their constitution more or less resembled that of our sun. The lines in the spectra of a few of the more conspicuous stars were described by Fraunhofer in 1823. He observed the solar lines of D, E, *b*, and F in the light reflected from the moon and planets. In the spectra of the stars Capella, Betelgeux, Procyon, and Pollux he saw the D line, and mentions the presence of the *b* line in the spectra of the first two. Sirius and Castor exhibited lines altogether different from those of the sun. It was not until about the period when Huggins and Miller entered upon their researches that any further observations on star spectra were published. Then some notices of stellar spectra appeared by Donati of Florence, and about the same time as Miller and Huggins laid the results of their first observations before the Royal Society, Rutherford in America, Secchi in Italy, and Airy in England published some diagrams of the lines in star spectra.

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* As quoted by Roscoe in "Spectrum Analysis."

Huggins and Miller found in the spectra of all the brighter stars lines as fine and as numerous as those of the solar spectrum, and every star sufficiently bright to exhibit a spectrum yielded one with lines, which differed in their position and grouping for each star. In the spectra of Aldebaran and of α *Orionis* the D line of sodium is visible, and the lines marked *b* by Fraunhofer, which are known to be due to magnesium. Four lines belonging to calcium were also observed in both spectra. In the spectrum of Aldebaran, the two lines C and F, indicating hydrogen, were conspicuous, in the spectrum of α *Orionis* they were altogether wanting. In the two stars upwards of 70 lines were recognized, indicating the existence of certain known chemical elements in those far-distant bodies. In Aldebaran, for instance, the spectroscope gave evidence of hydrogen, sodium, magnesium, calcium, iron, bismuth, tellurium, antimony, and mercury.

In a preceding chapter we had occasion to mention the apparition of a temporary star (page 85), and this is far from being the only instance of such a phenomenon. Still, these instances are so rare, that it may be esteemed a singularly happy chance that, while Huggins and Miller were engaged in the observation of stellar spectra, a phenomenon of this kind should occur. In May, 1866, a scarcely discernible telescopic star, τ *Coronæ Borealis*, suddenly blazed out, and became one of the most conspicuous stars in the heavens. Huggins turned his instruments to this extraordinary object, and found that its spectrum differed remarkably from that of any other star he had examined. It showed not only a spectrum with dark lines, but also with *bright* lines, among which C and F, that belong to incandescent hydrogen, were very conspicuous. The inference that the increase of brilliancy was due to intensely heated hydrogen, was irresistible. In twelve or fourteen days this star, by gradual diminution of its sudden brilliancy, had reverted nearly to its former condition of a star of the tenth magnitude. The bright lines disappeared as the light of the star waned. Other observers have announced that bright lines are visible in the spectra of several very small stars. Father Secchi in 1869 proposed to classify the stars in several groups according to the kind of spectrum that each exhibited. One group consists of those stars which give spectra intersected by many fine dark lines. Such are the stars Pollux, Capella, and, in general, those stars which shine with a yellow light. Our sun also is regarded as a star belonging to this group. A second group comprises many of the brightest stars in the heavens, namely, those which have a white lustre, and their spectra are characterized by a small number of broad dark lines: such are *Sirius* and *Lyræ*, etc. A third group includes reddish-coloured stars, which give spectra having alternate dark and light bands or spaces: such are *Herculis* and *Orionis* and *β Pegasi.*

The study of spectra was greatly facilitated by the substitution of photography for mapping in the delineation of the lines. In this way the spectrum is made in fact to produce its own map, and it is found that the "chemical rays" which accompany the luminous rays faithfully indicate on the prepared plate the position and intensities of all the lines. Rutherford, of New York, appears to have been the first to turn to account the power of photography in the delineation of spectra. Mr. Huggins photographed the spectrum of Sirius. It is marvellous that, from the inconceivable distance of 130 millions of millions of miles, the light of Sirius, reaching us after a journey of twenty-one years' duration, should bring with it the power of impressing the sensitive photograph-plate that secures for us the records of the chemical constitution of the star as it was twenty-one years before.

Mr. Huggins was the first to direct the spectroscope to those mysterious bodies, the nebulæ. He found that the light of some of the nebulæ was too faint to yield spectra distinct enough to give satisfactory results. He was able to determine the characters of the spectra of about seventy nebulæ. Of these about one-third gave a spectrum with bright lines, and differed one from another only by the relative intensities of the lines and the degree in which the lines were accompanied by a continuous spectrum. Bright line spectra are produced only by incandescent gases, so far as we know, and therefore the inference is, that in these nebulæ we have to do with gaseous matter. The next point was to determine the nature of the gases. One of these lines was exactly coincident with one of the hydrogen lines, another with the bright of the nitrogen lines, but the third occupied a position corresponding with no known element. Fig. 226 shows one of these nebulæ spectra, and the lines of hydrogen and of nitrogen are placed beside it. It will be observed that the nitrogen spectrum here shown has a double and not a single line at *N.* This is the spectrum yielded by an electric spark passed through nitrogen in a sealed tube; but by changing a little the conditions of the experiment, Mr. Huggins found that only one line appeared in the nitrogen spectrum, and this was coincident with the bright line of the nebulæ. There are, however, two other very conspicuous double lines in the ordinary spectrum of nitrogen, which lines are altogether absent from the nebulæ spectra. How then, it may be asked, has Mr. Huggins explained the absence of these two? He found that when between the eye and a spectroscope a certain thickness of neutral-tinted glass was interposed, the result was that all the lines of the ordinary spectrum of nitrogen ceased to be visible except this green line, which coincides with that of the nebulæ. Similarly with regard to the lines of the hydrogen spectrum, the intense blue line (corresponding with the Fraunhofer line F) was the last to disappear. This explanation of the disappearance of the weaker lines of a spectrum, attributes to something in the cosmical spaces traversed by the light a power of extinguishing indifferently the rays of light. The fact that this is quite consistent with theoretical considerations that have been advanced on other grounds increases the probability in its favour. The reader will perceive that these single coincidences do not prove the existence of nitrogen and of hydrogen in the nebulæ, with the same certainty as the 460 coincidences of the iron lines prove the existence of that metal in the sun. There are, however, considerations derived by analogy from the important part known to be played by the element hydrogen in our own sun, which add weight to the theory of Mr. Huggins. For the discoveries of Kirchhoff concerning the chemical constitution of the sun were soon followed by a series of remarkable discoveries in which the spectroscope opened out a new field of research. In total eclipses of the sun, red-coloured prominences have commonly been observed projecting from the sun's limb, and visible only when the bright light of its disc is entirely intercepted by the moon. Extending to a considerable distance beyond these is a fainter light, called the *corona*, the nature of which has been long a subject of much speculation. The *corona* and the prominences are roughly represented

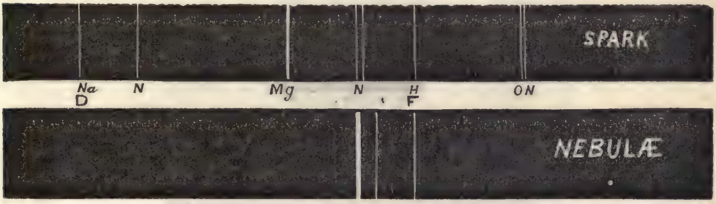

FIG. 226.

in Fig. 227 somewhat as they appeared in the total eclipse of 1869. The first distinct record of the red prominences is contained in a letter addressed to Flamstead by an observer of the eclipse of 1706. They were seen again in 1715, and in almost every total eclipse since, as well as in some partial eclipses. Some observers attributed the "red flames" to the moon and others to the sun, and it was not until the total eclipse of 1860, when the red flames were very distinctly seen at many stations, that it was conclusively proved they belonged to the sun and not to the moon.

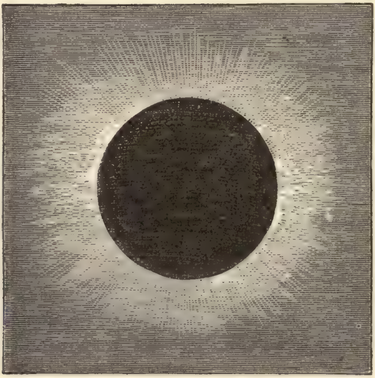

FIG. 227.—SOLAR ECLIPSE, 1869.

In 1866 Mr. Norman Lockyer suggested that by the use of the spectroscope it might be possible to obtain evidence of the presence of the red prominences without waiting for solar eclipses. But it was not until two years afterwards that Mr. Lockyer was able to realize his idea. On the 20th of October, 1868, he succeeded in obtaining the spectrum of a red prominence, and this spectrum he found to consist of three bright lines, one exactly coincident with Fraunhofer's C, the second nearly coincident with F, and the third near D. Strangely enough, the same discovery had just been made independently in a distant part of the world, though the French Academy of Sciences received the communication a few days after Lockyer had announced his results to the Royal Society. The French Government had sent M. Janssen to India to observe at Guntoor the total eclipse of the sun on the 18th of August. M. Janssen mentions that during the totality two fine red prominences were visible, one of which subtended an angle of more that 3′, and therefore must have been 90,000 miles in height. The spectroscope showed that this object was an immense column of incandescent hydrogen gas. During the progress of the eclipse, a method of observing the red prominences independent of eclipses occurred to Janssen's mind. This was the same that Lockyer was working out; and Janssen appears to have applied it successfully on the day following the eclipse. From this time until the 4th of September Janssen continued daily his observations of the red prominences. These observations give results comparable to such as might have been attained during an eclipse of seventeen days' duration. The enormous rapidity with which is effected the movements of the masses of gas constituting the red prominences was remarked by Janssen, and this has been confirmed by other observers. Mr. Norman Lockyer not only discovered independently the method of spectroscopically viewing the red prominences at all times, but he has also been one of the most assiduous and successful spectroscopists. In a communication to the Royal Society, dated November, 1868, Mr. Lockyer announces that the prominences are merely local aggregations of a gaseous stratum which completely envelopes the sun. He suggests for this layer of gas the term *chromosphere*, in allusion to its coloured light, while he calls the inner and most luminous layer the *photosphere*. The light from the photosphere is white or colourless, and this would of itself give a continuous spectrum, but for the presence of a third gaseous stratum, between the *photosphere* and the *chromosphere*. This layer is comparatively thin, and it contains those elements which by absorption of certain rays give rise to the Fraunhofer dark lines. It has, therefore, been called the *reversing* layer. In the sun there is, first and deepest, the photosphere; then the reversing layer; outside of the reversing layer is the chromosphere; and external to all is the more diffused *corona*. Mr. Lockyer observed that the spectrum of the chromosphere is modified in its lower regions, just as the spectrum of hydrogen is known by the experiments of Plücker, Hitorff, and Frankland to be modified by increased pressure and temperature. The spectroscope, therefore, not only reveals the chemical constitution of the sun, but indicates something of the conditions of temperature and pressure under which the elements probably exist there. Even the *motion* of masses of glowing gas have their spectroscopic indications. The rapid changes which occur in the forms of the red prominences imply movements of enormous velocity, a velocity which is comparable with that of light. The effect of these rapid movements is to increase or diminish the periodicity-rate (or wave-length) of the emergent rays. Their refrangibility is affected accordingly, and a slight displacement of the lines is observed towards the violet end of the spectrum if the gas is ascending, and towards the red end if descending. These results are confirmed by a recent observation, in which the spectra of the parts of the margin of the solar disc situated at each extremity of its equatorial diameter were viewed in juxtaposition, when a displacement of the Fraunhofer lines was distinctly perceptible. The reason of this is that the one part of the sun's surface is approaching towards, the other receding from, the spectator.

It would lead us far beyond our limits to state the facts from which observers have been able to infer the existence in the sun of several gaseous strata in which the different elements are arranged. Mr. Lockyer's spectroscopic researches have enabled him to specify the chemical elements contained in each layer, as in the following table:

| | |
|---|---|
| The Corona | An unknown element (?). |
| | Sub-incandescent hydrogen. |
| The Chromosphere | Incandescent hydrogen. |
| | An unknown element (?) for which the name *Helium* is proposed. |
| | Calcium. |
| | Magnesium. |
| The region of the Solar Spots | Sodium. |
| | Titanium. |
| | Chromium. |
| | Aluminium. |
| The Reversing Layer | Iron. |
| | Manganese. |
| | Cobalt. |
| | Nickel. |
| | Copper. |
| | Zinc. |
| | Potassium. |
| | Strontium. |
| | Barium. |
| | Cadmium. |
| | Lead. |

The reader must have observed how remarkably the results of the spectroscopic observation of the heavenly bodies has tended to confirm the nebular hypothesis of Laplace (page 269). The chemistry of nebulæ, stars, and suns was but a few years ago a thing which even wise philosophers deemed an impossible attainment. Now it has come to pass that, after Galileo's discoveries and Newton's great conception of universal gravitation, no additions to our knowledge have more deeply modified our ideas concerning the universe at large than those new facts which the spectroscope has revealed. It was for this reason imperative that a chapter should be devoted to the notice, however incomplete, of the revelations of the spectroscope. Necessarily a multitude of interesting researches have been passed over in silence, and some remarkable developments of spectroscopy altogether omitted. Such, for example, is the application of the spectroscope to the microscope by Mr. Sorby, in which this subtle mode of analysis is made to reveal the presence of substances which would defy all other methods of detection. Thus, a droplet of blood, less than a pin-head in size—nay, even the thousandth part of a grain—can infallibly be recognized, and also the state of that blood with regard to oxidation revealed.

This chapter has been occupied with the description of observed phenomena, from which many important discoveries have resulted, but from which no comprehensive general theory has yet emerged. In truth, the facts treated of here are in the condition of the facts of astronomy before Newton united them by the grand and simple conception of universal gravitation. Observations, facts, and measurements of the phenomena are still being accumulated, and they await the advent of a spectroscopic Newton, who will one day promulgate some comprehensive principle by which the relation of every line of the spectrum of each substance to its chemical constitution and physical state may be definitely expressed. Our knowledge of the conditions of the phenomena is yet far from complete, and the facts already known stand apart from the general theory of light. The general doctrine of the undulations of the luminiferous ether has here afforded little guidance. The conception of the particles of different gases executing vibratory movements of definite periodicity, which are communicated to the ether, and thus give rise to rays of certain refrangibilities, is satisfactory so far as it goes, but it does not touch on the relation of chemical constitution to the various kinds of spectra. Why should the same substance give at one temperature one or two lines only, and at a higher temperature give others in addition, or perhaps a quite different set? How are the differences between the spectra of compounds and their elements to be explained? Why does one element give a spectrum having few lines, while an analogous element may give one having many lines? Can any relations be traced between the positions of the lines and the physical or chemical properties of the substances? These are the questions which spectroscopists are seeking to solve, and their solution involves a profounder insight into the inner constitution of things than has yet been obtained. It is not an unfair inference from what the spectroscope has already accomplished, that it will yet prove one of the most efficient of all instruments for the attainment of this profounder insight.

There have not been wanting speculations bearing upon the chemical relations of the spectrum lines, but little appears to have been definitely made out. Helmholtz has propounded the theory that molecules—that is, the aggregation of atoms—give band spectra, while free atoms gave line spectra. Lockyer supposes that by increase of temperature the molecules of nearly all our so-called elements may split up into smaller groups of atoms, each of which yields its distinctive line. This separation by increase of temperature is, in fact, known to take place in certain cases, and is familiar to chemists under the name of "dissociation." This theory involves the supposition that our so-called elements are not really simple substances, but that they are resolved into true elementary bodies by a sufficient increase of temperature. The dissociation is progressive, and its final stage is reached at a temperature which is different for different substances. These views may be illustrated by the diagram in Fig. 228, which represents the lines due to a progressively increased temperature, in the case of calcium spectra, according to Lockyer's views. The particular compound of calcium existing in the flame of a Bunsen burner coloured by a lime salt, to which is due the intense red line indicated by the thick band in the lowest spectrum of the diagram (marked "flame"), is supposed to undergo dissociation in the stronger heat of the electric arc, so that one of its constituents gives rise to the intense blue line

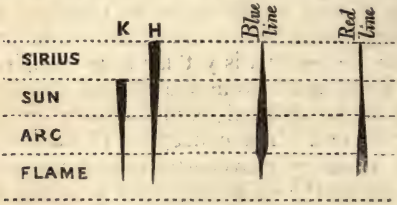

FIG. 228.

which is prominent in the arc spectrum of calcium. At the still more elevated temperature of the sun this last constituent is in its turn resolved into elements which give the intense lines H and K towards the violet end of the spectrum, and finally at the temperature of Sirius, which there is reason to believe is a hotter sun than our own, the chief element of calcium gives the marked line H, while the blue and red lines that are conspicuous at lower temperatures almost die out, because a very small proportion of the compound which gives these lines can exist at the very intense temperature of Sirius. According to this view the molecular grouping of the atoms which constitute the particles of calcium is resolved by elevation of temperature into other groupings, which have their principal lines in the violet part of the spectrum.

Plate XII. will serve to give the reader some notion of the several bright line spectra which have been referred to in the foregoing pages. The upper one (1) is intended to show merely the positions of some of the Fraunhofer lines. The spectra below are ranged thus: 2 sodium; 3 potassium; 4 lithium; 5 calcium; 6 strontium; 7 barium; 8 thallium; 9 indium; 10 Sirius; 11 nebula; 12 hydrogen; 13 nitrogen; 14 coal-gas.

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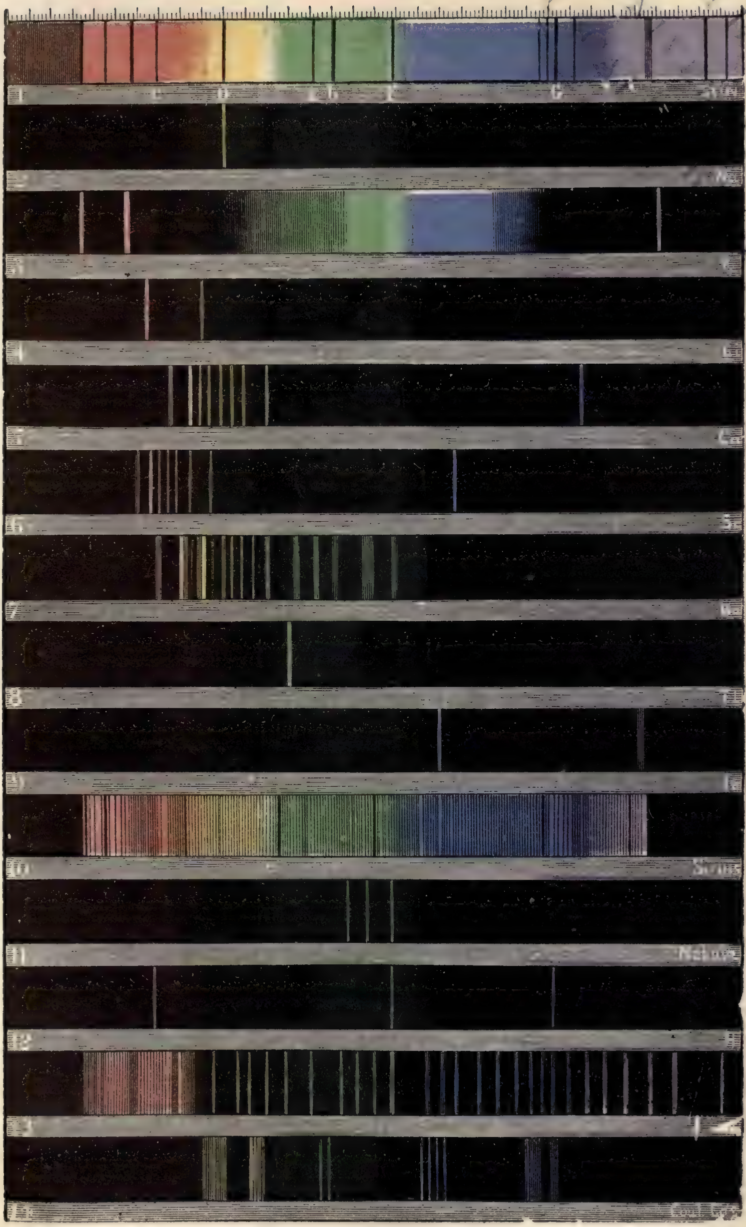

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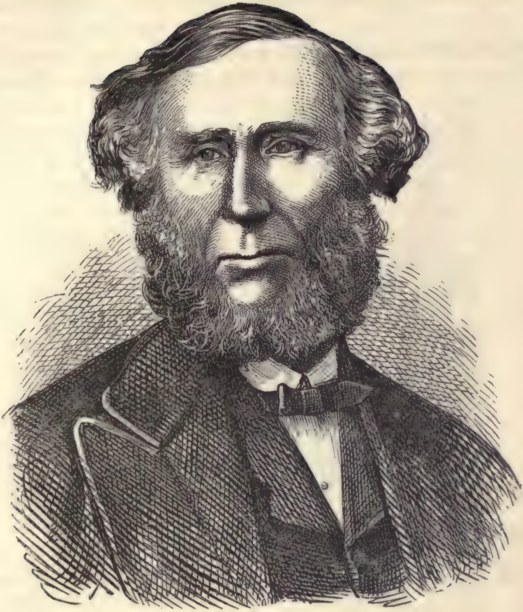

FIG. 226.—PROFESSOR TYNDALL.

# CHAPTER XIX.

## PHYSICS OF THE NINETEENTH CENTURY—(*continued*): OPTICS, RADIATION, HEAT, AND SOUND.

THE astronomical means of estimating the velocity of light have already been described, and the amazing amount of that velocity has been mentioned. Not long ago the direct measurement of the speed of light would have been supposed the most impossible of accomplishment. Yet about 1850 the problem was solved in different ways by two eminent French *savans.* The experiments first announced were those of M. Fizeau, and the principle of his apparatus may be understood from the diagram Fig. 230 A beam from a source of light A impinges on a plate of unsilvered glass B so as to be reflected along the axis of a horizontal tube, the extremity of which is provided with a convex lens not shown in the diagram. The lens is so placed that the beam emerges with parallel rays, and at a station several miles distant it falls on the plane mirror D, by which it is reflected back to B, and a portion traversing the glass B reaches the eye of the observer at O′, who, in fact, sees through a telescopic eye-piece the flame A′ by rays which have twice traversed the distance B D. The telescope at O is to enable the operators to so adjust the apparatus at the distant station that the ray reflected from B may return upon its path. At C is seen edgeways a wheel with many teeth. The teeth and the spaces between them are equal, and the wheel can be maintained uniformly at any required rate of revolution by clockwork not shown in the figure. This wheel occupies such a position that the beam of light is intercepted or allowed to pass according as a tooth or a space is in the axis of the tube. Suppose that the wheel is in motion, and that a space has permitted a beam to pass out. If, during the time that this beam takes to reach D and return, the wheel has moved so that a tooth now occupies the centre, the beam will be intercepted, and the eye at O′ would see no light. If, then, the velocity of the wheel C is gradually increased until a complete eclipse of the light takes place, this state of things will be realized, and from the velocity of the wheel and the number of its teeth it will be quite easy to deduce the time required for the light to pass to the distant station and back. An increase of the wheel's velocity would cause the light to come again into view, for the return ray would meet with a second space in the place of the first. A further increase of the velocity would again cause eclipse of the light, and that would give data for another determination. A further increase of the velocity would again produce an apparent extinction of the light, and so on. M. Fizeau's

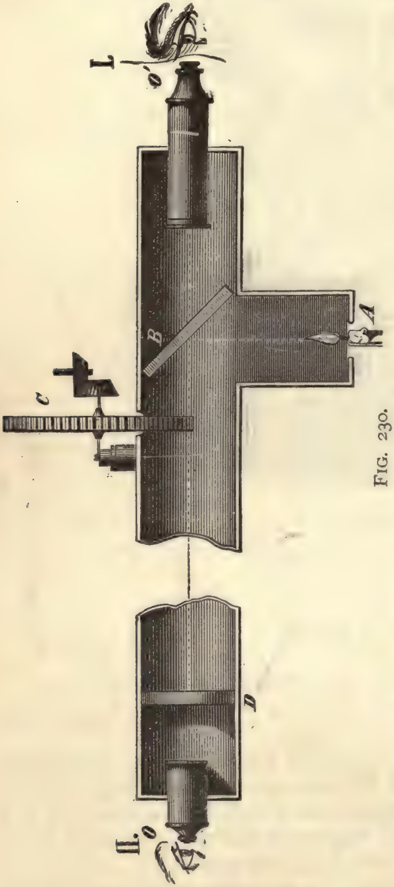

FIG. 230.

experiments with this apparatus gave the velocity of light as 314,262,944 metres, or 195,344 miles, per second.

The distance between the stations in M. Fizeau's experiments was more than five miles, and when we consider the enormous velocity of light, and that it accomplished the double journey in about the $\frac{1}{18144}$th part of a second, we cannot but admire the ingenuity of the method. This was the first time in which the speed of light had been directly measured. But soon afterwards Fizeau's compatriot, Foucault, actually measured the time taken by a ray of light in traversing the space of 26 feet. The principle Foucault employed was the same as that adopted by Wheatstone in his measurement of the duration of the electric spark. The displacement of the reflected image of a fine wire, by the angular change in the position of a small mirror making from 400 to 500 revolutions per second, gave the means of estimating the time required by the light to traverse a certain distance. A description of the apparatus would introduce more technical details than would be appropriate in this place. It may be interesting to remark, however, that the number of revolutions per second made by the revolving mirror was known by the pitch of the sound it emitted.

Some of the phenomena of radiant heat were to a certain extent investigated by Sir W. Herschel. It was he who discovered that the visible spectrum (page 217) of a solar beam does not represent the whole of the rays that are refracted by the prism; that the spectrum is extended beyond the red end in rays of obscure heat. Sir John Leslie in 1811 examined the radiating power of different substances. He devised the instrument called the *Differential Air Thermometer* (Fig. 231), which consisted of a narrow upright U-shaped glass tube having each branch terminated by a glass bulb. The tube, hermetically closed, contained air, and a little liquid in the bend of the tube. Whenever the temperature of one of these bulbs exceeded that of the other, the expansion of the air in the hotter bulb moved the column of liquid toward the colder bulb. Leslie used cubical vessels filled with hot water and coated on the four vertical sides with the various substances of which he desired to ascertain the radiating power for low heat. The face of the cube to be tested was placed before a concave mirror, from which the rays of heat were reflected upon the bulb of the air thermometer. He found that a surface covered with lampblack emitted at the same temperature more heat than a surface of polished nickel in the proportion of 100 to 12. Other substances were intermediate. The radiation was found to depend upon the condition

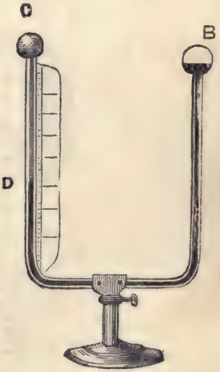

FIG. 231.

of the surface. Thus tarnished lead radiated 45, while lustrous lead radiated only 19.

The researches of Leslie were surpassed both in delicacy and extent by those of the Italian physicist MELLONI (1801—1853), to whom we are indebted not only for much of our knowledge of radiant heat, but also for the most delicate instrument for detecting minute differences of temperature. It will be unnecessary here to describe the several steps by which Melloni, working partly in conjunction with Nobili, brought the instrument to its present perfection. We shall describe the instrument as represented in Fig. 232, and as used by Melloni in his classical researches. At A B is seen the arrangement of five *thermo-electric* couples, composed each of a bar of bismuth (shaded light) and

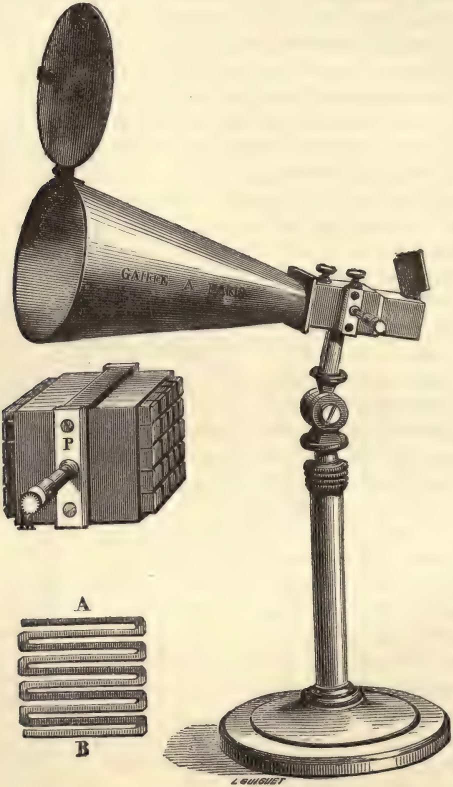

FIG. 232.

a bar of antimony (shaded dark), and joined alternately in the way shown in the diagram. The pile P is constituted of five such series as are represented at A B; that is, it contains 25 elements. The law according to which an electric current is generated when one side of the pile is warmer than the other will be found in Chapter XX. The faces of the thermo-pile are coated with lamp-black, which more perfectly than any other substance absorbs all kinds of radiant heat. The first and the last elements of the piles are connected with binding-screws, one of which is seen at M, and by wires from these it is put into connection with a galvanometer (Chap. XX). The pile is generally mounted on an adjustable stand, and a cone of silvered copper is provided for experiments on radiant heat, in order that a greater number of rays may be caused to fall upon the face of the pile by reflection from the internal surface of the cone. When a pile so fitted up is connected with Thompson's reflecting galvanometer (Chap. XX.), the arrangement becomes an apparatus of the most wonderful delicacy. Thus it will indicate the radiation from the hand held opposite to the cone at the distance of 10 feet or more.

The results obtained by Leslie, Melloni, and others, established several important laws, among which may be specified that which enunciates the equality of the absorptive and emissive powers of bodies for each kind of heat. Melloni broke ground almost quite new in his researches into the passage of radiant heat through different bodies, which he termed *diathermancy*. Diathermancy expresses the same action with regard to heat as transparency does with regard to light. Melloni used heat from four different sources, viz., a lamp; platinum wire heated in an alcohol-flame; copper at 400° C.; and copper at 100° C. The experiments were made by noting the galvanometrical deflection produced by the heated body at a suitable distance from the pile, and then observing the deflection when a layer of the substance whose diathermancy was to be determined was interposed. One of the most notable results was that of all substances, rock-salt was the most pervious to all kinds of heat-rays, permitting in all cases 92 per cent. of the total heat to pass. Glass and Iceland spar, both as transparent to light as rock-salt, entirely stopped the radiation from copper at 100°, and allowed only 6 per cent. of that from copper at 400° to pass, while they stopped three-fourths of the radiation from incandescent platinum, and three-fifths of that from the lamp. Alum allowed 9 per cent. of the radiation from the lamp and 2 per cent. of that from the platinum to pass, and stopped all from the other two sources. Ice behaved like alum, except that it permitted only 6 per cent. of the radiation from the lamp and $\frac{1}{2}$ per cent. from the incandescent platinum to pass. The few substances here mentioned differ very remarkably as to their *diathermancy*, although as regards *transparency* there is no perceptible difference. On the other hand, substances of nearly equal diathermancy may differ very greatly as to their power of transmitting light. Thus clear quartz is as transparent as the finest glass, while "smoky" quartz is dark brown and almost opaque; nevertheless, these substances are alike in their power of transmitting heat-rays, except a slight difference in the case of rays from the lamp, of which the smoky quartz transmits 37 per cent., while the clear quartz transmits 38 per cent.

One of the facts just mentioned may find a familiar experimental demonstration in the drawing-room *fire-screens*, made of plate-glass, which are now not uncommon. Such a screen permits the luminous radiations to pass, but cuts off the hot non-luminous rays, of which most of the calorific radiation consists.

The diathermancy of gases was the subject of a very elaborate investigation in the hands of one of the best known of our English physicists, DR. TYNDALL. The eminence of this gentleman, not only as an investigator, but as one of the ablest and most eloquent of all our expositors of science, will render interesting the following brief notice of his career, extracted from "Men of the Time."

"John Tyndall, LL.D., F.R.S., was born about 1820, in the village of Leighlin Bridge, near Carlow, in Ireland. His parents were in very humble circumstances, but they gave him the best education in their power, and sent him to a school where he acquired a sound knowledge of mathematics. At the age of nineteen he joined in the capacity of "civil assistant" a division of the Ordnance Survey which was stationed in his native town. In 1844 he was engaged by a firm in Manchester, and for about three years he was employed in engineering operations in connection with railways. In 1847 he accepted an appointment as teacher in Queenwood College, in Hampshire, a new institution, devoted partly to a junior school and partly to the preliminary technical education of agriculturists and engineers. Here he became acquainted with Mr. (now Dr.) Frankland, who was resident chemist to the college, and here he commenced those original investigations which have placed him in the foremost rank among the explorers of science. In 1848 the two friends quitted England together and repaired to the University of Marburg, in Hesse-Cassel, where they studied under Bunsen and other eminent professors. Afterwards Mr. Tyndall prosecuted his researches in the laboratory of Magnus, at Berlin. He conducted investigations on the phenomena of dia-magnetism, and on the polarity of the dia-magnetic force, including researches on the magneto-optic properties of crystals and the relation of magnetism and dia-magnetism to molecular arrangement. He has recently published a volume on these subjects. In 1853, having been previously elected a Fellow of the Royal Society, he was chosen Professor of Natural Philosophy in the Royal Institution of Great Britain, and succeeded the celebrated Faraday as Superintendent. The publication of an essay on the cleavage of slate rocks was the proximate cause of his joining his friend Professor Huxley in a visit to the glaciers of Switzerland in 1856; and they afterwards published a joint paper on the structure and motion of glaciers. He returned to Switzerland in 1857, 1858, and 1859, and pursued his investigations, reaching Chamouni on Christmas night, 1859, through deep snow, and two days afterwards succeeded in attaining the Montanvert, where he remained nearly three days, for the most part amid blinding snow, and determined the winter motion of the Mer de Glace. In 1859 he commenced his researches on radiant heat, which have disclosed relations previously unthought of between this agent and the gaseous form of matter. Numerous memoirs published in the 'Philosophical Transactions' are devoted to this subject. In one of them a ray-filter is described, by means of which the luminous rays of the sun, the electric light, and the limelight are detached from the non-luminous ones, combustion and vivid incandescence being effected at foci absolutely dark. Mr. Tyndall is a Rumford Medallist of the Royal Society, and a member of various foreign scientific societies; he was made LL.D. of Cambridge in 1855, and LL.D. of Edinburgh in 1866, when Mr. Carlyle was installed Rector of the University. In 1872 Professor Tyndall went on a lecturing tour in the United States; in the course of it he delivered thirty-five lectures, which returned him $23,100. After paying expenses, a fund of over $13,000 remained, and this, before leaving for Europe, the professor placed in the hands of a committee, who were authorized 'to expend the interest in aid of students who devote themselves to original research.' On the occasion of his receiving the honorary degree of D.C.L. from the University of Oxford, June 18, 1873, Dr. Heurtley, Margaret Professor of Divinity, protested against the proceeding, on the ground that Professor Tyndall 'had signalized himself by writing against and denying the credibility of miracles and the efficacy of prayer, thus contravening the whole tenour of that book, which, with its open page, inscribed "Dominus Illuminatio mea," the University still bears as her device, and therefore still professes to acknowledge as her guide.' Professor Tyndall presided at the annual meeting of the British Association held at Belfast in August, 1874. He accepted the presidency of the Birmingham and Midland Institute for the year 1877. He has written the 'Glaciers of the Alps,' 1860; 'Mountaineering,' 1861; 'A Vacation Tour,' 1862; 'Heat considered as a Mode of Motion,' 1863; 'On Radiation: the "Rede" Lecture, May 16, 1865,' published in 1865; a volume on 'Sound;' 'Faraday as a Discoverer;' 'Fragments of Science;' 'Notes on Electricity,' 1870; 'Notes on Light,' 1871; 'Hours of Exercise in the Alps,' 1871; 'The Forms of Water in Clouds and Rivers, Ice and Glaciers,' 1872; 'Address delivered before the British Association assembled at Belfast, with Additions and a Preface,' 1874; and 'Fragments of Science,' 1876. He married, February 29, 1876, Louisa Claud Hamilton, eldest daughter of Lord and Lady Claud Hamilton."

The apparatus used by Dr. Tyndall in his researches on the diathermancy of gases consisted of a metallic tube 3 inches in diameter and 4 feet long. The ends of the tube were closed with parallel plates of rock-salt (see page 509). A Leslie's cube (page 507), or other source of heat, was placed at one end of the tube, the thermo-pile, with its cone, at the other. The air could be exhausted from the interior tube to any required degree, and various gases and vapours introduced as required. The radiation through the tube was usually balanced by a constant source of heat, so adjusted on the other side of the thermo-pile that the needle of the galvanometer was brought to zero. The effect of any change in the radiation by the introduction of substances into the tube was to destroy this balance and produce a deflection. Some of the results obtained by Dr. Tyndall are very interesting and instructive. The space at our command will not admit of their enumeration. The following, however, may be specified, as they are important in the economy of nature. *Dry* air permits radiant heat to pass as freely as a vacuous space; but aqueous vapour, if present even in very small proportion, powerfully arrests the passage of radiant heat.

When Melloni found that the obscure rays were in a great measure intercepted by glass, while rock-salt permitted all rays to pass without impediment, he caused to be constructed a prism and a lens of rock-salt, with which he repeated Sir W. Herschel's experiments on the heating rays of the solar spectrum. He found the heating effect feeble in the violet, and gradually increasing to the red end of the spectrum. Beyond the red it continues to increase, the maximum is at a point out of the visible spectrum, and the intensity then diminishes. The invisible part of the calorific spectrum is but little inferior in extent to the visible spectrum.

But if there are invisible rays beyond the red end of the spectrum, there are also invisible rays beyond the violet end. Scheele early in 1781 had noticed that chloride of silver is more blackened by the violet than by the red end of the spectrum, and Wollaston (page 480) discovered that this action extended beyond the visible violet. There are therefore in the solar spectrum rays more refrangible than the last visible gleam of violet, and it is precisely these *ultra-violet* rays which exert the most powerful chemical actions. These are the rays which are most active in producing *photographic* impressions, and it is indeed only by this chemical activity that we are acquainted with their existence. The ultra-violet part of the solar spectrum has, like the visible part, numerous gaps, for it is crossed by an immense number of lines. Many hundreds of these are visible in a good heliograph of the ultra-violet spectrum. And as they are to the chemical spectrum what Fraunhofer's lines (page 480) are to the visible spectrum, E. Becquerel has proposed to extend Fraunhofer's nomenclature to the most marked ultra-violet lines by assigning to them the letters L, M, N, O, P, Q, R, S, T. Diagrams have been constructed like that in Fig. 233 to show the gradations of intensity in each class of rays throughout the spectrum. It must not, however, be supposed that in the spectrum three *physically different* spectra are superimposed. Modern theory regards light and heat as physically identical, and if that theory be accepted, the ultra-violet, chemical, actinic, or photographic rays must also be regarded as physically continuous with the rest. They differ only as regards rapidity of vibration. But while vibration-rates between certain limits are capable of so acting on our organs as to produce visual impressions, with colours depending on the rate of vibration, so higher or lower rates beyond these limits are incapable of giving visual impressions, while they are nevertheless competent to produce thermic and chemical effects.

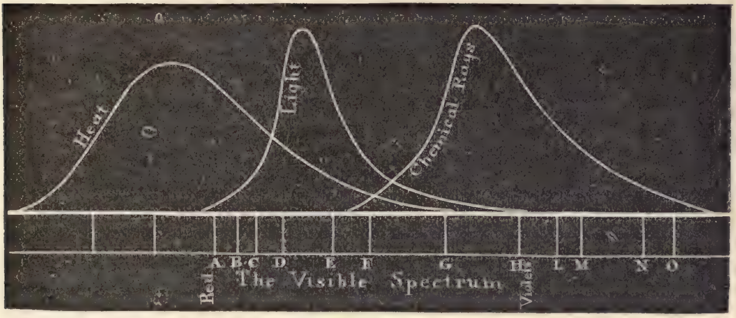

FIG. 233.

The history of the important researches which have been made upon the chemical actions of light would require more detailed treatment of the subject than is here deemed suitable. This subject derives additional interest from its connection with the beautiful art of photography; but in spite of the perfection to which that art has been brought, and the existence of a large store of curious facts relating to the chemical actions of light, no comprehensive theory has yet been brought forward to satisfactorily explain the phenomena.

There is, however, a chemical action due to the solar radiations, which is every day taking place on the grand scale, and exercises the important function of restoring to the atmosphere the oxygen which is constantly removed, combined with carbon, by the respiration of animals, and other natural processes. Bonnet of Geneva, about the middle of the eighteenth century, observed that leaves plunged in water and exposed to sunlight gave off a gas from their surfaces. In 1773 Priestley showed that the gas was vital air, or oxygen, and that plants by this action prevented the vitiation, which would ultimately result from the respiration of animals. Sennebier inferred that the leaves of plants decompose carbonic acid under the influence of sunshine. The whole question was reviewed by Saussure, who made some additions to the facts already known. The French chemists, Dumas and Boussingault, about 1840 applied exact chemical analyses to the determination of the action of vegetables with regard to air, water, and manure, and to the chemical relations of animals to the air and to their food. The result of these researches was the confirmation of the facts that plants decompose carbonic acid derived from the atmosphere, liberating the oxygen and appropriating the carbon, while animals cause the combination of carbon and oxygen. It was found that only the green parts of plants have this power, and only under the influence of sunlight, and it has been supposed that *chlorophyl*, the green colouring matter of plants, is in some way specially concerned in the decomposition of the carbonic acid. It was discovered by Mr. Stokes that chlorophyl exhibits remarkable absorption bands in its spectrum (page 483), and that besides this it exhibits peculiar fluorescent qualities. Dr. Draper of New York placed in the several rays of a solar spectrum narrow tubes, each containing a single blade of grass immersed in water charged with carbonic acid. The spectrum was rendered stationary by means of a heliostat, an instrument in which a mirror is so moved by clockwork that the sun's rays are reflected in one fixed direction, notwithstanding the change of the sun's apparent position. The quantities of oxygen collected in the different rays in one of Draper's experiments were as under:

| | | | | |
|---|---|---|---|---|
| In the | red . . . . . | 0·33 | cubic | centimetres. |
| ,, | red and orange . | 20·00 | ,, | ,, |
| ,, | yellow and green | 36·00 | ,, | ,, |
| ,, | green and blue . | 0·10 | ,, | ,, |
| ,, | blue . . . . | 0·00 | ,, | ,, |
| ,, | violet . . . . | 0·00 | ,, | ,, |

The chemical separation of carbon and oxygen implies the expenditure of energy, and for the source of this energy we must look to the sun's rays. The energy they supply is stored up in the chemical separation, and when the vegetables are consumed as food by animals, these last re-combine the carbonaceous materials with atmospheric oxygen, whereupon the stored-up energy reappears as animal heat and in various physiological and mechanical forces. The animal world is thus dependent for its existence on the vegetable world, and were the latter removed for a single year from the face of the earth, the animal life of our planet would necessarily disappear.

The nineteenth century has witnessed not only the vast accessions to our knowledge of light which have been in part indicated in a preceding chapter, but optical instruments have been the subject of so many improvements, that microscopes and telescopes, of an excellence which 100 years ago would have been considered quite unattainable, are now within the reach of the most moderate means. The obligations of physiology and anatomy to the modern compound microscope are immense. As we have already seen, the simple microscope did wonders in the hands of Leeuwenhoek, and enabled Malpighi, Grew, Swammerdam, and others to discover much ; and although some forms of compound microscopes had been constructed even as early as the beginning of the seventeenth century, they were mere curiosities, for it was not until about the first quarter of the present century that methods were discovered of correcting the spherical and chromatic aberrations of the lenses. In 1821 we find Biot stating "that opticians regard the construction of a good achromatic microscope as impossible," and Wollaston declared that "the compound instrument would never rival the single." However, the subject was pursued by some excellent practical opticians, among whom we may name Fraunhofer at Munich, Amici at Modena, Chevalier at Paris, and M. Selligues. A microscope with an achromatic object-glass of several lenses, made by Chevalier, was presented to the Academy of Sciences in 1824. In the same year Tulley of London succeeded in constructing for the first time in England an object-glass of three lenses. Sir John Herschel, Professor Airy, and Professor Barlow furnished valuable contributions to the theory of the achromatic object-glass. More recently a suggestion of Sir David Brewster's has been carried out by the construction of lenses of diamond. By these and other modern improvements, especially in the mode of illuminating the objects, investigations are now carried into structures so minute that magnifying powers of 2,000 or 3,000 diameters have to be used. The applications of polarizing and spectroscopic apparatus to the microscope were further additions to its powers. The binocular form of construction, though attempted very long ago, was not successfully carried out until 1851. A microscope of this construction, one model of which is represented in Fig. 234, has the advantage of causing less fatigue and strain to the eye, and of showing the relief of objects under low powers.

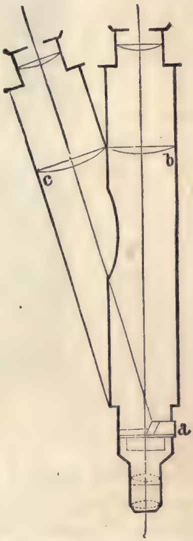

FIG. 234.—BINOCULAR MICROSCOPE.

The great advances in the construction of astronomical telescopes have been particularly alluded to in the chapter recording the recent progress of astronomy. A brief summary of what is there stated may be inserted here for the sake of continuity. The improvements in the manufacture of optical glass have enabled large and perfect discs to be produced, and achromatic object-glasses of a very large size—2 feet and more in diameter—have been constructed. In reflecting telescopes the heavy metallic mirror has always presented many inconveniences in its management. But Foucault, taking advantage of the simple chemical process by which Liebig deposited upon glass surfaces an adherent film of brilliantly polished silver, proposed to construct the mirrors of glass, ground and polished, of the required form, and silvered *in front.* The film of silver retains its brilliancy longer than speculum metal, and has this advantage, that when required it can be renewed in all its original brilliancy. A splendid reflector, having a silvered glass speculum 4 feet in diameter, was a few years ago erected at the Observatory of Paris on this plan. The grinding of the glass mirrors to a true form is a work of much delicacy, and requires special skill. In this country many telescopes with glass mirrrors ground by Mr. With, of Hereford, have been mounted by Mr. Browning, the London optician, who has also fitted up a popular model of this kind of astronomical telescope.

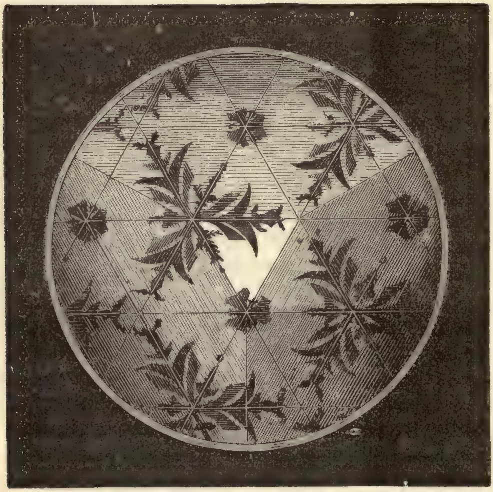

FIG. 235.—THE KALEIDOSCOPE.

As a pendent to the foregoing notice of the improvements which have been effected in the two noblest instruments of optics, we may glance briefly at two optical contrivances which have had an astonishing popularity. They are perhaps too well known to need description. One is the *kaleidoscope*, which was at one time the most popular of scientific toys. Hundreds of thousands were sold in one month in London and Paris when the kaleidoscope was first made in a cheap form. The instrument, though accounted but as a child's toy, is really a very elegant illustration of the principle of symmetry by reflection.

It is formed of two, or better, three plane mirrors inclined to each other at angles of 60°. However irregular may be the form of the small objects placed in one end of the tube of the kaleidoscope, the reflections convert them into beautiful symmetrical patterns. An example is given in Fig. 235, in which the central equilateral triangle contains the real objects. The kaleidoscope was invented in 1816 by Sir DAVID BREWSTER (1781—1853), to whom opticians are indebted for many other ingenious inventions and interesting discoveries.

The *stereoscope* in its ordinary and well-known form (Fig. 236) is also an invention of Sir D. Brewster's. In its original form, invented by Professor Wheatstone (Fig. 236), the pictures were viewed by reflection. The impression of relief in the objects represented in stereoscopic pictures is obtained by the circumstance of each eye seeing a slightly different picture, in which the differences exactly reproduce those of the images to which the real objects would give rise.

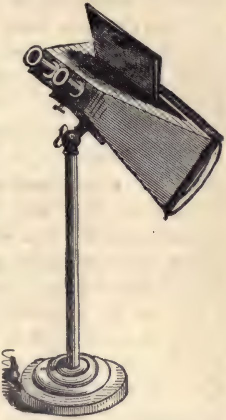
FIG. 236. — BREWSTER'S LENTICULAR STEREOSCOPE.

Since 1872 Mr. Crookes, the discoverer of thallium (page 488), has been engaged in a long series of investigations, which have already revealed to us the existence of peculiar and hitherto unknown conditions of matter. The opening out of a new field of research is always an interesting circumstance in the history of science, and some of the facts which Mr. Crookes has arrived at are so extraordinary that a boundless prospect of the future insight into the most recondite and subtle operations of nature, which may yet be obtained, rises before the imagination. The facts Mr. Crookes has discovered have for the most part been beyond the power of our received theories of light, heat, or "molecular actions" to predict, and the theoretical ex-

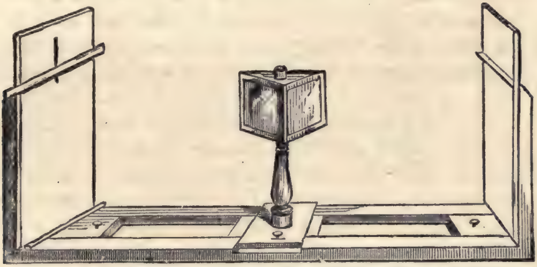
FIG. 237.—WHEATSTONE'S REFLECTING STEREOSCOPE.

planations which have as yet been advanced to account for them leave so many points to be cleared up that these explanations cannot yet be considered as satisfactory. For this reason, and because these matters are still *sub judice* or subjects of controversy among physicists, we shall refer to them as little as possible, but confine ourselves to the facts, in which our readers will hardly fail to recognize the great experimental skill and ingenuity displayed in the conduct of this refined and delicate investigation.

After Mr. Crookes had discovered thallium and examined some of its properties, he undertook to determine with the utmost possible accuracy the atomic weight of the new element. Such determinations, simple as they are in principle, require in practice a multitude of minute precautions extended over a prolonged series of experiments. Thus when substances are to be weighed with scientific accuracy, it is necessary to correct the apparent weight by a calculation for the effect of the surrounding air in buoying up both the weights and the body to be weighed. This requires observations of the barometer, thermometer, and hygrometer to be made at the time of weighing, in order that the weight of a given volume of air may be estimated. The volumes of the weights and of the body to be weighed must also be known. Besides saving the labour in these observations and calculations, there are other great advantages in having a balance working in a vacuum. The weight of a vessel is liable to change from the deposit on its surface of a film of moisture. Further, it is impossible to find, by weighing in the air, the weight of a body having a temperature different from that of the air. Thus if a vessel warmer than the surrounding air were placed in the pan of a balance, the vessel would seem to be lighter than its true apparent weight on account of an ascending current of air which would be produced by the excess of temperature. Now, it was precisely in order that he might be able to weigh in a warm condition certain things required for his thallium determinations, that Mr. Crookes had constructed a delicate balance enclosed in an air-tight metallic case with a glass front. From this case the air could be exhausted. In this apparatus Mr. Crookes observed some curious results, particularly that a body appeared to be lighter when warm, notwithstanding the vacuum. He then made special experiments to render the action, if possible, more evident, and to clear away certain sources of error. He was led to devise apparatus of a more and more delicate kind, by which he was at length enabled to demonstrate at will, that radiant heat and light acting on bodies within vacuous vessels were capable of producing motion in visible masses.

The power of light and of heat to impart motion had been previously the subject of but few experiments. Mr. A. Bennett in 1792 had tried in vain to make a delicately suspended beam turn under the influence of light concentrated on one of its arms. One or two physicists of the present century have stated that bodies of different temperatures attracted each other. Other observers have spoken, on the other hand, of a repulsive force between such bodies.

Like many other new phenomena, those presented by vacuous spaces became the subjects of possible investigation by reason of improvements in instrumental appliances. It was not until means had been devised for carrying the exhaustion of vessels to a far higher point than could be attained by means of the ordinary air-pump, that the beautiful phenomena produced by the electrical discharge in Geissler's tubes (Chap. XX.) could be studied. Geissler invented the first arrangement by which vessels could conveniently be exhausted by communication with a Torricellian vacuum. Now, the amount of air remaining in the receiver of a good air-pump of the ordinary kind is capable of sustaining the pressure of a column of mercury $\frac{1}{20}$th to $\frac{1}{50}$th inch high, after the pump has been worked to the utmost of its power. With Geissler's machine, however, a vacuum corresponding with only $\frac{1}{250}$th inch may very easily be obtained. This implies that only $\frac{1}{7500}$th part of the original air remains in the vessel. The mercury apparatus was improved by Sprengel, and his apparatus has with slight modifications been always used since, in order to obtain the highest degree of rarefaction. Fig. 238 will illustrate the principle of the Sprengel Pump by exhibiting the simplest form of the apparatus. A B is a strong glass tube with a bore not exceeding $\frac{1}{10}$th of an inch. The height of this from A to B must be at least 3 feet. Its lower end dips a very little below the level of the mercury in the vessel C, which has a spout D, from which the mercury falls into another vessel E. The top of the tube is connected with the funnel F by a short india-rubber tube, which can be compressed by the clamp G when required. At A the tube H A opens obliquely, and at H this tube can be connected with the vessel to be exhausted, I. When mercury is poured into the funnel F, it exhausts air at A, and the whole length of A B may be observed to be filled with lengths of mercury separated by air-spaces, all moving down into the vessel B, whence the air and the mercury escape by the spout D. From time to time the vessel E is replaced by another, while the collected

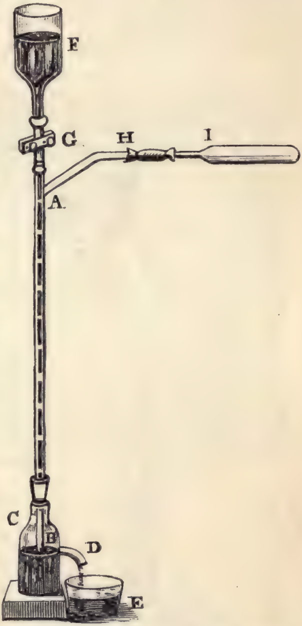

FIG. 238.—THE SPRENGEL PUMP.

mercury is passed back into F. The lengths of mercury in A B act as so many pistons, and as the exhaustion proceeds, the air-spaces between them are observed to get smaller and smaller, and at length the descent of the mercury takes place with the clicking sound peculiar to all liquids shaken in a vacuum. When the mercury falls without enclosing the smallest bubble, if the tube at G be clamped, the column in A B will stand at the full height of the barometer. The exhaustion can by this apparatus be carried beyond the millionth part of an atmosphere, and even the $\frac{1}{1{,}300000}$th part has been attained. The most perfect vacuums possible are, however, procured by using tubes filled with carbonic acid gas, and after this has been exhausted as far as possible by the Sprengel pump, the residual gas is absorbed by caustic potash.

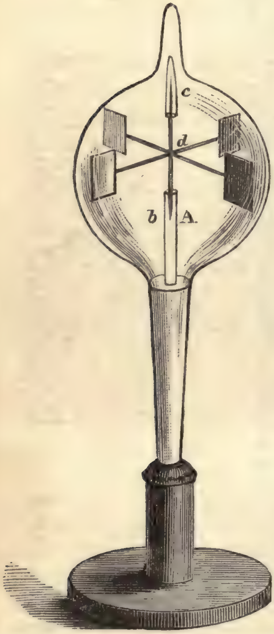

FIG. 239.—THE RADIOMETER.

It was in general in vacuums obtained by use of the Sprengel pump that most of Mr. Crookes's experiments were conducted. He was led on to try one form of apparatus after another. One of the earliest forms had a straw carrying a pith ball at each end, balanced in the tube on pivots formed by passing through the straw, a little above the centre of gravity of the system, the pointed end of a fine needle broken off a very little shorter than the interior diameter of the containing tube, and pointed very finely at the broken end. The needle was thus supported very delicately by its points against the sides of the tube, and with the least possible friction. This form of experiment was superseded by an instrument on the same principle as the torsion balance (p. 335). The straw beam was supported horizontally by means of a single fibre of cocoon silk, or of an extremely delicate filament of glass, and the movement was indicated by a reflected ray of light, as in Thompson's "reflecting galvanometer." The horizontal beam was made to carry various substances, and the amount of the deflecting force under assigned circumstances was noted in a great number of cases. Finally, Mr. Crookes was led to a form of apparatus which has since been the subject of many experiments, and is now well known as the "Radiometer." Its ordinary form is represented in Fig. 239. A is the exhausted vessel, in which a vertical spindle *c b* turns freely upon its point at *b*. The spindle carries four arms at right angles to each other, and the extremity of each arm bears a small vertical piece of metallic foil, one surface of which is covered by lampblack, while the other is polished, and the several blackened and polished surfaces are respectively turned one way. When radiant light or heat falls upon this apparatus, its effect is that the little mill begins to rotate in the same direction as if the radiant light or heat *repelled the blackened surfaces* of the vanes. The velocity of rotation is proportional to the intensity of the radiation which falls upon the instrument. It has accordingly been proposed to employ the radiometer to measure the intensity of light, instead of the ordinary photometrical methods depending upon the estimation by the eye of equality of illumination.

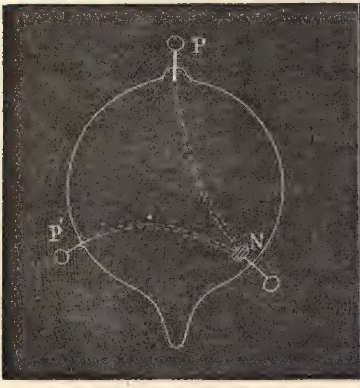

FIG. 240.

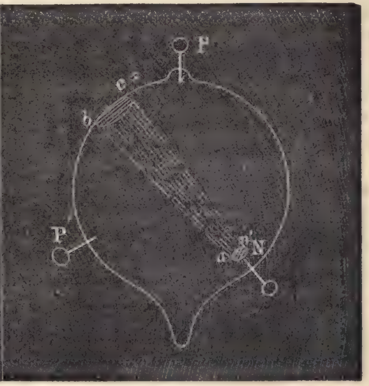

FIG. 241.

In following out the investigations which these discoveries suggested, Mr. Crookes was led to certain experiments on the passage of electricity in extremely attenuated media. On account of their relation to the other vacuum experiments, the results of those experiments may be mentioned in this place rather than under the head of electricity, and also because the phenomena recall certain properties of radiations.

Let Fig. 240 represent a bulb exhausted by a Sprengel pump. Platinum wires are sealed into the tube at the points P P' and N, the latter terminating inside in a small flat surface. N is connected with the negative pole of a Ruhmkorff's induction coil (Chap. XX.), and then, according as P or P' is connected with the positive pole, the discharge takes the form of a luminous track between P N, or between P' N; or both, P and P' may be simultaneously connected with the positive pole, when the discharge will take the forms represented in the figure. Fig. 241 shows an exactly similar bulb, in which, however, the exhaustion has been carried to the $\frac{1}{1000000}$th of an atmosphere. When the connections are made as before, no luminous track is visible between the positive and negative poles, and whether the positive pole be at P or P′ the appearances are precisely the same. But from N a very faint bluish light, *a b c n′*, passes straight across the bulb in a direction perpendicular to the surface *a n′*. This beam widens a little as it recedes from N, and if the bulb be made of glass tinged with oxide of uranium, the place where the beam impinges upon the interior of the bulb is marked by a circle of phosphorescent light. The position of this circle and the direction of the faint blue illumination are quite independent of the position of the positive pole; but they are changed on the approach of a magnet. The action of the latter is, however, markedly different from that which it exerts on the *current* indicated by the luminous track in inferior vacuums. Also whereas in the latter the track follows all the sinuosities of a bent tube, the faint blue light streaming from the *negative* pole in the very highly exhausted tube passes in straight lines only, and refuses to turn a corner.

This question then presents itself: Is the faint blue light due to a *current* of electricity, or is it a stream of negatively electrified material particles shot off from the negative pole? The solution devised by Mr. Crookes, of this problem, is as conclusive as it is elegant. The experiment was arranged as shown at Fig. 242. A is the exhausted tube, with a platinum wire sealed in at *p* to form the positive pole, while at the other pole there are two negative poles *n* and *n′* exactly similar to each other. At a short distance from these is a metallic screen perforated by two small apertures *a a′*. When *n or n′* is the negative pole, the faint blue light streams through the aperture parallel to the axis of the tube. But the moment *both n* and *n′* are made negative poles, the beams take the divergent directions *a c* and *a c′*. Now, if these two streams conveyed currents of electricity, the currents being parallel, would, according to Ampère's law (Chap. XX.), *attract* each other. On the other hand, the observed divergence is precisely what ought to be seen in the case of a stream of negatively electrified particles.

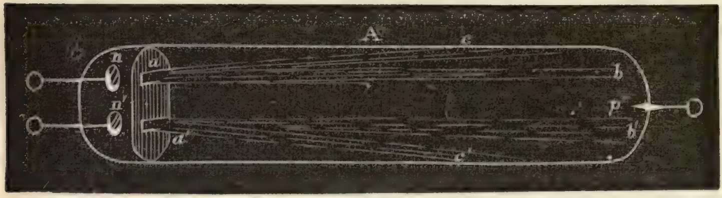

FIG. 242.

When the negative pole is concave instead of plane, the stream radiating from it converges to a focus. If, at this focus, a strip of platinum is placed, it is as quickly raised to a glowing temperature. The stream can also be made to produce such mechanical movements within the tube as would be caused by a stream of particles discharged from the negative pole.

We pass on now to record some of the most important scientific work of the present century, namely, the establishment of the *Dynamical Theory* of heat, and the determination of the "*Mechanical Equivalent.*" Dr. Young, in commenting upon Davy's experiment of rubbing two pieces of ice together (page 310), pointed out in 1807 that the heat in this case cannot be received from surrounding bodies, as no fall of their temperature occurs, nor can it be derived by any conceivable diminution of "capacity for heat" from heat accumulated in the ice. It must therefore be admitted that the heat is actually generated by the friction. If it be thus generated it cannot be material, for this would amount to the creation of matter out of nothing. As heat cannot therefore be a substance, it must be a quality, and this quality can only be motion. It was indeed Newton's opinion (and had been that of others) that heat consists in a minute but rapid vibratory movement of the particles of bodies. This motion may be communicated through an apparent vacuum by undulations of the same elastic medium which is concerned in the phenomena of light. "If the arguments which have been lately advanced in favour of the undulatory nature of light be deemed valid, there will be still stronger reasons for admitting this doctrine respecting heat, and it will only be necessary to suppose the vibrations and undulations principally constituting it to be larger and stronger than those of light, while at the same time the smaller vibrations of light, and even the blackening rays derived from still more minute vibrations, may perhaps, where sufficiently condensed, concur in producing the effects of heat. These effects, beginning from the blackening rays, which are invisible, are a little more perceptible in the violet, which still possesses but a faint power of illumination; the yellow-green afford the most; the red give less light, but much more heat; while the still larger and less frequent vibrations which have no effect on the sense of sight, may be supposed to give rise to the least refrangible rays, and to constitute invisible heat." Young considered that according to this view of heat its phenomena had in several respects much analogy to those of sound.

In spite of the clear and forcible manner in which Young showed the inadequacy of the doctrine that regarded caloric as material or quasi-material, that theory prevailed, and was generally entertained by chemists and physicists until nearly the middle of the present century. The reason of this may be found in the facility and convenience with which it lent itself to the expression of the phenomena of latent and specific heat. Before the new doctrine of heat was systematically formulated, however, there were not wanting speculations on the connection and mutual convertibility of heat and mechanical action.

Stephen Montgolfier, the celebrated inventor of the fire-balloon, appears to have conceived the idea of a certain equivalence between heat and mechanical work. His nephew, MARC SÉGUIN, in a book on railways published in 1839, developed the uncle's ideas. But in 1842 the relation between heat and work was for the first time reduced to a quantitative form by Dr. J. R. MAYER, a physician of Heilbronn in Germany. Mayer was the first to use the expression "equivalent of heat," the exact meaning of which we shall presently have to show. His first paper lays down the general principle of the indestructibility of force, and the mutual convertibility of the different forms of force. Mr. (now Justice) GROVE was, in the same year, independently enunciating in England similar views on the relations of all the various forces of nature. These views were afterwards extended, and embodied in Mr. Grove's treatise on "The Correlation of the Physical Forces." The teaching of both Mayer and Grove did not, however, rest upon that solid basis of experimental demonstration which is needed to establish definitely a great principle like that of the Conservation of Force. Mayer had, indeed, calculated the value of the "equivalent of heat" from the difference between the specific heat of gases when the volume is maintained unchanged, and the specific heat when the gases are allowed to expand under a uniform pressure. The extended basis of experimental truth, upon which alone the Conservation of Force could be upheld as a demonstrated law of nature, instead of a mere speculation, was supplied by Mr. JAMES PRESCOTT JOULE, of Manchester, in the course of six years of laborious research. Mr. Joule's investigations were conducted altogether independently, and though the results were published from time to time, they were at first coldly received, and for some years attracted little attention. In the Report of the British Association for the meeting at Cork in 1843, there appears in a dozen lines a brief abstract of Mr. Joule's first paper on the subject.

While Joule was working at his experiments, Mayer was boldly applying his theory to the explanation of the mechanical force exhibited by animals, and to other phenomena. He published in 1845 a paper on Organic Motion, and in 1848 an essay on Celestial Dynamics, in which last work he propounded the celebrated meteoric theory of the sun's heat. This theory supposes that the intense heat of the sun is maintained by the impact of meteoric stones drawn into it by its gravitative attraction. Sir William Thompson and others have since fully developed this theory.

The prominent positions which must be assigned to both Mayer and Joule in connection with the subject we are now considering have been thus discriminated by Professor Tyndall:—"It is not his experiments alone, but the spirit which they incorporate, and the applications which their author made of them, that entitle Mr. Joule to a place in the foremost rank of physical philosophers. Mayer's labours have, in some measure, the stamp of a profound intuition, which rose, however, to the energy of undoubting conviction in his mind. Joule's labours, on the contrary, are an experimental demonstration. Mayer *thought out* his theory and rose to its grandest applications; Joule *worked out* his theory and gave it the solidity of natural truth. True to the speculative instinct of his country, Mayer drew large and weighty conclusions from slender premises; while the Englishman aimed above all at the firm establishment of facts. The future historian of science will not, I think, place these men in antagonism. To each belongs a reputation which will not quickly fade for the share he has had, not only in establishing the dynamical theory of heat, but also in leading the way towards a right appreciation of the general energies of the universe."

Mr. Joule's paper "On the Calorific Effects of Magneto-Electricity, and on the Mechanical Value of Heat," appears *in extenso* in "The Philosophical Magazine" for 1843. The first part of the paper related to the heat generated in a magneto-electric circuit. An electro-magnet contained in a tube filled with water was rotated between the poles of a powerful magnet, and the increase of temperature of the water was observed. It then became a question of interest to find whether a constant ratio existed between the heat developed and the mechanical force used in turning the apparatus. The mechanical force was found by winding twine round the spindle carrying the electro-magnet, passing the twine over an easily-moving pulley, and attaching a scale pan. The difference between the weights required to produce equal velocities with the induced circuit complete and interrupted, gave the work done against the electric forces which reappeared as heat. Under the circumstances of these experiments there were many attendant interfering effects to be considered, and it was therefore almost inevitable that the results should be somewhat discordant among themselves. The mean result of the thirteen experiments recorded in the paper is thus stated: "The quantity of heat capable of increasing the temperature of a pound of water by one degree of Fahrenheit's scale is equal to, and may be converted into, a mechanical force capable of raising 838 lbs. to the perpendicular height of one foot." A very much simpler apparatus, consisting of a piston perforated by a number of small holes, working in a cylindrical glass jar, is mentioned in a postscript. The mechanical force required to urge the piston through the water was such, that one degree per lb. of water corresponded with a force capable of raising 770 lbs. to a height of one foot. "I shall lose no time," adds Mr. Joule, "in repeating and extending these experiments, being satisfied that the grand agents of nature are, by the Creator's fiat, *indestructible;* and that wherever mechanical force is expended, an exact equivalent of heat is *always* obtained." He applies this last view to the case of chemical combination by suggesting that heat there results from the collision of atoms rushing together. In their separated positions the atoms have a power latent in them—a mechanical power ready for action like that of a wound-up watch-spring.

In 1845 Mr. Joule estimated the heat developed when air is compressed. The apparatus employed was very simple in principle, consisting merely of a strong copper vessel and a pump for forcing air into it up to a pressure of more than 20 atmospheres. The pump and receiver were both immersed in a large vessel of water, and a very delicate thermometer was employed in ascertaining the temperatures of the water. It will be unnecessary to detail here all subsidiary precautions and corrections employed in the conduct of the experiments, as, for instance, how the air was supplied to the pump dry and of uniform temperature; how the effects of friction were eliminated, etc. The results gave 795 foot-lbs. as the mechanical equivalent of the heat of a pound of water raised one degree. A very ingenious experiment was devised to prove that the mere change in the position of the particles of the air—their greater or less approximation—does not of itself involve any development or absorption of heat. Two copper receivers, V, C, Fig. 243, were provided, which could be connected together by a pipe provided with a stop-cock, *a*, of a construction suitable for high pressures. Into the receiver C dry air was condensed to about 22 atmospheres, and on the other hand V was completely exhausted. The two receivers were then connected by a coupling-piece, *b*, and placed in a tin vessel containing water. The temperature of the water was ascertained, and then communication was opened between the two vessels. The air of course rushed from C into V until the pressures within the two vessels had become equal. The temperature of the water was again noted and found to have undergone no change. The experiment was modified by placing the charged vessel, C, in one pan, and the vacuous vessel, V, in another. When equilibrium had been restored by opening the stop-cocks, it was found that the temperature of the water surrounding the vessel from which the air had expanded had fallen 2°·38, while that surrounding the other receiver had gained 2°·36; that is to say, the quantity of heat lost by one receiver had been acquired by the other. In other words, the total heat produced or absorbed by the operation was *nil*. The compressed air, expanding into a vacuum to double its former volume, had performed *no mechanical work* in reaching the state of equilibrium; nor had any *work* been expended upon it by any external agent: hence there was neither loss nor gain of heat. On the other hand, when one of the receivers containing condensed air was placed in a vessel of water, and communication opened (under proper conditions) with the external atmosphere, the fall of the temperature of the water showed a loss of heat. In this

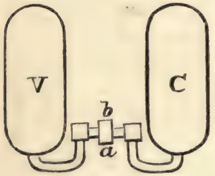

FIG. 243.

case the air in leaving the receiver performed work, for by expanding against the pressure of the superincumbent atmosphere, it acted precisely as if it had raised a weight exerting a pressure upon it of 15 lbs. to the square inch. The mechanical equivalent of the unit of heat, as calculated from this experiment, agreed well with the former determinations. Mr. Joule, in concluding the paper in which these experiments are detailed, very clearly points out the application of his principles to the theory of the steam-engine. "From them," he says, "we may infer that the steam, while expanding in the cylinder, loses heat in quantity exactly proportional to the mechanical force which it communicates by means of the piston; and that, in the condensation of the steam, the heat thus converted into power is *not* given back. Supposing no loss of heat by radiation, etc., the theory here advanced demands that the heat given out in the condenser shall be less than that communicated to the boiler from the furnace, in exact proportion to the equivalent of mechanical power developed."

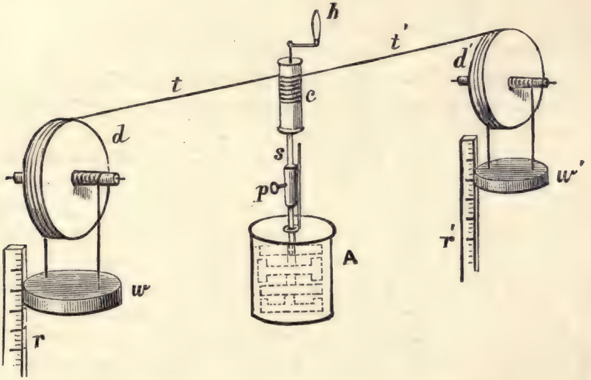

FIG. 244.—JOULE'S APPARATUS.

Another method which Mr. Joule adopted for the determining of the mechanical equivalent will be understood by the diagram Fig. 244, in which the apparatus is represented. A is a vessel which may be filled with water, oil, mercury, or other liquid. The spindle *s* passes through the cover of the vessel and carries four vanes at right angles to each other. These vanes are cut out or shown by the dotted lines, and they pass through corresponding apertures in eight fixed radial partitions. By this means the liquid is prevented from whirling round with the rotating paddles. The spindle *s* carries in its upper part a cylinder *c*, which turns with it or without it, according as the peg *p* is inserted or withdrawn. The cords, *t t'*, attached to the drums, *d d'*, are wound upon the cylinder, and the descent of the weights, *w w'*, therefore cause the rotation of the spindle. The height through which the weights descend is measured by the rod, *r r'*, and the operation is several times repeated, the spindle *s* being only turned by the *descending* weights. A very delicate thermometer, *h*, indicates the temperature of the liquid. The value found by experiments in 1847 was 781·8 foot-lbs. Several years' experience in experiments of this kind enabled Mr. Joule to apply to a series, completed in 1849, all possible refinements and corrections, and the mean results of more than 100 determinations were

| | | | |
|---|---|---|---|
| 772·69 | from the | friction of | water, |
| 774·08 | ,, | ,, | mercury, |
| 774·98 | ,, | ,, | cast iron. |

He finally fixed upon 772 foot-lbs. as the mechanical equivalent of the heat which will raise the temperature of a pound of water one degree Fahrenheit between 55° and 60° F.

Quite recently (1879) Professor Rowland, of the newly-founded *John Hopkins University* at Baltimore, U.S., has conducted a series of experiments to re-determine the mechanical equivalent by an arrangement invented by himself. The calorimeter with its fixed and revolving paddles, are like those in Joule's apparatus (Fig. 244); but the work done on the water is determined in this way: the spindle carrying the revolving paddles is turned by power derived from a steam-engine, and the number of revolutions is automatically indicated. The tendency of the vessel itself to rotate is, in Rowland's apparatus, opposed by cords that support weights, and these therefore furnish the measure of the mechanical force applied to the water. The water in this apparatus may be raised to the boiling-point by the mere churning action of the paddles. The numerical results are nearly identical with those of Joule; for the mechanical value of a pound of water raised from 80° to 81° F. was found by the Baltimore experiments to be 775·7 foot-lbs.

The principle of a *mechanical equivalent* of heat, suggested by Grove and Mayer, and firmly established on an experimental basis by Joule, is far-reaching and of vast importance. It gave to science not merely a new conception, but a law which, worked out in its consequences and relations, was found to include in its highest generalization the widest laws of all the several branches of science. The result has been that within the last thirty years each branch of science has been found capable of presenting a new aspect in its relation to other branches. They are now connected and controlled by the great laws of *energetics*, which assert the constant inter-equivalence of different forms of force, and by the law of the *Conservation of Energy*. The scientific meaning of the term *energy* should be clearly understood. Energy may be defined as the *power of doing work*. Work is another term of which the scientific sense may require to be mentioned. The labouring power of a man or of a horse has long been for practical purposes expressed by engineers and others in terms of the loads that the muscular effort of either could raise to some specified height in a given time. The invention of the steam-engine was the occasion of bringing into more notice this conception of forces doing work. Watt estimated the efficiency of his engines by their "*horse-power*." Not that the engine was directly compared with the actual labouring power of horses, but the "*horse-power*" was arbitrarily defined as that which would in one second raise 550 lbs. one foot high. In the scientific sense, *work is done when anything is moved against a resistance, and the work is measured by the space through which the thing is moved, and by the amount of the resistance.* Time does not enter at all into the measure of work. The space may be measured in feet, and the resistance may be compared with that which is overcome when the lump of metal called a pound weight is raised vertically. Hence the compound unit of work (in England) is the "*foot-lb.*" The expression "100 units of work" would therefore imply 100 lbs. weight raised 1 foot; 1 lb. raised 100 feet; 10 lbs. raised 10 feet; 20 lbs. raised 5 feet; 5 lbs. raised 20 feet; or, in fine, a weight raised to such a height that the product of the weight (expressed in lbs.) into the height (expressed in feet) is 100.

Joule's experiments proved that by the expenditure of 772 units of *work* one unit of heat can be produced, and with greater generality they also proved that chemical action, electricity, magnetism, heat, and mechanical energy are convertible into each other in definite and invariable equivalents. Meanwhile the relations of heat and motive power were being studied from the theoretical side by HELMHOLTZ and CLAUSIUS in Germany, and by WATERSTON, RANKINE, W. THOMPSON, MAXWELL, and others in England. The laws of these relations are now formulated in a branch of science denominated as *Thermo-Dynamics*, but their discovery and statement cannot here be discussed at length. One important law of *thermo-dynamics* was enunciated as early as 1824 by CARNOT, a son of Napoleon's famous general of that name, in a work entitled "*Réflexions sur la Puissance Motrice du Feu.*" This law holds good now, although the reasonings on which it is founded were based on the then prevailing view of the material nature of *caloric.* Carnot compared the mechanical power of heat to the mechanical power of a head of water; for, just as work is done by the latter only in its descent from a higher to a lower level, so, he declared, work done by heat is necessarily attended by a fall of some body *from a higher to a lower temperature.* Whatever might be the nature of the substance, whether steam, air, vapour, gas, or liquid, this fall of temperature Carnot showed to be essential to the production of work, and the amount of work done by the heat to be independent of the nature of the substance employed.

The acceptance of the theory which regards heat as the motion of the particles of bodies furnishes a remarkably direct and satisfactory explanation of the facts of thermo-dynamics and of many physical properties of bodies, more especially of gases. The older conception of a gas regarded it as constituted of material particles kept apart by surrounding repulsive atmospheres of a highly elastic imponderable fluid called *caloric*. The dynamical theory represents gases as formed of particles moving freely in space according to the ordinary laws of mechanics. The particles move in straight lines in all directions, with doubtless different velocities, but their average velocity under defined circumstances would also be definite. Such of the particles as encounter others will have the direction of the movement changed, but the time of the moving forces of the whole will remain unchanged. The same will be true of particles striking the wall of an enclosing vessel. But the shocks of the impacts against the latter succeeding each other with great rapidity and equally in every direction, explain the pressures exercised by gases. The increase of temperature when a gas is made to occupy a smaller bulk—when, for instance, being contained in a cylinder, it is compressed by a piston—is accounted for by the communication of the movement of the piston to the particles. The like, *vice versâ*, is true in the case of a gas allowed to expand. In fact, the dynamical theory explains with great readiness and clearness not only Boyle's law (page 231), but the laws of the expansion by heat, specific heats, diffusion, and other facts relating to gases.

The laws according to which gases expand by heat have been the subject of many elaborate experimental investigations. GAY-LUSSAC (1778—1850), an eminent French chemist, summed up the results of his experiments and those of others in the three following propositions, which are often called Gay-Lussac's Laws:—

1. *All gases expand equally by heat.*
2. *Their expansion is independent of the pressure.*
3. *One volume of any gas taken at the freezing-point of water becomes* 1·375 *volumes at the boiling-point.*

The subsequent experiments of M. REGNAULT (born 1810) showed that these laws are true only approximately. All gases do not expand alike, for the nearer to the degree of pressure and temperature at which a gas becomes a liquid the experiment is made, the more expansible is the gas. Thus the expansion, instead of being ·375 for all gases, as stated by Gay-Lussac, was found by Regnault to be for hydrogen ·366, for air ·367, for carbonic acid ·371, and for sulphurous acid gas ·390. Faraday succeeded in liquefying by cold and pressure many gases which had not previously been condensed, and the half-dozen gases that had resisted all his efforts have quite recently been reduced to the liquid state by MM. Cailletet and Pictet. This result is of importance, as it has completely proved the generality of the recognized laws by actually removing the apparent exceptions. Some very remarkable experiments by Professor Andrews have shown, too, that matter may exist in a condition intermediate between the gaseous and the liquid states, and that, in fact, there is a continuity in its passage from the one to the other.

The absorption of heat attending the vaporization of liquids has in recent times been practically applied in the production of artificial cold. Thus the apparatus of M. Carré, which is merely a simple but very efficient air-pump, quickly produces iced water by means of the cold attending the evaporation of the liquid itself in a vacuum. The same inventor has applied Faraday's plan of condensing gases to the obtaining of a liquid that on resuming the gaseous state rapidly absorbs heat. The apparatus is represented in Figs. 245 and 246. A is a strong wrought iron vessel partly filled with a concentrated solution of ammonia, and connected by the tube G with an annular space surrounding the vessel D, and the whole is hermetically closed. At the beginning of the operation all the liquid is contained in A, which is gradually heated as shown in Fig. 245, while D is plunged in a cistern of cold water E. The ammonia then condenses into a liquid in D, and when A is cooled as in Fig. 246, the pressure which caused its condensation being thus removed, it rapidly returns to the gaseous state and is absorbed by the water in A. This change of state is ac-

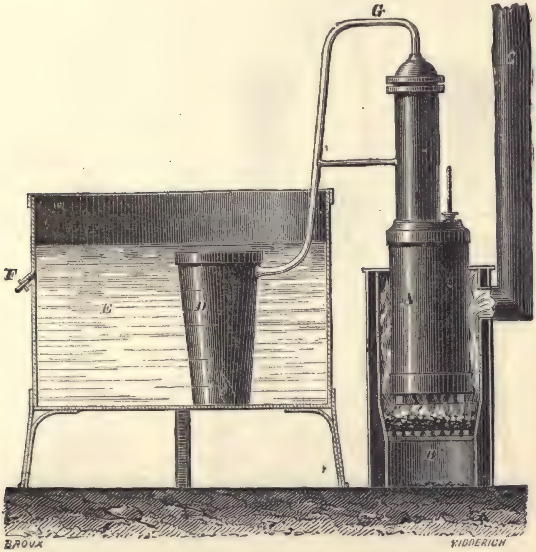

FIG. 245.

companied by a great absorption of heat, and water in a cylindrical vessel C put in the hollow part of D is quickly frozen. This action is explained on the dynamical theory of heat by the transference of the molecular motions of the vessel D and its contents to the particles of the liquid ammonia in their passage to the gaseous state. The particles of bodies at all temperatures above ·273° Cent. are supposed to be in constant movement with greater or less velocity according to their temperature. It is their loss of movement by the impulses taken up by the ammonia particles that gives rise to the effects we denominate cold.

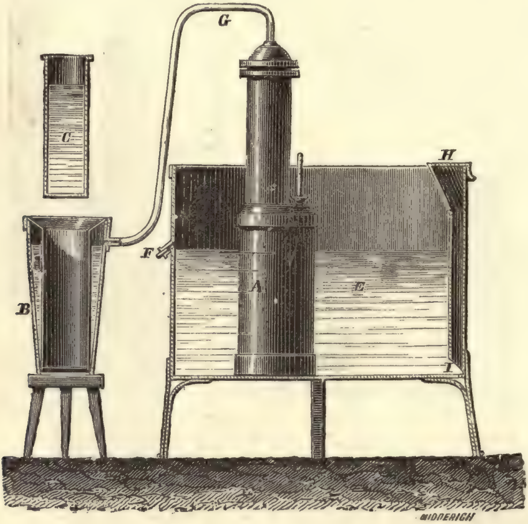

FIG. 246.

A singular class of phenomena connected with the vaporization of liquids was made known by LEIDENFROST, and in 1843 was further illustrated by BOUTIGNY, who supposed that they were due to some hitherto unrecognized condition that liquids were capable of assuming, and denominated by him the *Spheroidal State*. If a silver vessel be treated as represented in Fig. 247, and a drop of water from a pipette be allowed to descend upon it, the drop will not flash into steam, and will not touch the metal, but will glide over its surface like a drop of dew on a cabbage-leaf. In fact, an attentive observation will reveal to the eye a distinct interval between the drop and the hot metal. When water is dropped into a nearly red-hot silver dish, a considerable quantity may be introduced without the liquid wetting the dish or reaching the boiling temperature. It rolls about in the dish and evaporates quietly. It is then, according to Boutigny, in the "*spheroidal state.*" The true explanation of this experiment was given by Sir W. Armstrong, who showed that the liquid received heat from the metal by *radiation* and not by *contact*, for it rests upon a stratum of its own vapour, continually renewed, as upon an elastic cushion. It is the escape of the vapour from beneath the liquid which occasions the movements, and the metal must be at a sufficient temperature to maintain the layer of vapour, otherwise the drop of water comes into contact with the vessel and is instantly converted into steam.

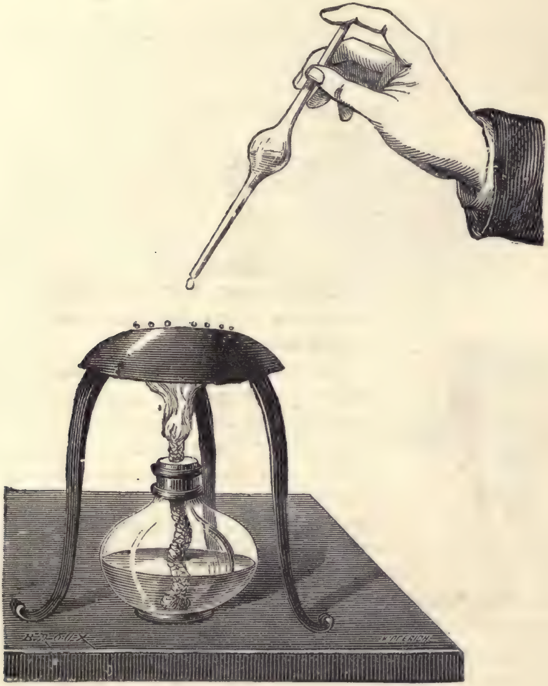

FIG. 247.

Scarcely any discoveries in the science of Acoustics have yet been noticed in these pages. Those of Pythagoras (page 14) and Galileo's researches (page 164) are indeed almost the only steps in the progress of this science that have yet been referred to. As the present century has witnessed an advance in acoustical science beyond all comparison greater than that of former times, the few brief notes which must suffice to mark the course of the science have been, for the sake of continuity, reserved for presentation in this place. Many of the researches belonging to this science are of a highly mathematical form, and therefore unsuited for our purpose. Such were the investigations of Newton, Leibnitz, the Bernouillis, Euler, D'Alembert, and others. Passing by all these deductive investigations, we shall mention some experiments of importance in the development of our knowledge of Sound.

The part played by the air in the conveyance of sound from the sounding body to the ear was, soon after the invention of the air-pump, experimentally proved by Hawksbee. The now well-known experiment is represented in Fig. 248, where on the plate of the air-pump is seen a case containing clockwork, resting at B B on felt supports to prevent the vibrations passing to the plate of the pump. The clockwork causes a hammer to strike the bell T, and when this is done in the exhausted receiver, scarcely any sound reaches the ear; but when the air is admitted, the bell is heard, faintly at first, but soon with its full power.

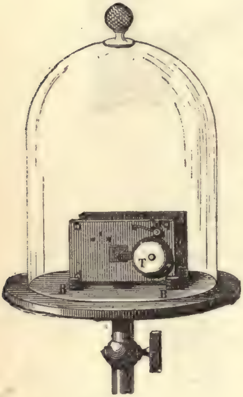

FIG. 248.

The vibrations of strings were theoretically and experimentally investigated about the beginning of the eighteenth century by Newton and by Euler, and by Sauveur (1653—1716). In Sauveur's life there is a curious circumstance that, in relation to the researches for which he became famous, appears still more curious. The first seven years of his life he was quite dumb, nor did his vocal organs ever attain their full development. The man who first counted the actual numbers of the impulses which give the notes of the musical scale had himself neither voice nor ear, and in his experiments on the vibration rates of strings of different lengths, tensions, etc., he had to rely upon the assistance of friends for the discrimination of consonances and dissonances. Sauveur was originally destined for an ecclesiastical career, although early in life he had manifested great delight in mechanical contrivances and arithmetical computations. A copy of Euclid's "Elements of Geometry," which the youth accidentally met with, determined the course of his after-life. He abandoned the training which was to qualify him for the Church, and having in consequence lost the support of his relatives, he obtained a livelihood by teaching mathematics. By good fortune he found an appreciative patron, through whom he obtained in 1686 an appointment as professor of mathematics. During the remainder of his life, the study of acoustics, and particularly the scientific theory of music, occupied much of his attention. Many of his valuable papers on these subjects were published in the "Memoirs" of the Academy of Sciences between the years 1700 and 1714.

The theoretical velocity of sound in various media was determined by Newton. An interesting experimental investigation was that in which Colladon and Sturm in 1826 determined the velocity of sound in water. The experiments were conducted in the Lake of Geneva. A bell was struck under water at the same instant that a little gunpowder was fired, and the observer at a distance seeing the flash, for the experiments were made at night, counted the number of seconds which elapsed before he heard the stroke on the bell. The arrangement of the apparatus is shown in Fig. 249, where C is the bell, M the hammer by which it is struck, moved by means of a lever. The same movement of the lever which causes the hammer to strike the bell brings the lighted match M to the gunpowder at P, which is thus fired at the very instant of the stroke. The distant observer applies his ear to an ear-trumpet at O, the expanded end of which, T, is submerged, and collects the sound. The velocity of sound in water as given by these experiments is 4,708 feet per second.

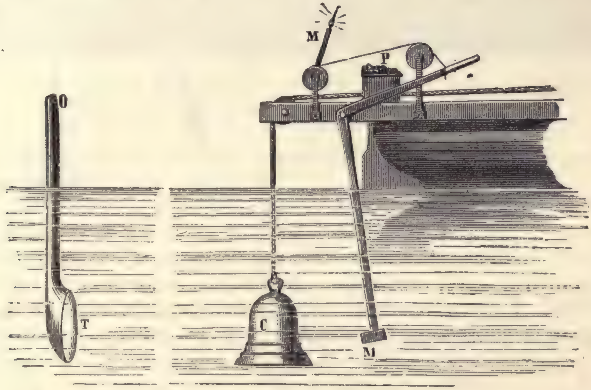

FIG. 249.

The vibrations of a stretched cord can be followed by the eye when they do not exceed about twelve per second. More rapid vibrations give the appearance of a spindle-shaped nebulosity, as suggested by the diagram Fig. 250, which represents a cord vibrating as a whole.

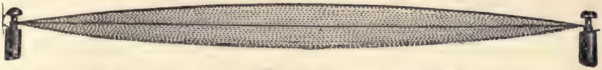

FIG. 250.

In general, however, a cord does not thus vibrate, but divides into a number of equal parts, as shown in Fig. 251, which represents the method of causing a stretched wire to divide into several vibrating segments. The tip of the finger is gently pressed at some point of aliquot division as at $\text{K}_2$, where $\text{K}$ $\text{K}_2$ is one-third of the whole length. When a violin-bow is drawn across the middle of the segment, as at $\text{S}_1$, the wire divides itself into the segments $\text{S}_1$, $\text{S}_2$, $\text{S}_3$, and the stationary points, $\text{K}_2$, $\text{K}_3$, are called the *nodes*. A method of exhibiting the positions of the *nodes* in a vibrating wire was devised by Sauveur (1701), and is shown by Fig. 252, where we have represented the division of a wire into *four* vibrating segments. Little pieces of paper are first placed astride the wire in various places. When the vibrations are excited by drawing the bow across the middle of the first segment, all the riders are thrown off the vibrating parts of the wire, while those which happen to be placed at the nodes, $\text{K}_2$, $\text{K}_3$, remain stationary.

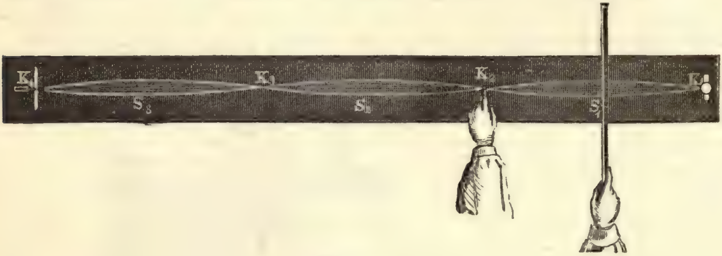

FIG. 251.

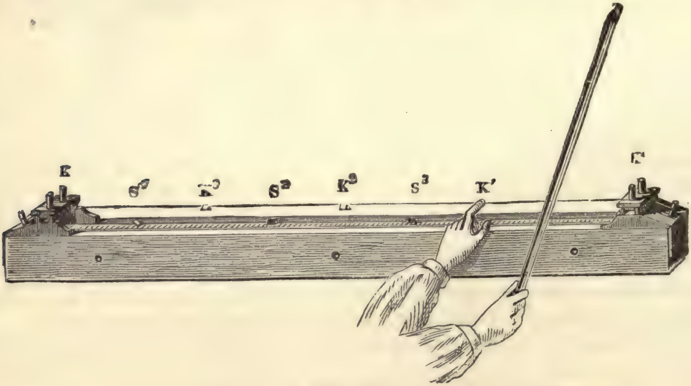

FIG. 252.

Among those who applied mathematical analysis to the transverse vibration of cords was Dr. BROOK TAYLOR (1685—1731). A little later the same problem was treated by the Bernouillis, Euler, Riccati, Lagrange, and others. From these investigations the following laws of vibrating cords ultimately emerged, and were confirmed by experiment:—

(1). *When different tensions are applied to the same length of string, the frequency of vibrations is directly as the square root of the tension.*

(2). *When different lengths of the same string at the same tension are taken, the frequency of vibrations is inversely as the length.*

(3). *When the tension and the length of string are unchanged, but the weight of the string is altered, then the frequency of the vibrations is inversely as the square root of the weight of the string.*

If we must look to Pythagoras as the ancient founder of acoustical science, we may justly regard E. F. F. CHLADNI (1756—1827) as the author of the modern aspects of the science. Chladni's father was Professor of Law at Wittenberg in Saxony, and destined his son to a legal career. The son's education was so strictly conducted that the period of youth was for him one of bitterness. The continual restraint imposed upon him developed by a natural reaction a stronger inclination towards occupation of his own choice. In deference to his father's wishes, however, Chladni studied at Wurtemberg and at Leipzig, and was admitted to the degree of Doctor in Law and in Philosophy. But on his father's death he abandoned the legal profession, and devoted himself entirely to the study of nature, which had always been his most pleasing, although hitherto but secondary, occupation. He had, at the age of nineteen, begun to learn the elements of the musical art, and he soon observed that the physical theory of sound was in a very backward state. He was seized with the desire of doing something to fill up this void in science. In 1785 he made a number of experiments, in the course of which he observes that plates of glass or metal gave different sounds, according to the manner in which they were held or struck. An account of some experiments on bells suggested to him the use of the violin-bow to throw his plates into vibration; and as the means of revealing the nature of the vibrations, he was induced, by reading of Lichtenberg's figures (in which powdered sulphur, etc., is strewn on an electrified plate), to try the plan of sprinkling his plates with a light powder. Chladni accordingly scattered a little fine sand over a circular plate, and when the plate was thrown into vibrations, he saw the sand arrange itself into well-defined radiating trains, showing the position of nodal lines (Fig. 253). This was a new phenomenon, and after some reflection on its cause, Chladni multiplied and varied the experiments, and discovered methods of producing an amazing variety of the sand figures, indicating of course an equal variety of forms of vibrating segments in the plates.

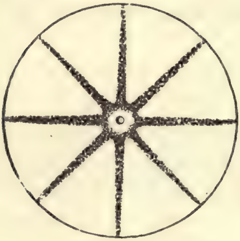

FIG. 253.

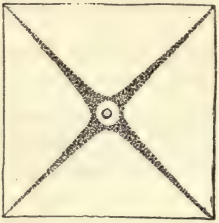

FIG. 254.

The plates used in these experiments were generally made either of glass or of copper. The plate was supported horizontally by being firmly fixed at one or more points by clamps; and the figures varied according as the bow was applied at one or another part of the edge, and according as segments were determined by touching the plate at certain points with the fingers. Thus, if a rectangular plate was fastened at the centre, and the bow was applied at the middle of one of the sides, the plate divided into four segments, the diagonals of the square being nodal lines (Fig. 254). Again, if a rectangular plate were handled in the manner shown in Fig. 255, the formation of additional semicircular nodal lines was determined, and the sand arranged itself into the symmetrical forms seen in the figure. Many figures more curious than this are producible by slight

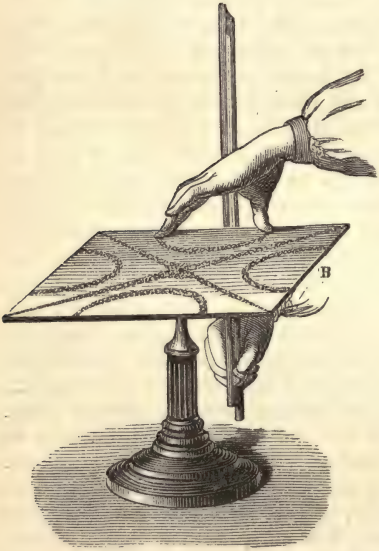

FIG. 255.

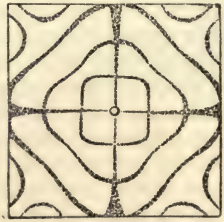

FIG. 256.

variations of the mode of treatment (*see* Fig. 256). Chladni, in 1787, published a memoir describing the vibrations of a round and a square plate. He afterwards figured and described several hundred forms obtained with plates of various shapes.

Chladni devised a very simple method of counting the number of vibrations corresponding with each note. He took a strip of iron or of brass about half an inch wide, one-twelfth of an inch thick, and of such a length that when one end of the strip was fixed in a vice, the vibrations were sufficiently slow to be counted by the eye, a task which is not difficult if they do not exceed eight per second. Taking such a length of the strip that its vibrations were, say, four per second, the number which would be counted if the length of the strip were made one-half, would be sixteen—that is, *four* times as many, and so on; the law, as established from mechanical considerations, being that the number of vibrations are *inversely* as the square of the length. It was thus perfectly easy to deduce the number of vibrations corresponding with any length of the strip. In practice, however, there were found to be circumstances attending this method of experimentally ascertaining the number of the vibrations which detracted much from its theoretical accuracy. It was difficult, for instance, to obtain a strip of metal of absolutely the same thickness and elasticity throughout its entire length, and the experimenter would never be certain that no play of the strip could occur between the jaws of the vice used for fixing it.

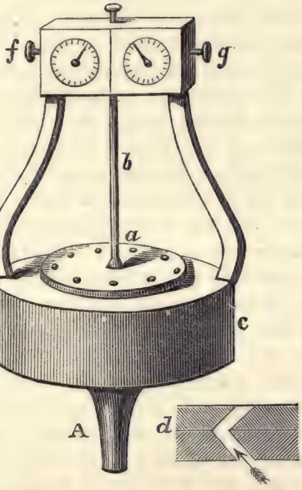

FIG. 257.

Some years afterwards an elegant instrument was invented by Cragniard de la Tour. The *Syren*, for so the instrument was called because it could sound with water as well as with air, is represented in Fig. 257. A shows the external appearance of the syren; *a* is a metallic disk perforated with a number of holes—say eight—at equal distances. This disc is attached to the spindle *b*, which turns with it with the least possible friction. The disc covers as closely as may be the fixed top of the drum C, consisting of a plate of metal perforated with holes coinciding with those of the disc. These holes are not perforations perpendicular to the surfaces of the plates; they pass obliquely through both, but the direction of the obliquity in the disc is opposed to that in the fixed plate. At *d* this is shown in section, and it will be seen that air escaping by the lower orifice as the upper one passes over it will drive the disc in the direction of the arrow. When, therefore, air is forced into the drum at *c*, the effect will be that when the rotation of the disc has once been set up, it will be accelerated by the impulse of the escaping air until it attains a constant velocity, determined by the pressure of the air in *c*. As the disc revolves, the passage of the air is alternately permitted and prevented, and in this way sounds are produced. It is only necessary to count the number of revolutions per second made by the disc while the instrument emits a given note, to ascertain the number of impulses corresponding with that note. The revolutions are registered on dials by causing the teeth of a wheel to engage an endless screw on the spindle. The buttons *f g* are for throwing the counting apparatus into and out of action at any required instant of time. The syren was improved by the physicist Dovē, who made the disc with several concentric circles of holes, increasing in number from the inside to the outside, with a separate air-chamber to each circle, to which the air was admitted at pleasure. By this arrangement the instrument could be made to sound several different notes simultaneously. Further improvements have since been introduced by Helmholtz.

About 1830 M. Felix Savart, a skilful French experimenter, set himself to investigate the limits of the sensibility of the ear. In his researches he adopted a method of producing musical notes which appears to have been first employed by that extraordinary genius Dr. Hooke (page 220), who in 1681 exhibited before the Royal Society a plan of producing musical notes by the striking of the teeth of wheels in very rapid rotation. Savart's apparatus allowed him to increase at will the sharpness of the shocks given to the air, and at the same time to determine with the greatest precision the number of strokes or vibrations per second. He used brass wheels of different sizes, from 9 to 30 inches in diameter, divided each into several hundred teeth. A piece of card, or a thin strip of wood, was so fixed that each tooth should strike it in passing. It was easy, by a simple mechanism, to cause the number of rotations to be registered on a dial-plate. On comparing this apparatus with a monochord (page 14), he found that when each sounded the same note, the number of blows given to the card was equal to the number of complete, double, or to-and-fro vibrations of the cord. A comparison of the toothed wheels with Cragniard de Latour's instrument showed that exactly the same sounds were obtained by the two methods of communicating impulses to the air. The toothed wheels could indeed be employed after the manner of the revolving discs in the syren, by directing a current of air against the teeth of the wheel, perpendicularly to its plane, from the orifice of a small tube; so that as each tooth passed the current of air was interrupted. The notes thus yielded by the wheels were identical with those produced by the shocks given to the piece of wood or cardboard. Savart endeavoured to ascertain the limiting number of vibrations per second at which the ear ceases to have any perception of these vibrations as sounds. With a wheel 9 inches diameter, however, the tones lost their clearness when 4,000 impulses per second were given. But when a wheel 18 inches in diameter, with the same number of teeth, was used, sounds up to 15,000 vibrations per second were audible. With a still larger wheel the number of vibrations producing an audible sound was carried to 24,000 per second. It would appear, therefore, from these experiments that, had the diameter of the wheel been further increased, while the number of teeth remained the same, a greater number of shocks than 24,000 per second might have been made audible. This shows that an increase in the intensity of the impulses extends the limits of audibility. In fact, Despretz afterwards proved that sounds may be heard corresponding with the rate of 38,000 vibrations per second. The whole range of musical tones distinguished by the human ear extends, therefore, over nearly seven octaves.

The most interesting inquiries belonging to the science of acoustics are undoubtedly those relating to musical tones. We have seen that the ancient Greeks were accurately acquainted with the relative lengths of vibrating strings which yield harmonic intervals. But these ratios were by them, and by others down to comparatively recent times, considered as significant of some recondite relation between whole numbers and musical sounds. When the investigations of the eighteenth century brought to light the laws of vibrations in strings, it was of course perceived that the simple ratios belonged essentially to the *vibration rates*, and that they applied to the tones of all musical instruments. Very curious, however, were the explanations given of the pleasure derived by the ear from concordant tones. Even Euler entertained a notion that the listener was thus pleasurably affected because the mind easily comprehends simple ratios, whereas, as a matter of fact, no numbers or ratios are presented to the consciousness of the hearer, but are associated with musical harmonies in the minds of the scientific inquirer only. It has been reserved for a distinguished investigator of our own time to discover the physical causes of the consonances, discords, and qualities of tones. It has been by bringing to the task great mathematical acquirements, rare experimental skill, a profound knowledge of physiology, and something of the inspiration of genius, that Helmholtz has succeeded in

> Untwisting all the chains that tie
> The hidden soul of harmony.

HERMANN LOUIS HELMHOLTZ was born in 1821, and was educated for the medical profession. He has been Professor of Physiology at Königsberg, Heidelberg, and Berlin successively. As an aid to the study of physiology, Helmholtz turned his attention to physics, and as necessary to the mastery of physics he devoted some time to mathematics. In each of these three great departments of science he has distinguished himself by his original researches. The works by which he is well known throughout Europe are "The Conservation of Force," 1847; "A Manual of Physiological Optics," 1856; "The Sensations of Tone as a basis for the Physiological Theory of Music," 1862.

The reader doubtless understands that just as a disturbance at any point on the surface of a still piece of water raises a wave which expands in an ever-widening *circle*, so a sound-wave spreads from its point of origin through the air in all directions as an enlarging *sphere*. But there is this essential difference: the free surface of the water rises into ridges and sinks into hollows, whereas there is no unoccupied place into which the disturbed particles of the air can move. They are therefore impelled towards the contiguous particles, and there the air is condensed until its elasticity reacts in repelling the particles and producing a corresponding rarefaction. Thus the ridges and troughs of water-waves would correspond respectively to spherical shells of condensation and rarefaction in air-waves. That different systems of waves may simultaneously and independently traverse a piece of water is a matter of every-day observation, which may help us to understand the like independence of innumerable systems of sound-waves moving at the same time through the same mass of air. It is the frequency of the vibrations which determines the *pitch* of any tone, and their amplitude, *i.e.*, the *amount* of compression and rarefaction which determines its loudness. There is a third property of musical tones, the cause of which was a mystery until Helmholtz's masterly investigations cleared it up. When the same note is sung with the same loudness on different vowel sounds, a very different tone is heard; or, again, when the same vowel is sung in the same pitch, the voice of the singer is distinguishable by the *quality* of the tone. Again, the difference of character in the sound given out by the several musical instruments is readily distinguished by the most careless listener, so that if an air be performed on an unseen instrument, he recognizes the notes of a flute, clarinet, accordion, or trumpet, as the case may be.

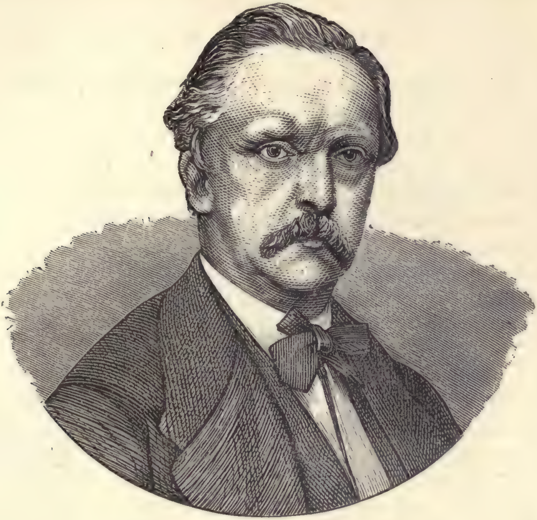

FIG. 258.—PROFESSOR HELMHOLTZ.

Now, Helmholtz has demonstrated by various ingenious experimental contrivances that whenever we hear a musical note sounding on any instrument, there are present not only the principal set of vibrations which gives the pitch of the note, but other sets determined by properties of the sounding body. Thus, for example, a stretched string will not vibrate simply as a whole, but will also divide itself into segments, each of which vibrates independently. The result is that accompanying the note due to the vibration of the string as a whole, there are sounds corresponding with a string of half the length, of one-third, one-fourth, one-fifth, etc., of that length. The vibration rates of these secondary tones, which are called by Helmholtz *partial tones*, would therefore be twice, three times, four times, etc., as great as that of the fundamental tone. If we represent the vibration rate of the latter by the number 24, the figures representing the vibration rates of the several partial tones would be as follows :—

| | |
|---|---|
| 1st partial (the prime or fundamental tone) | 24 |
| 2nd upper partial tone | 48 |
| 3rd ,, ,, | 72 |
| 4th ,, ,, | 96 |
| 5th ,, ,, | 120 |
| 6th ,, ,, | 144 |
| 7th ,, ,, | 168 |
| etc. | etc. |

Now, the vibration ratios of an octave of the natural scale are

24 : 27 : 30 : 32 : 36 : 40 : 45 : 48,

and by doubling these numbers we obtain the figures representing the vibrations for the next higher octave, and so on. It will be seen that the *partial tones* of the string up to the 6th, form harmonious combinations with the 1st; for they are octaves, or octaves of the third and of the fifth of the fundamental tone. The 7th partial does not belong to the scale, and forms a discord. Now, the makers of pianofortes, guided only by practice, in order to obtain a fine tone, have long made the hammers of their instruments to strike the strings at a particular part of their length, where the vibrations that would give the 7th partial are annulled. This circumstance may serve to illustrate the scientific importance of such discoveries as those of Helmholtz. We may say of it what Tyndall ("Sound," Lect. vii.) says, speaking more generally of the progress of music: "The musicians engaged in this work knew nothing of the physical facts and principles involved in their efforts; they knew no more about them than the inventors of gunpowder knew about the law of atomic proportions. They tried and tried until they obtained a satisfactory result; and now, when the scientific mind is brought to bear upon the subject, order is seen rising through the confusion, and the results of pure empiricism are found to be in harmony with natural law."

When it is understood that every sounding body has its own peculiar system of partial tones, which may vary according to certain laws with the mode in which the vibrations are excited, the cause of the variety in the *qualities* of the composite resulting tones no longer remains a mystery. The case of strings is mentioned here as a simple instance. With other sounding bodies more complicated laws appear. Thus, for example, the 1st *upper* partial tone of a tuning-fork has a vibration rate 6¼ that of the fundamental tone. Helmholtz has shown also that the various vowel sounds depend upon combinations of different *partial tones*, and that for each vowel there is a particular part of the scale where the characteristics of the sound are best brought out. The space at our command will not allow us to do more than mention Helmholtz's examination of *differential tones*, which were first discovered by a German organist named Sorge in 1740, and afterwards brought into notice by Tartini. These are the sounds sometimes called *grave harmonics*, which are heard when two musical tones of different pitch are sounded loudly and continuously, and they correspond with the difference of the vibration rates of the two notes. A third kind of tones were discovered by Helmholtz himself, namely, those with a vibration rate equal to the sum of the rates of the generating tones, and were called by him *summational* tones.

There are two quite recent inventions which in the most remarkable manner confirm the results of Helmholtz, and in the highest degree combine popular and scientific interest. These are the *Speaking Telephone* and the *Phonograph.* Before explaining the former it will be desirable to describe an earlier form of telephone which reproduced

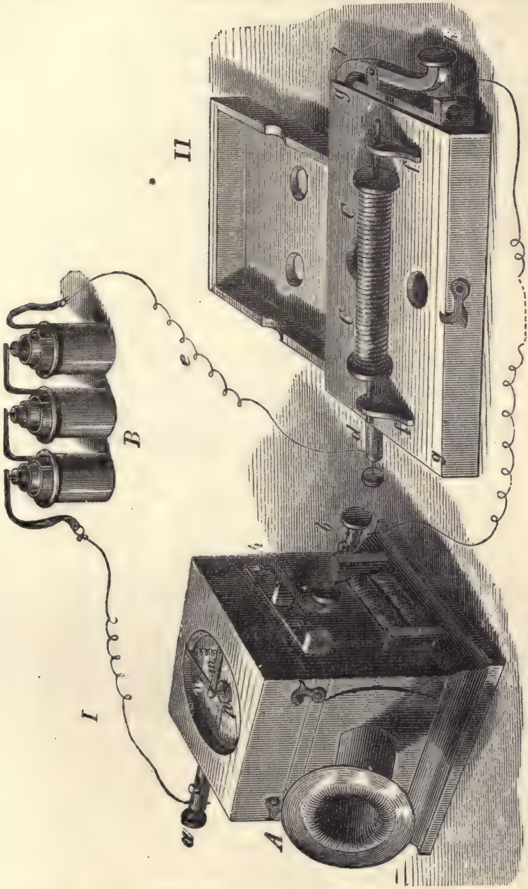

FIG. 259.

musical notes at a distance. It was founded on a discovery made in 1837 by an American physicist named Page. This gentleman found that at the instant when a bar of soft iron is magnetized, by passing a current through a coil of wire surrounding it (see Chap. XX.), a sharp click is emitted. *C C* in Fig. 259 represents such a coil traversed by a slender rod of iron, which is mounted by the supports *f f* on the sounding-board *g g*. *A* is a hollow box with a wide trumpet-mouthed opening in one side, and at the top is a circular opening, over which is stretched a membrane *u*. A very small disc of platinum *m* attached to the centre of the membrane is caused by its vibrations to complete the electrical contact by which the current from the battery *B* flows through the circuit including the coil *C*. When a person sings into the opening at *A* the membrane is thrown into vibration, and at each vibration a click is emitted by the sounding-box at *I I*. Such was the apparatus contrived in 1860 by M. Reiss of Friedrichsdorf, who thus successfully solved the problem of causing electricity to reproduce sounds at a distance. Other forms of musical telephones were subsequently proposed; but in 1876 Mr. GRAHAM BELL, who had for some years devoted himself to the study, had produced the speaking telephone, which Sir W. Thompson has not hesitated to call the wonder of wonders. By this instrument persons separated by hundreds of miles may converse, and even recognize the tones of each other's voice. Here no battery is required, for the vibration of a thin iron plate is made to generate the currents. Fig. 260 is the apparatus in section. H S is a cylindrical steel magnet, about 3 inches long. One end is enclosed by a small coil, B, of fine silk-covered copper wire, the extremities of which pass through the wooden case M at *f f*, and are connected at I with the line wire C C. By the influence of the magnet the iron diaphragm L L is made magnetic, and when it is set vibrating by speaking into the opening R R′, its approach to and recession from S act according to principles which will be mentioned in the following chapter, to induce momentary currents in the coil and through the circuit. These currents are received by an exactly similar instrument, and by the inverse actions the iron diaphragm of the latter is thrown into vibrations which reproduce those of the transmitter.

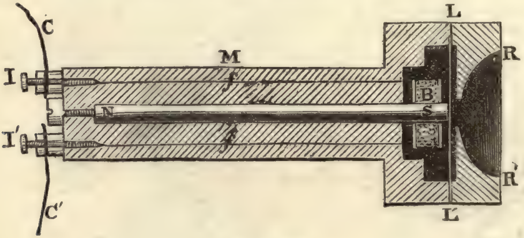

FIG. 260.

The other invention, the phonograph of Mr. EDISON, dates from 1877. In the phonograph a membrane thrown into vibration by speech or other sound slightly presses a blunt steel point against a sheet of tinfoil drawn along at a uniform rate. If the membrane were quiescent, the point would mark a very shallow trace on the tinfoil, which is supported on a cylindrical or other grooved surface so that the steel point is always opposite the grooves. When the sounds have been registered on the sheet of tinfoil, they are reproduced by the instrument when the indented sheet is again drawn under the steel point. Phrases in three different languages have been simultaneously spoken into the instrument, and the superimposed sounds have been so reproduced that the words of each language were distinctly recognized. We see, therefore, that the vibrating discs of the telephone and the phonograph can combine different systems of impulses, however numerous and complex, into definite movements, in which all the systems are simultaneously represented.

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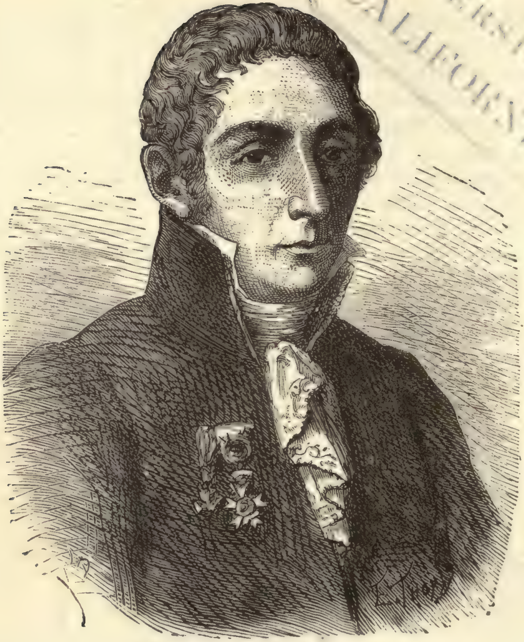

FIG. 261.—VOLTA.

# CHAPTER XX.

## PHYSICS OF THE NINETEENTH CENTURY—ELECTRICITY.

IF the facts relating to electricity which had been discovered by the middle of the eighteenth century be remembered, it will be seen that nothing was more natural than that men's minds should be occupied in the following period by the idea that there existed some mysterious connection between electricity and the principle of life. The known effects of the Leyden jar made the nature of the action of electricity on the animal economy a subject of study for the physiologist. At the end of the eighteenth century the chair of anatomy at the University of Bologna was occupied by a certain ALOYSIUS GALVANI (1737—1798), who was not only an adept in his professed science, but a skilful experimentalist in chemistry and physics. He was occupied at the period of which we are speaking in experiments on the effects of electricity on the organs of animals. In 1780 Galvani was one day demonstrating to some of his pupils certain points in physiology, in which recently killed frogs were the subjects operated upon. One of them had been prepared for the purpose we have indicated, when Galvani, having for the time to leave his laboratory, laid down the dead frog near an electrical machine which some of his pupils were using at the time for electrical experiments. During Galvani's absence one of his assistants, in completing the anatomical preparations, happened to touch the nerves of the frog's hind leg with his knife at the same moment that one of the electrical experimenters was drawing a spark from the machine. The legs of the dead frog made then a sudden convulsive movement, to the great surprise of all present. Galvani was immediately informed of the circumstance, and he hastened with eagerness to repeat the experiment. The necessary conditions were soon recognized: when one of the party touched one or other of the crural nerves of the frog with the point of the knife, the corresponding leg was thrown into violent action every time a spark was drawn from the conductor of the neighbouring electrical machine. The frog or the person touching it had, it should be understood, no direct connection with the electrical machine, the result being, in fact, due to inductive action, or that which is known as the "return shock." This incident is represented in our engraving. Galvani, having discovered a new phenomenon, immediately proceeded to investigate its causes and relationship by the well-known experimental device of varying the circumstances. The movement of the frog's legs occurred the first time the phenomenon was observed at the touch of the dissector's knife. Galvani tried the effect of touching the nerves with substances of different kinds, and he soon found that any substances would produce the effect, provided they were good conductors of electricity, and that the contact was made in the neighbourhood of the conductor of the electrical machine, and while a spark was drawn from the conductor. The next step was to find whether electricity from different sources produced the same effect. He tried the negative electricity of the machine, the electricity of the Leyden jar, and that of the electrophorus. The same effect was observed in all these cases. There remained only the atmospheric electricity. Galvani instituted experiments on the effect of this also, by attaching the prepared legs of a frog to the lower part of a wire which was connected with a metallic rod elevated in the air. Violent contractions of the frog's legs were observed whenever flashes of lightning appeared. Galvani had now recognized the fact of the frog's legs being one of the most delicate of electroscopes. And it was in studying the atmospheric electricity by this new kind of electroscope, that he made the capital discovery which appeared to confirm all his previous suspicions of the action of electricity in vital phenomena. He had prepared a number of these animal electroscopes, and by means of brass hooks passed through the spinal cord, had them hung upon some iron railings. He found that they exhibited contractions not only when flashes of lightning occurred but at other times when the sky was clear. He attributed these effects to changes of the electric condition of the atmosphere. One one occasion, many hours having elapsed without any movements of his electroscopes having been observed, Galvani, wearied of having often looked in vain to discover signs of electricity, pressed the brass hooks forcibly against the iron rails, in order to see whether this would operate to make the frog's legs more sensitive to the changes of the atmospheric electricity, on account of the more perfect contact and electrical connection. During this action he observed contractions in abundance, which had no relation whatever to the electric state of the atmosphere. Here, let it be observed, Galvani for the first time saw the muscular contraction take place, without the presence of any electrical apparatus, or of any electrical disturbance of the atmosphere. It flashed instantly upon his mind that the electricity must exist in the animal organs themselves, and that the metals acted merely as *conductors* of the organic electricity. A clue appeared to have at length

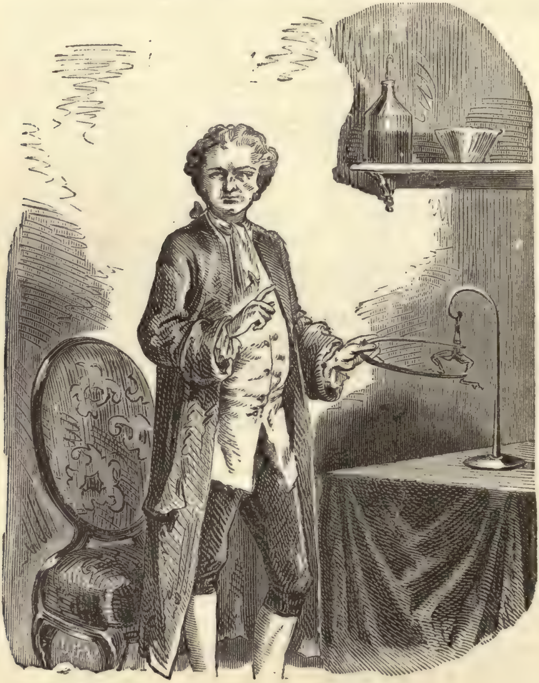

FIG. 262.—GALVANI'S EXPERIMENT.

been obtained to the mystery of animal movement, and Galvani proceeded to follow it up in the true spirit of experimental investigation. In order to eliminate any effect which might be due to the magnetic condition of the iron railings or to their condition, he reproduced the arrangement within doors with bars of polished iron, and with these in every position. He observed the same contractions as before. He had now entered upon a new and untrodden path of discovery, which he began to explore with eagerness, and yet with philosophic caution; for as he himself said (repeating a very old observation), we are easily deceived in an experiment, and what we have wished to see we think we have seen. He examined the consequence of constructing with different materials the circuit connecting the nerves and the muscles of the frog's legs. He found that it was necessary to form the circuit entirely of substances capable of conducting electricity. The most powerful contractions were obtained when the lumbar nerves were covered with tinfoil, the muscles of the leg with silver leaf, and a copper wire used to establish a connection between the two metals. These experiments appeared to establish beyond doubt the idea with which Galvani commenced his experiments, namely, that a certain distribution of electricity naturally existed in the animal organism. These experimental results he formulated in a theory in which the muscle was regarded as a kind of Leyden jar, the nerve and the metallic connections in the experiment acting as conductors through which the discharge took place. In this discharge positive electricity passed, he declared, from the inside of the muscle to the nerve, and from the nerve through the metallic connector to the outside of the muscle. We shall see presently how Galvani's idea of the existence of electric currents in the muscles themselves was overshadowed for a time by other theories. Nevertheless years afterwards, when the progress of science had furnished means of investigation not dreamt of in 1791, Nobili confirmed (1827) the correctness of Galvani's views as to the muscular currents; and at a still later period Matteuci, and especially Professor Dubois-Reymond of Berlin, have made the muscular currents the subject of most elaborate and thorough experimental investigations. One distinct result of these modern investigations has been the establishment of the law that in the muscles of *all animals* any outer point is electrically *positive* with regard to any inner point in the same transverse section of the muscle. This law was clearly established by Dubois-Reymond in 1843, and the same distinguished observer has since greatly extended our knowledge of this subject. For instance, he has shown that the more powerful the mechanical effect which a muscle is destined to produce, the more intense is the electrical current in a conductor connecting one part of the muscle with another. Portions of the muscles of a dead animal properly arranged constitute, in fact, a voltaic pile capable of yielding currents which will deflect a magnetic needle. Galvani's fundamental experiment, as now repeated in every course of practical physiology, is shown on a larger scale in Fig. 263, where a copper and and a zinc wire, marked respectively c and z, are seen twisted together so as to insure contact. The copper wire touches the crural nerves where these nerves issue from the spine. At the instant when the end of the zinc wire touches the legs the muscles exhibit a violent contraction.

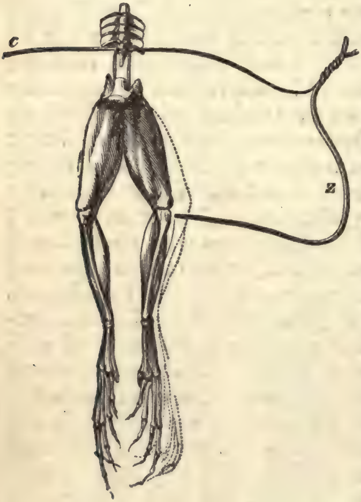

FIG. 263.

Galvani carried on his experiments during a period of eleven years before he published (1791) his results and his views thereupon. The sensation produced by the publication of his work will be adequately understood only by those who are acquainted with the ideas which were occupying men's minds at the time. In many quarters Galvani's views were enthusiastically adopted, but dissentients soon appeared who called in question the physiological origin of the electricity. Singularly enough, Galvani himself had at first been inclined to attribute the origin of the electricity to the metals, but he had abandoned this theory as inconsistent with the facts. A compatriot of Galvani's, named Alexander Volta, following up the observations made by Galvani himself as to the greater energy of the contractions when the arc is made of two different metals, opposed Galvani's theory by another in which the contact of metals of different kinds was advanced as the origin of the electricity. According to Volta, the facts proved that contact of *any* substances of different kinds was a source of electricity; and the contraction of the frog's legs, which took place when only one metal was employed, was due to the contact of the metal with the humour of the muscles. A memorable dispute arose. Galvani showed that the intervention of a metal was not necessary to excite the contractions —it sufficed to cause the severed nerves to touch the exterior of the muscle. He even brought forward an arrangement of the experiment in which there was no *heterogeneous* contact whatever. Notwithstanding this apparently decisive instance, the contest was carried on for some years, dividing the scientific world into rival camps. At length, in 1799, Volta, in pursuit of facts with which to support his own theory of Galvani's experiment, was led to the construction of a piece of apparatus which proved to be the point of departure for a new exploration of the arcana of nature. No one invention was ever more fertile

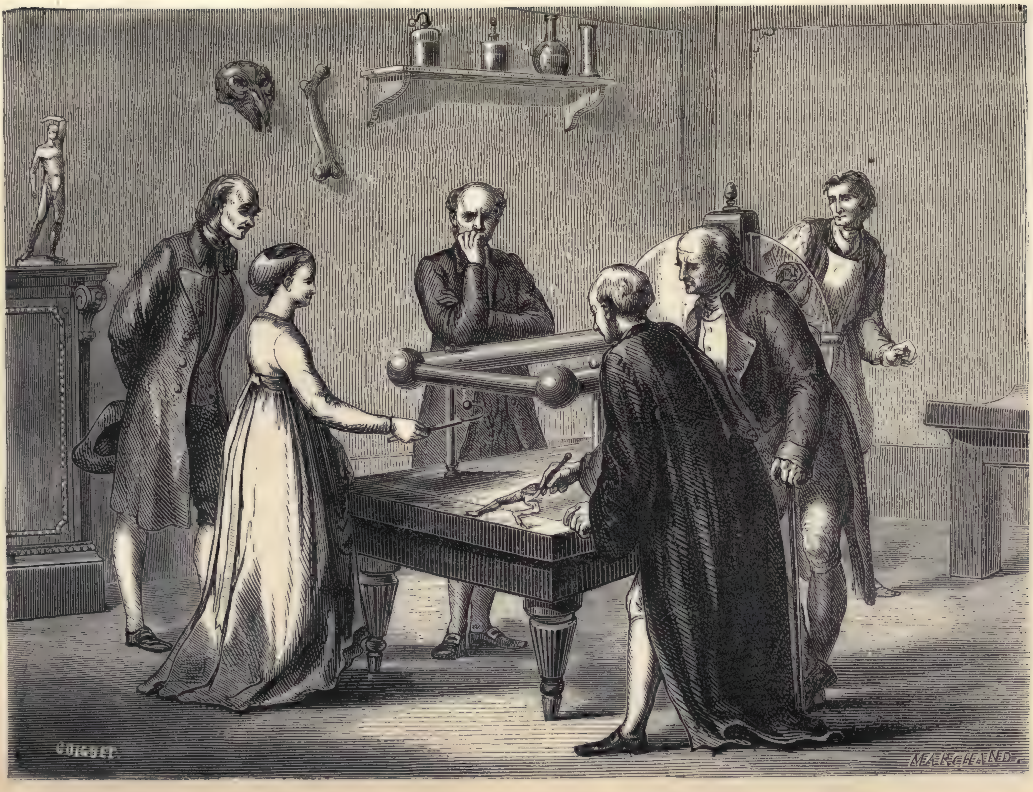

PLATE XIII.—GALVANI'S DISCOVERY.

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in the number, variety, and importance of its ultimate scientific consequences than this one of Volta's. VOLTA (1745—1826) had been the Professor of Natural Philosophy in the University of Pavia, and had retired from the duties of that position when, at Como, he made the great discovery which was first announced to the scientific world in a letter to the Royal Society of London. This letter, dated the 20th of March, 1800, was read before the Society on the 26th June, 1800. He says that he has made an apparatus which, in some of its effects, is like a Leyden jar feebly charged, but constantly recovers its charge, and thus forms a perpetual source of electricity, and is, moreover, different from other electrical apparatus in being made entirely of conducting substances. It is formed of a number of discs of copper, zinc, and paper or leather steeped in salt water. He compares it to the electric organs of the torpedo and similar animals. The discs may be about an inch in diameter, but their shape and size are immaterial. As silver gives better results than copper, he sometimes uses silver coins, and thus constructs his apparatus:—he places a silver coin on a horizontal support, and above the coin a disc of zinc. Upon the zinc he lays a disc of moistened leather or paper, above that another silver coin, then again a disc of zinc, followed by one of paper, and so on until a column has been formed containing several dozens of each kind of discs, always arranged in the same order. Volta explains how the apparatus gave signs of electricity and a spark might be obtained, and at the moment when the fingers were made to touch simultaneously the lowest and highest of the metallic discs, a slight electrical shock was felt.

Such was the apparatus which became famous under the name of the *Voltaic Pile* (*see* Fig. 264).

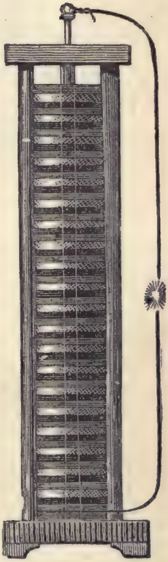
FIG. 264.—VOLTAIC PILE.

Volta himself seems to have had no idea of the many capabilities of his apparatus. He is content with finding only those we have already mentioned, and he passes on to show that his apparatus is an "*experimentum crucis*" in favour of the contact theory. The action by which the electric fluid is put into operation does not, he contends, take place where the moist substance touches the metal, though perhaps some insignificant effect might even there be also produced; but its chief seat is at the contact of the two *dissimilar* metals, *e.g.*, of the silver and the zinc. The layers of moisture are, he says, only the medium of connecting the different metallic couples by which the electric fluid is driven in one direction throughout. He found, indeed, that when the discs were moistened with a saline or acid solution, the effects were far more powerful than when only water was used; but he attributes this to the better conducting power of such solutions. Seeking to remedy the rapid diminution of the power of the pile, which he attributed to the drying up of the moisture in his porous discs, Volta was led to the happy modification of the apparatus that received the name of the "Crown of Cups" (*couronne de tasses*). This is represented in Fig. 265, where the saline solution is seen to be contained in distinct vessels, into each of which is plunged a plate of zinc, Z, and a plate of copper, K, and the copper in one vessel is connected by a wire with the zinc in the next vessel, and so on throughout. The meaning of the arrows placed over the wires will be explained in the sequel.

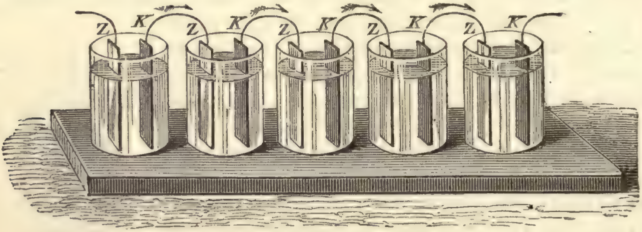

FIG. 265.—THE "CROWN OF CUPS."

Volta failed to study, or at least he passes over in silence, the *chemical* changes, which are sufficiently obvious in one of the arrangements just described. The corrosion and solution of the zinc by the acid or saline solution were probably regarded by himself as accidental effects in no way related to the development of the electricity, and the prepossession of his mind by the contact theory would indispose him to look any further into the matter. Now, the discovery announced in the first instalment of Volta's letter having been privately communicated by Sir Joseph Banks, the President of the Royal Society, to several of the members before the complete communication was received, experiments were instituted in London to confirm and elucidate Volta's results. Thus it happened that the chemical powers of the pile were discovered by Carlisle and Nicholson on the 2nd of May, 1800. They were endeavouring to find the nature, *positive* or *negative*, of the electricity at the ends of the voltaic pile they had constructed with half-crowns and discs of zinc, and in placing the end of a wire in a drop of water to obtain better contact, they noticed that the wire became covered with minute bubbles of gas. This led to further investigations, and the two experimenters were soon able to announce that they had succeeded in decomposing water by the agency of Volta's pile. The decomposition of water by electricity, an experiment which soon became classic, is described in another chapter. By July in the same year, 1800, William Cruikshank had discovered the power of the pile to decompose salts, and to cause in certain cases their constituent metals to be deposited in the solid form. Experiments in voltaic chemical decompositions were now everywhere repeated, and as will be found stated in relation to the history of chemistry, an important theory and new classification of substances, based on their electrolytic properties, were proposed by Davy and Berzelius.

Davy at once entered eagerly into an experimental investigation of the powers of the pile. We find him in October, 1800, propounding very distinctly the *chemical theory* of the pile. He declares that galvanism is a purely chemical action, depending entirely on the oxidation taking place at the metallic surfaces, upon which the solution acts in different degrees. If the plates are moistened with *perfectly pure* water, the pile will not act, because pure water does not act upon zinc, and, in fact, the electrical power of the pile is always proportionate to the energy with which the oxidizable metal is acted upon.

The original forms of the voltaic pile and the *couronne de tasses* were united into one in the arrangement invented by Cruikshank. We may think of this as a horizontal instead of a vertical pile, with moistened discs replaced by liquid strata, or as the *couronne de tasses*, with the metallic connections between the plates so abbreviated that the copper of one cell and the zinc of the next are soldered to each other back to back, and themselves constitute the walls of the cells which contain the liquid. "Cruikshank's battery" is the simplest of all voltaic arrangement. The double plates of copper and zinc are of a rectangular form, and are cemented into grooves in the bottom and sides of a long rectangular trough, which they divide into so many cells. The exciting liquid is poured into these cells. The simplicity of this arrangement at once permitted voltaic piles, or batteries, as we more frequently call them, to be conveniently made of a far greater number of elements than had before been used. In February, 1802, Pepys had made a battery of sixty pairs of plates 6 inches square. Experiments with this revealed new and wonderful powers of galvanism. Iron wires were burned, platinum wires made to glow with an intense light, charcoal was rendered incandescent, gold and silver leaf were deflagrated.

Other improvements in the arrangement of the plates of copper and zinc in the voltaic battery were effected by Wollaston, and soon after Davy had entered upon the professorship of chemistry at the Royal Institution, the managers had the large Wollaston battery constructed, by which Davy made his famous discoveries of the metals of the alkalies. This battery contained 250 pairs of plates; but after Davy had announced his great discovery, another battery of 600 pairs of plates was constructed for the Royal Institution, the cost being defrayed by a subscription of the members. It was with this that Davy succeeded in completely demonstrating the existence of the metallic bases in the alkaline earths. He obtained an amalgam of the metal by forming the negative pole of the battery of a drop of mercury resting on the oxide to be decomposed, which was supported on a plate of platinum (*See* Fig. 266).

Here it will be convenient to mention the principal modifications of Volta's pile, which have been in use since Wollaston's, whose improvement of Cruikshank's battery consisted in folding the plates of copper so that each of them had a plate of zinc within it—without contact, of course. Thus a copper surface was presented to the *two* sides of the zinc plate instead of *one*, as in Cruikshank's arrangement. A plan by which large surfaces of metal are exposed to the action of the exciting liquid was devised by Dr. Hare, of Philadelphia. A long sheet of copper is separated from a similar sheet of zinc by narrow strips of cloth, and the two are wound round a wooden cylinder. This compound roll plunged into a vessel of dilute acid forms an extremely powerful single couple, and by combinations of a few such couples extraordinary effects have been produced.

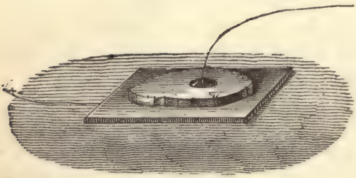

Fig. 266.

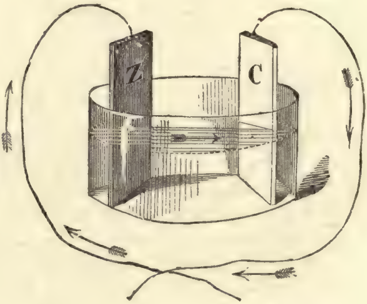

Fig. 267.—A Voltaic Element.

It is desirable, in order that the reader may understand certain terms which must now be frequently employed, and that he may have a clear idea of the theories of the pile which have been proposed, that he should know the facts which are presented by a voltaic element. Let Fig. 267 represent a vessel which contains diluted sulphuric acid. z is a plate of *pure* zinc, c a plate of copper, and to each a wire is attached. We suppose, in the first instance, that these wires do not touch, but are insulated. If the wire attached to c were examined by appropriate and delicate tests, it would be found to exhibit feeble indications of a charge of vitreous or *positive* electricity. Similarly the wire attached to z would exhibit indication of a very feeble *negative* (page 328) charge. So long as the wires are not in contact with each other no action occurs in the vessel—the zinc does not dissolve in the acid. The instant, however, that a metallic communication is established between the two plates the zinc begins to dissolve, but no visible action is perceived on its surface; whereas the copper plate, on the contrary, which is not acted upon, is immediately covered with minute bubbles of hydrogen gas, and these rise up through the liquid. While this action is proceeding, the wire which connects the metals, and which may be of any length, is possessed of very remarkable properties, and it will be the business of several of our subsequent pages to relate the discovery of these properties. Further, although the liquid intervening between the plates remains unchanged to the eye, it also possesses certain properties in common with the wire. For example, there is a particular effect on a compass-needle (of which more hereafter); but the thing now to be noted is that the liquid, or *any* part of the wire, equally possesses this power, and that it is immediately lost if the smallest break occurs anywhere—that is, there must be a perfect continuity of metallic and liquid connection throughout. It will hereafter be seen, also, that the action is such that we must recognize the influence of the wire or circuit as having a *definite direction.* These circumstances at once suggest to the mind the idea of something flowing through the whole, and thus the phenomena of voltaic element appear in harmony with the previous conception of the nature of electricity. It must, however, be once more repeated that the "electrical fluid" has no objective existence. It is only to facilitate our records and reasonings that we picture the unknown influence in our minds under the image of something flowing in one direction like water through a pipe. It is *assumed*, by convention, that the *current flows* always from the positively electrified body to the negatively electrified. This is the direction shown by the arrows in Figs. 265 and 267. It will be observed that there is, and must be, a *complete circuit through which the same current of electricity may flow equally at the same time across every section.* At what part of the circuit does the force originate by which we imagine the current to be driven on? This is a point which has been the subject of great controversies and of many researches. Two contending hypotheses, each of which has in turn prevailed, are already before the reader, and the views entertained by several eminent physicists of the present day are something like a fusion of the contact theory of Volta (page 553) and the chemical theory advanced by Davy (page 555). Supposing the wires represented in Fig. 267 to be of copper, the origin of the electrical force is, according to the contact theory, chiefly at the junction of the copper wire with the zinc plate, because there are at that point two different metals in contact. The heterogeneity of the liquid and metal contacts would also be admitted as contributing to the result, and the chemical action would be considered as the effect of the current. The chemical theory, on the other hand, places the origin of the current at the junction of the liquid and the zinc, and regards the current itself as the necessary result of the chemical actions.

To follow the discussions and relate the experiments which the rival theories of the pile have elicited would occupy no little space. A fundamental experiment in favour of Volta's theory is represented in Fig. 268, where E is a condensing electroscope, and C Z a compound bar, one moiety of copper, the other zinc. Holding the zinc in his hand, the operator touches the copper plate of the condenser with the copper end of the bar. The electroscope thereupon indicates that the copper possesses a feeble charge of positive electricity. The objection made by the supporters of the chemical theory to this experiment was that the result was due to the action of the natural moisture of the hand on the zinc. Other experiments were devised in refutation or support of each theory: the most distinguished scientific men of the time have contributed something to the discussion, as Wollaston, Priestley, Biot, Davy, Gay-Lussac, Berzelius, De la Rive, Faraday, Sir William Thomson, etc. How much may be said for either theory may perhaps be inferred from the circumstance that, in twenty years (1820—1840), the number of scientific papers which related to methods of reconciling or blending the two rival theories amounted to more than two thousand. A great number of ingenious and telling experiments were brought forward by Faraday, and his arguments appeared so convincing that for a time the chemical theory was triumphant.

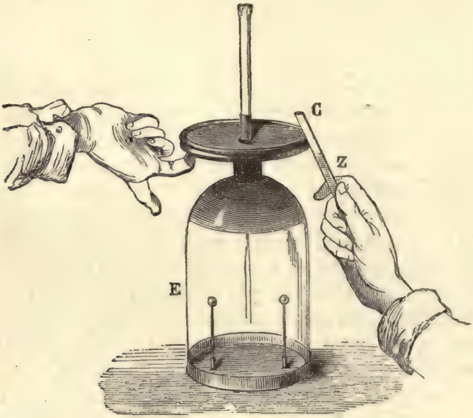

FIG. 268.

All the voltaic arrangements we have described have one inconvenience; their action, however powerful at first, rapidly diminishes. The cause of this defect has been traced to the layer of hydrogen gas which appears on the copper or other metal of the couple *not acted* upon by the acid. In 1836 Professor Daniell contrived an ingenious method of avoiding this inconvenience. In his battery the zinc is in contact with one liquid, and the copper with another, the two liquids being separated by a porous partition. Fig. 269 shows a cell in section. A A is a cylindrical copper vessel, within which is a cylinder of *porous* earthenware C C; within which, again, is a rod of zinc, externally covered with a little mercury, which dissolves only the pure zinc, and presents it to the action of the dilute sulphuric acid contained in the porous cell. Outside of the porous cell is a saturated solution of sulphate of copper. In the action of the cell no gas appears, but metallic copper is deposited on the inside of the copper vessel. The theory was that the hydrogen, instead of appearing on the copper plate as a gas, replaces the dissolved copper in the solution. Several other arrangements of "constant batteries" have been devised since, and some of these are very energetic, or have other special advantages. It will suffice to mention Bunsen's battery, the arrangement of which is exhibited in Fig. 270, where the complete cell and its parts are severally shown. A is a glass or porcelain vessel containing dilute sulphuric acid. B is a thick plate of zinc bent round and amalgamated. C is a vessel

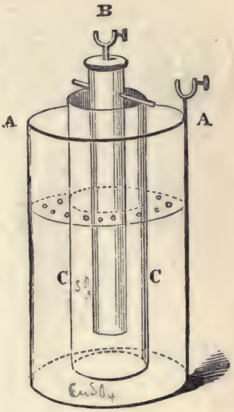

FIG. 269.

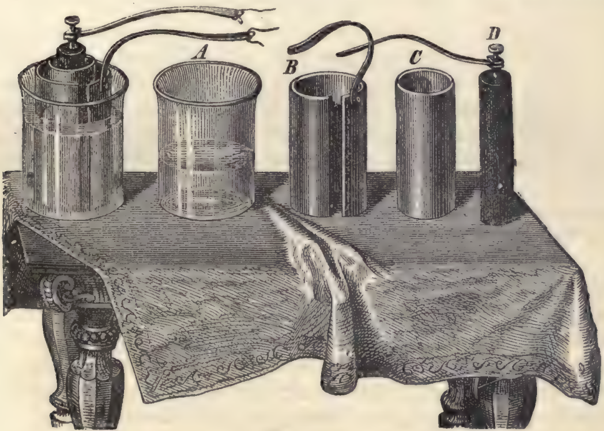

FIG. 270.

of porous earthenware. D is a solid cylinder of hard gas-retort carbon, which is placed within the porous cell and surrounded by strong nitric acid. The hydrogen, which in the action of the battery would otherwise cover the carbon, is oxidized at the expense of the nitric acid. Fig. 271 shows in plan the mode in which four such Bunsen's cells may be connected with each other.

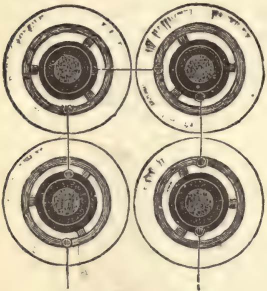

FIG. 271.

Before the year 1820 many experimental and speculative attempts had been made to identify the causes of magnetic and electric phenomena. The analogies between magnetic and electric attractions and repulsions are, indeed, too obvious not to have suggested the idea of the "fluids" being either the same in the two classes of phenomena, or at least of their being closely related to each other. Among others who discussed such questions at the beginning of the present century was CHRISTIAN ŒRSTED (1777—1851), Professor of Natural Philosophy at the University of Copenhagen. In a work published in 1812 Œrsted adduces proofs of the probable identity of electricity and magnetism. He also occupied himself in attempts to demonstrate this identity by actual experiments. But the attractions and repulsions of the extremities of Volta's piles were too feeble to admit of experimental demonstration in the same way as the forces acting at the poles of a magnet. Besides, the piles presented the peculiarity that when they were in full activity, that is, when their extremities were connected by a conducting-wire, the electrical attractions and repulsions could no longer be observed. But accident, which operated so fortunately in Galvani's discovery, was equally favourable to Œrsted. Yet it should be observed that in these, as in most other so-called accidental discoveries, the accident happens with any result only to the man whose mind is already prepared for and occupied with similar truths. It is related that in the winter of 1819 Œrsted, while lecturing before his class, was exhibiting the heating effects of Volta's pile on a slender wire, and he noticed that a compass-needle which happened accidentally to be upon the lecture-table at a little distance was set in oscillation, apparently at the instant the circuit of the pile was completed, that is, when the connecting-wires were joined so that the metallic communication between the two poles of the pile was complete. When his students had withdrawn, Œrsted hastened to repeat and vary the arrangement in which the disturbances of the magnetic needle occurred, and he soon found that when the wire joining the poles of the pile was brought near the compass, the needle was strongly deflected. This was the first step on a new path of exploration which the science of electricity now entered upon, and its progress has from that day to this

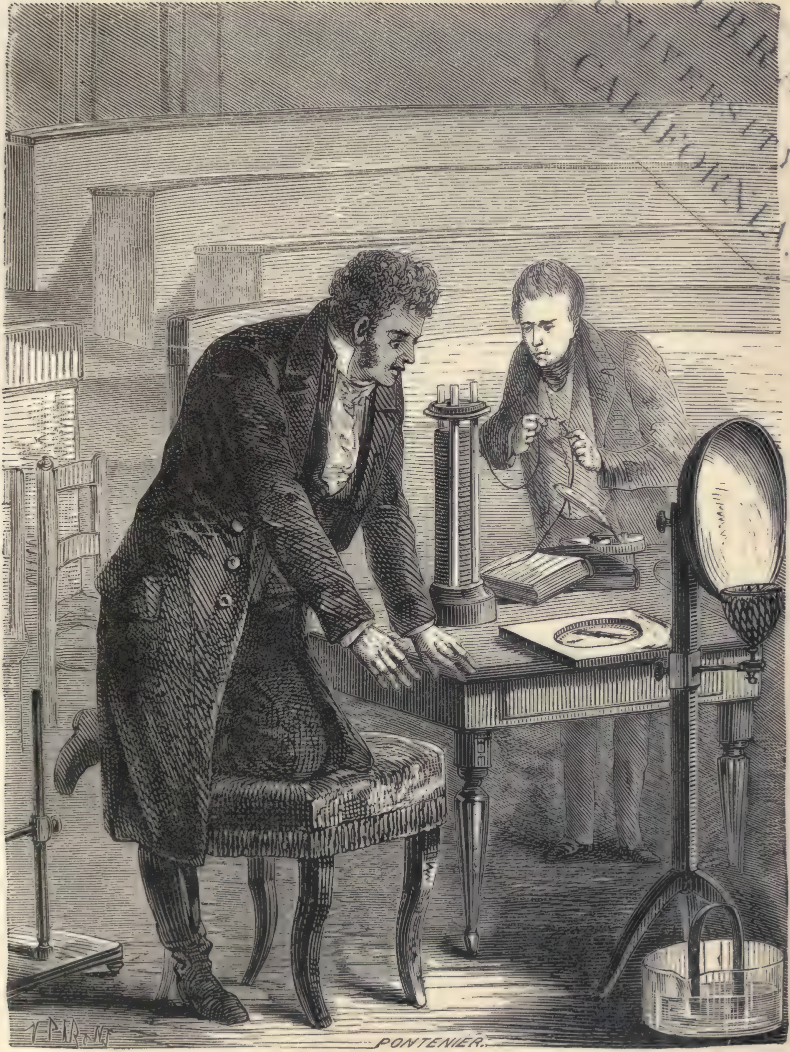

PLATE XIV.—ŒRSTED'S DISCOVERY.

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been marked by an unparalleled series of wonderful discoveries. That the practical applications to every-day life of certain of these discoveries now realize the wildest marvels of the magician is a fact known to all, and enhances the interest with which the first principles of electro-magnetism must be regarded.

Œrsted announced his discovery in a short pamphlet published in Latin in July, 1820. The Royal Society of London and other learned bodies acknowledged the merit of the discoverer by awards of medals and other distinctions. He was also raised to the highest scientific and social position in his native country. Œrsted's name will always be remembered in connection with the memorable event represented in our illustration, but he made also not a few valuable contributions to the physical and chemical sciences, and his scientific and philosophical writings attained a wide popularity,—especially the work translated into English under the title "The Soul in Nature."

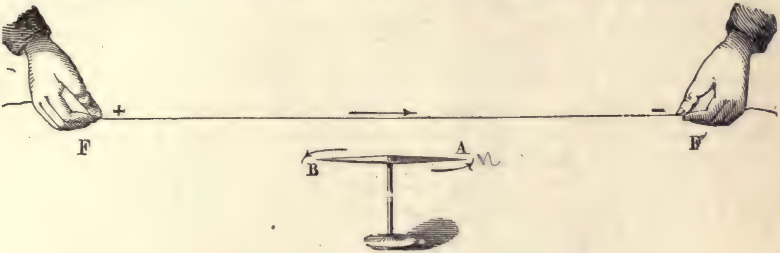

FIG. 272.

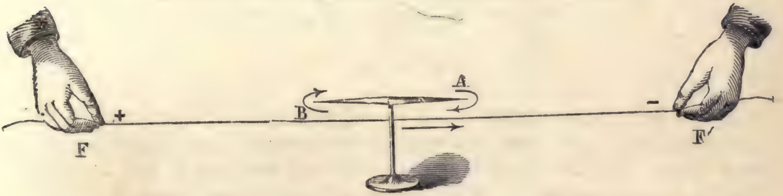

FIG. 273.

It behoves us now to state with exactness the conditions of Œrsted's fundamental electro-magnetic experiment. Let A B (Fig. 272) represent a magnetic needle poised on a pivot, and in its position of rest, in which it points nearly north and south (at least, it does so in Western Europe). The end of the needle pointing towards the north is supposed to be that marked A, and the needle to be lower than the spectator's eye. F F′ is a copper wire held above the needle and parallel with it. Now a copper wire has of itself no effect on the needle, but if the wire held as shown be connected at the end F with the positive pole (page 556), and at the end F′ with the negative pole of a voltaic pile or battery, an immediate effect is observed the moment these connections are completed. The needle swings round in the direction shown by the curved arrows at A and B, and finally settles in a position in which it is transverse to the wire, partially or entirely according to the strength of the current. The direction of this movement is always the same under the same circumstances. If the wire, instead of being held above the needle, be held below, as in Fig. 273, the direction of the movement is inverse to the former case. If the wire be held vertically instead of horizontally, deflection of the needle will also take place; and in all cases the deflected position into which the needle settles after a few oscillations will be maintained so long as the current continues to flow through the wire, kept in the same position. On the cessation of the current, or the removal of the wire, the needle reverts to its ordinary position in the magnetic meridian. We see by this experiment that the current flowing through a wire strangely modifies the surrounding space, which acquires the new property of magnetic polarity. The action of a wire conveying a current is defined and will be readily remembered by what is called Ampère's Rule. Imagine that a man is swimming with the current, that is, in the direction (page 551) in which it flows, and that his face is towards the needle, *then the north pole of the needle will be deflected towards his left.* This is illustrated in Fig. 274, where the action of each of the currents represented by the arrows would be to turn N, the north-seeking pole, out of the plane of the paper towards the spectator.

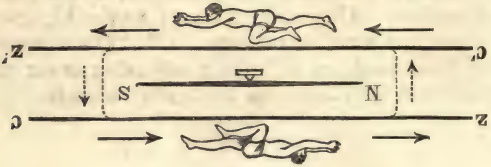

FIG. 274.

These details will at once make clear to the reader the meaning of the word "current" as applied to electricity. He will understand that every part of the circuit, including the pile or battery itself, has the same effect on the magnetic needle, and that the notion of a current, with its imaginary direction, is only a matter of convention. In the conducting-wire which unites the poles of an active voltaic pile the electric "fluids" are conceived to be constantly flowing, and the phenomena to which the current gives rise are said to be those of *dynamic electricity.* The opposite condition, namely, that in which the "fluids" are not flowing, is *static electricity.*

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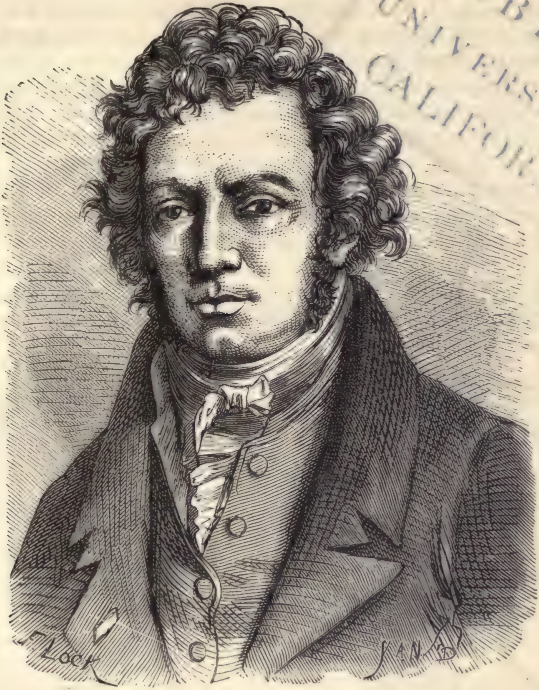

FIG. 275.—ANDRE-MARIE AMPÈRE.

ANDRÉ-MARIE AMPÈRE (1775—1836) had no sooner heard of Œrsted's discovery than he proceeded by ingeniously contrived experiments to extend and supplement it. If the fixed wire conveying a current attracts or repels a magnetic pole, a movable wire conveying a current may be expected to exhibit movements when a magnetic pole was brought near it. Ampère not only discovered that such was the case, but that similar forces were in action between currents. The mode in which he was able to render mobile the conductor conveying a current will be understood from Fig. 276, where C and D are two horizontal metallic supports through which the current circulates in the direction shown by the arrows. Each support terminates in a small iron cup, one cup being vertically below the other. The ends of the mobile conductor terminate in points or pivots, one of which turns in the upper cup, and the other in the lower cup. In each cup a globule of mercury is placed to insure perfect metallic contact. The mobile conductor A A has in the figure a circular form, but it may be rectangular or of other forms. Now, Ampère found that when another current was brought near a mobile one, as for example the wire B B represented in the figure as held in the hands, certain attractions and repulsions manifested themselves. The laws of these forces may be expressed thus: *Parallel currents attract each other if they are flowing in the same direction, and repel each other if they are flowing in opposite directions. The current free to move will always tend to place itself parallel to and in the same direction with the fixed current.*

Ampère was able to sum up all the observed facts of the mutual actions of magnets and currents by a theory of magnetism which supposes that round each molecule of iron and steel an electric current is continually circulating. In the ordinary condition of a piece of iron or steel these currents circulate in every possible direction, and the magnetization consists in giving to the molecular currents a determinate direction, which, in the case of steel, they permanently retain. This being admitted, all the rest can be deduced by the elementary laws of the mutual actions of currents (electro-dynamics).

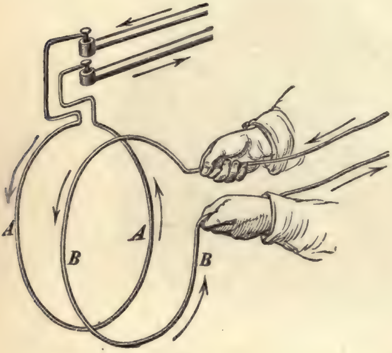

FIG. 276.

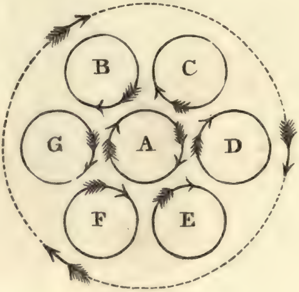

FIG. 277.

The resultant action of a series of currents circulating in one direction about the particles of a magnet would be equivalent to that of a single current moving in the same direction about the circumference of the magnet. If A, B, etc., Fig. 277, represent sections of the molecular filaments which collectively form a magnetized bar, it will be seen that the effects of those currents on external bodies will be neutralized as regards all the inner currents, because everywhere these are contiguous to similar currents in *the opposite* direction. The outer part of the bar will, however, have uncompensated currents, and the aggregate of these would be equivalent to a current circulating about the circumference of the bar. Fig. 278 represents the direction of the resultant currents in a bar magnet—N being the north, and S the south pole.

Ampère arranged electric conductors to convey the current in the same manner as he imagined the resultant of the molecular currents to circulate in a magnet. The conductors thus constructed consisted of a wire wound in the form of a helix, and suspended in such a manner as to admit of free movement. Such a conductor, called a *solenoid*, is represented in Fig. 279, and in Fig. 280 a mode of suspending it is shown by which it is free to move horizontally. Solenoids behave exactly like magnets; thus, when a strong current circulates through the apparatus represented in Fig. 280, C D will, under the influence of the earth's magnetism, turn and settle in the magnetic meridian; and, if the current be ascending in the side of the solenoid next the spectator, it will be the end C that will be directed towards the north. C may, therefore, be called the north pole of the solenoid, and D the south pole. If the pole of another solenoid be brought near C or D, there will be attraction or repulsion according as the poles are of diffferent or of the same names.

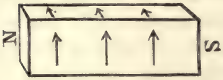

FIG. 278.

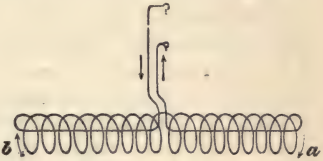

FIG. 279.

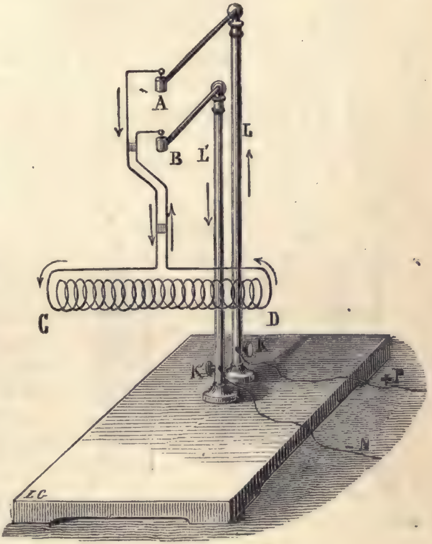

FIG. 280.

This ingenious and elegant theory presents the most important phenomena of magnetism, of electro-magnetism, and also those of magneto-electricity (a subject we have not yet arrived at) in the light of a single simple notion. For instance, it explains by the observed laws of electro-dynamics such facts as the induction of temporary magnetism in soft iron by permanent magnets and by currents. It explains magnetic attractions and repulsions, and shows how it happens that each fragment into which a magnetized bar may be broken is itself a complete magnet.

Ampère announced his discovery of the mutual action of electrical currents to the Académie des Sciences in September, 1820, a week after that body had received the report of Œrsted's discovery. Within a few days after the reading of Ampère's paper, another announcement, also the prolific germ of a new series of important discoveries, was made by FRANÇOIS ARAGO (1786—1853). It was his observation that a copper wire, while traversed by the current from a powerful battery, attracts iron filings, which cling to it on all sides so long as the current passes, but drop off when it is interrupted. The effect is produced with particles of iron only, and not with particles of brass, copper, etc. This experiment thus confirmed that of Œrsted in showing the connection between electric and magnetic actions. Arago succeeded also in making a sewing-needle permanently magnetic by placing it across the wire conveying the current. Ampère at once perceived that the most powerful effect would, if his theory were correct, be obtained by placing the needle within a helical coil of the wire. Arago and Ampère, pursuing these experiments, found that a bar of soft iron placed in such a coil (Fig. 281) became more powerfully magnetic during the time the current was passing than did steel; but on the cessation of the current, the iron reverted to its former condition. The poles of the permanent and temporary magnets thus formed were found to be in all cases those which Ampère's law requires, that is, the north pole was always on the left of current (page 562). The different cases are represented in Fig. 282, where N is the north and S the south poles of the bar, surrounded by current passing from + to —. In experiments of this kind the conducting-wires are covered by some insulating substances, silk, for example, in order that the current may be compelled to traverse their length. This discovery not only ex-

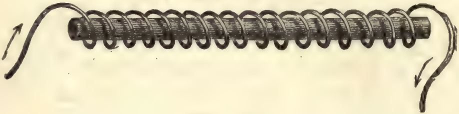

FIG. 281.

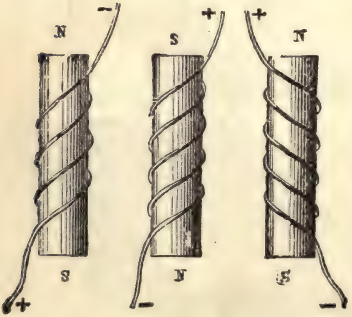

FIG. 282.

tended that of Œrsted and confirmed Ampère's theory of magnetism, but it placed at the disposal of the physicist magnets of a power immensely superior to any that could be constructed by other means. The magnetic energy which can be developed in soft iron by voltaic currents is almost without limit. This magnified power afterwards brought into view new and unsuspected properties of magnets.

The electro-magnet has therefore been the parent of other discoveries, and in numberless practical applications of electricity it is an essential part of the apparatus. As represented in Fig. 283, its usual construction will be readily understood. The wire, instead of forming but one helix, as in Figs. 281 and 282, is wound many times upon itself. For convenience the wire, which is usually of copper covered with silk thread, is wound upon a hollow wooden bobbin, A, capable of receiving in its axis a cylindrical bar of soft iron. This bar of iron becomes a powerful magnet the moment the coil is traversed by a current of electricity; it continues a magnet while the current continues, and on the interruption of the current instantly reverts to its ordinary condition of unmagnetic soft iron. Consequently, a piece of soft iron F, attached to a spring E, will, the instant the contacts with the battery are made, be drawn to the electro-magnet, and there retained so long as the current continues to flow; but, when the current ceases, F will be drawn back by the spring and resume its former position. As the battery which supplies the current may be at any distance whatever, provided only that proper conducting-wires connect it with the coil A, this combination offers obvious advantages for instantly calling into action mechanical forces at a distance.

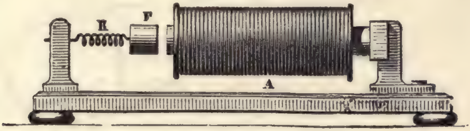

FIG. 283.

The applications of scientific principles to practical arts, however useful these applications may be, are not in general subjects coming within the range of the present work. The results which have been attained by Electric Telegraphy have, however, impressed upon the popular mind an idea of the wonder-working power of science. It is, therefore, by no means unlikely that the purely scientific discoveries of Volta, Œrsted, Ampère, and Arago, will, in the eyes of many, derive additional interest from a glance at the manner in which the discoveries of these philosophers have been made available for every-day purposes. Proposals to employ electricity as the means of telegraphy were made as long ago as the middle of the eighteenth century. But electricity

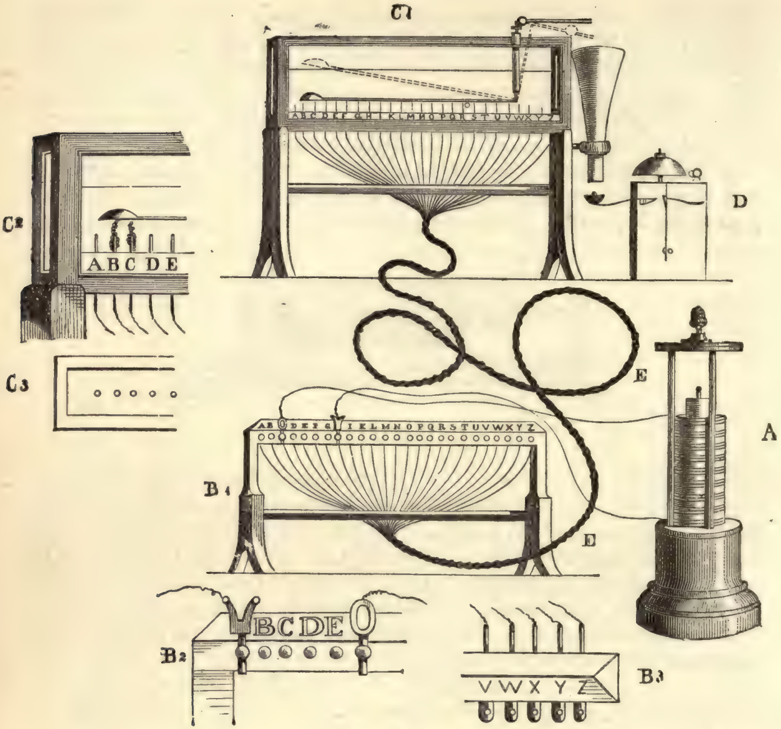

FIG. 284.

is generated by the machine under conditions less favourable than those which the voltaic pile presents. In 1809 Sömmering invented the first telegraph in which a voltaic current was employed. His apparatus displayed much ingenuity; but, although it was shown in operation, and from time to time short experimental lengths were set up, it never came into actual use. Sömmering's apparatus is represented in Fig. 284, where A is the voltaic pile. B$_1$ is a frame in which are fixed twenty-four short metal rods, each marked with a letter of the alphabet. Each rod is attached to a wire, and has a small hole drilled vertically through it. Part of this frame is shown on a larger scale in B$_2$, and in plan in B$_3$. The twenty-four wires, which are insulated from each other by a non-conducting covering, are twisted into a single cable, E E, which reaches to the station to which the messages are to be sent. Here the wires separate, and each passes through the bottom of a trough with glass sides, where it is marked with the corresponding letter of the alphabet. This arrangement is shown at C$_1$, and part of it on a larger scale at $C_2$, of which $C_3$ is the plan. The trough is filled with water, and the letters are indicated by the gas which rises from the ends of the wires by the decomposition of the water. When a message was to be sent, the attention of the observer at the receiving station was called by means of a bell rung by clockwork, and the clock was set in motion by a current from the sending station at the required moment through an ingenious adjunct to the trough. Over the wires marked B and C in $C_1$ and $C_2$ is shown in the figure what appears like a small inverted spoon. It is, in fact, a small hemispherical bowl attached to one end of a long lever, the other arm of which is bent twice at right angles, the whole being capable of taking up the position shown by the dotted lines in $C_1$. A leaden ball slides on the upper arm, and is so adjusted that when the water in the hemispherical bowl is displaced by gas rising from the wires B and C (as shown in $C_2$), the bowl rises to the top, and the lead ball slips off through the funnel-shaped vessel, and in falling acts on a detent, which releases the clockwork, and then the bell is rung. The polar wires of the pile terminate in metallic pegs, which are placed by the sender in the terminals where the letters he wishes to signal are marked. He would begin his operations by placing one at B, the other at C, and then after a sufficient interval for the setting in action of the alarum apparatus, and for obtaining the requisite attention at the receiving station, he would begin to spell out the letters of the message two at once. For gas will be seen to move from two of the terminals in $C_1$; and, as the reader is already aware, there will be twice as much hydrogen as oxygen gas (page 376) —a difference which is at once appreciable to the eye. It will be noticed that the pegs terminating the polar wire have different shapes. Suppose that with the V-shaped peg is attached to the negative pole of the pile; if they be placed as shown at $B_2$, the observer at $C_1$ would see hydrogen gas issuing from the "A" wire, and oxygen from the "F" wire; and he would read the letters *a-f*, it being agreed that the letter indicated by the large stream of gas should be read first in order. If the sender places the pegs as at $B_1$, the letters indicated would therefore not be *c h* but *h c*. By certain expedients afterwards suggested it would be possible to reduce the number of wires to two.

When Œrsted had announced his discovery of the action of the current upon the magnetic needle, Ampère at once suggested the application of the principle to telegraphy. At first it was intended to use as many needles and as many wires as there are letters in the alphabet. But in order that the action of a single wire (as represented in Fig. 272) should be distinct and rapid, a rather powerful current is required; and as such currents cannot be sent through long circuits except by very great battery power, practical success would not have been attained for the electric telegraph had not the same expedient which furnished electric science with one of its most useful instruments of research, also supplied the means of making the current from an ordinary galvanic battery produce its effect on the needle at very great distances. It was the idea of a physicist named Schweigger that the effect of a current on the magnetic needle could be multiplied indefinitely by carrying the conducting-wire many times round the needle. Fig. 285 represents such an arrangement, and the reader can have no difficulty in seeing that by Ampère's rule (page 562) every part of the coil A will concur in producing one and the same effect on the needle mounted on a pivot within the coil. In fact, with such a voltaic element as that shown, ordinary water might be the liquid in the vessel B, and the current would suffice to deflect the needle to be nearly perpendicular to the magnetic meridian. In all the needle telegraphs such "*multipliers*" were used. Baron Schilling in 1833 constructed a telegraph at St. Petersburg with five horizontally-mounted needles, which could be made to point out anything required on a dial. The idea was soon improved upon, notably by Professor Wheatstone in England, who made the five needles move in vertical planes. This was in 1837, and soon afterwards it was found possible to signal letters by the movement of a single needle. This, the simplest form of the needle telegraph, soon came into very general use. Fig. 286 represents the external appearance of the combined receiving and sending instrument. The handle in the base of the instrument, according as it is moved to the right or to the left, connects the line wire with one of the poles of the battery, and the current circulating through similar coils at each end of the line moves the needles right or left, at the will of the operator. There is a conventional system of signs: thus, two movements of the needle to the left indicates "A," etc.

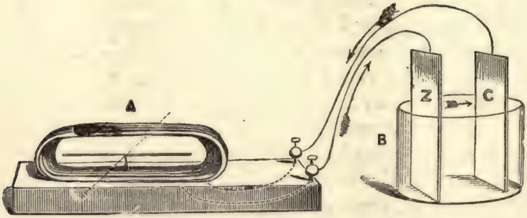

Fig. 285.—The Galvanometer.

Ampère's and Arago's discovery of electro-magnetism has been employed in a great number of ingenious telegraphic systems, in which messages are printed, or even written, at the distant station. As a printed message has certain obvious advantages over one received merely by the eye or ear, and leaving no permanent record, several kinds of printing telegraphs have been used; but, from its simplicity and efficiency, the telegraph devised by Morse has been far more generally employed than any other, and in the public service of England and America it has almost entirely superseded the needle telegraph. The

Morse apparatus, which prints the despatch at the receiving station, is shown in Fig. 287. At M′ the reader will not fail to recognize a pair of electro-magnets, above the poles of which he will see a bar of soft iron, B, attached to one end of a lever, A, turning on a pivot C. The other end of the lever has a projecting pin with a blunt point. This point, when B is depressed by the attraction of the electro-magnets, applies itself to a groove traced in the cylinder W; but when the current is not circulating it is drawn away by the action of the spring *f*. A continuous strip of paper, *p*, half an inch wide, is drawn from the roll R, by the action of clockwork, during the reception of the message, so that the pressure of the pin indents it with a longer or shorter line, according to the time during which the electro-magnet is in action. Different combinations of two signals, a very short line or dot, and a longer line, indicate the several letters. When a message has to be received, the clerk at the receiving station is warned; he sets the clockwork of his instrument in motion, and the message is automatically wound off printed in the conventional signals of *dot and dash*, and capable of being read off at leisure, and preserved for reference

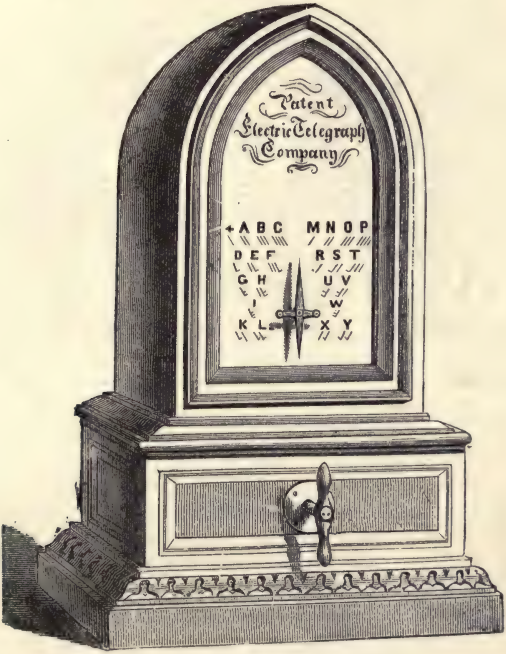

FIG. 286.—THE NEEDLE TELEGRAPH.

at any future period. By practice, however, the clerks recognize the signals by the sounds made by the instrument, and they are frequently able to write out the message by the ear alone. The sending instrument is simply a kind of spring lever, with which shorter or longer contacts are made, so as to produce the *dot and dash* characters. Figs. 288 and 289 show the operations of transmitting and receiving a message by the Morse instruments.

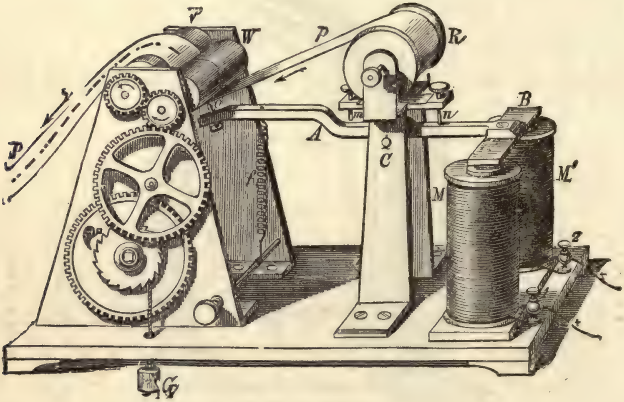

FIG. 287.

The scientific instrument constructed on the same principle as the telegraphic needle is called the *Galvanometer.* One form of it is represented in Fig. 290. A coil of fine copper wire, covered with silk, is wound round a slender wooden frame, within which a magnetic needle is free to move horizontally. This needle is usually attached to another similar needle immediately above the coil. The two needles are parallel, and their poles are in reverse directions. Two advantages are gained by this : first, the directive force of the earth is reduced to merely the difference of its contrary actions on the poles; second, the action of the coil on the upper needle is concurrent with the action on the needle within the coil. These *astatic* needles are suspended by a single fibre of silk. An instrument of this kind indicates the presence and the direction of the feeblest electric current. Thus, if the terminals are connected by wires with two pieces of different metals, the plunging of the metals into a tumbler of water will cause a considerable deflection of the needle. The direction of this deflection ascertained once for all for a known current, the instrument will always indicate the direction of any current; and indications of the relative strengths of currents are afforded by observations of the angle of deflection, as shown on the graduated circle. The galvanometer is one of the most delicate and most valuable instruments of research which science has placed in the hand of the electrician.

The discoveries of Œrsted, Ampère, and Arago completely established the fact of the close connection if not absolute identity of current electricity and magnetism. They showed not only that the current would affect the magnet, but that itself generated magnetism. The inverse transformation of magnetism into current-electricity was one of the great discoveries of a man who was the especial ornament of English science during the present century—MICHAEL FARADAY.

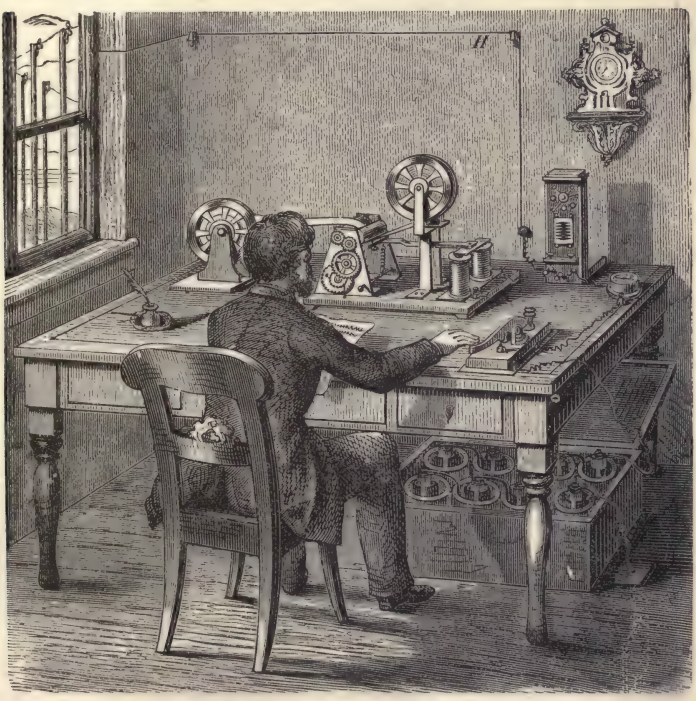

FIG. 288.—TRANSMITTING A MESSAGE.

Faraday was born at Newington, South London, on the 22nd of September, 1791. His father, James Faraday, was a journeyman blacksmith, who had a few years before come to London from his native village of Clapham, situated at the foot of Ingleborough Fell in Yorkshire. At the age of thirteen Faraday went as errand boy to a stationer and bookseller in Blandford Street, after an education consisting of little more than the rudiments of reading, writing, and arithmetic. After a year's trial, his master received him as a regular apprentice to the trade of stationer and bookbinder, and, in consideration of the value of his services, required no premium. Faraday remained with Mr. Riebau, the stationer and bookbinder, for seven years, during which he became proficient in the art of bookbinding. Nor did he neglect the opportunity of reading some of the books which passed through his hands. Amongst these he was especially interested in Mrs. Marcet's "Conversations on Chemistry" and the article on "Electricity" contained in an encyclopædia he was binding. The first book which roused his thoughts at all was Watts "On the Mind," and his attention was afterwards more particularly attracted to science by the article on "Electricity." He took an opportunity of attending a dozen private lectures on natural philosophy, for which his elder brother provided the fees. Through attending these lectures he became acquainted with some young men of similar tastes, and they established among themselves a Mutual Improvement Society.

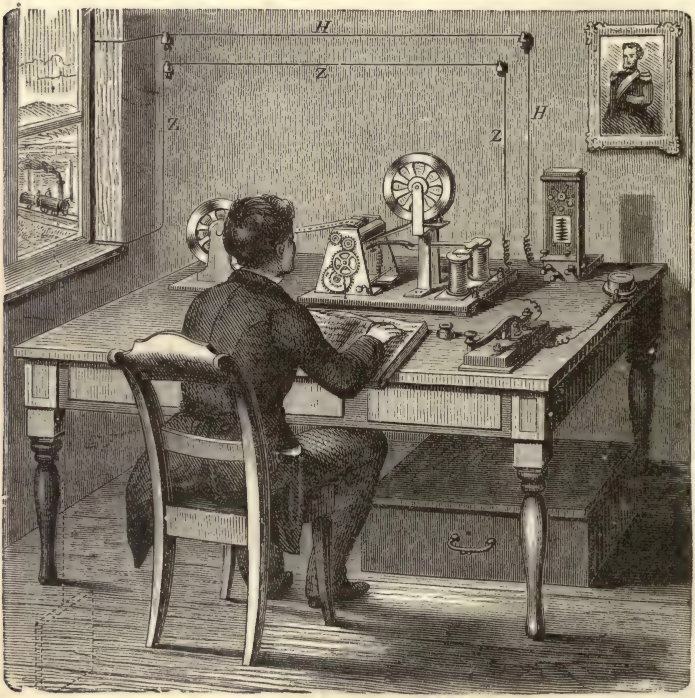

FIG. 289.—RECEIVING A MESSAGE.

By the kindness of one of his master's customers, Faraday was taken to the Royal Institution to hear the four last lectures which Sir Humphry Davy ever delivered, the dates being the 29th of February, 14th of March, 8th of April, 10th of April, 1812. He made notes of the lectures, wrote them out fully and neatly in a quarto volume, with drawings of the apparatus employed. This volume was soon afterwards sent to Davy with a letter in which Faraday expressed his earnest desire to enter upon some scientific employment, and hoped that Sir Humphry would favour his views if opportunity occurred. Davy at once sent a kind reply, and the result was that in March, 1813, he engaged Faraday as assistant in the laboratory of the Royal Institution at a salary of twenty-five shillings a week, with two rooms. In the minutes of the committee of management of the Royal Institution under date 1st of March, 1813, is this entry:

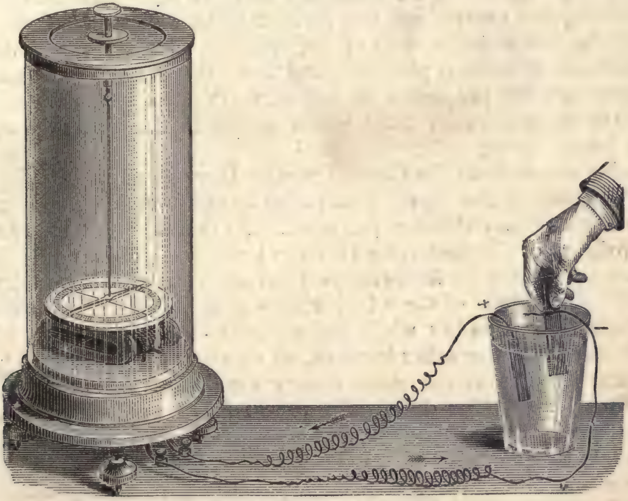

FIG. 290.—THE GALVANOMETER.

"Sir Humphry Davy has the honour to inform the managers that he has found a person who is desirous to occupy the situation in the Institution lately filled by William Payne. His name is Michael Faraday. He is a youth of twenty-two years of age. As far as Sir H. Davy has been able to observe or ascertain, he appears well fitted for the situation. His habits seem good, his disposition active and cheerful, and his manner intelligent. He is willing to engage himself on the same terms as those given to Mr. Payne at the time of quitting the Institution. Resolved,—That Michael Faraday be engaged to fill the situation lately occupied by Mr. Payne on the same terms."

The laboratory work in which Faraday was first engaged was the extraction of sugar from beetroot, and the preparation of sulphide of carbon, and of the dangerously explosive compound called the chloride of nitrogen. Davy and Faraday were both several times wounded by explosions of this substance. A few months after Faraday's appointment, Sir H. Davy set out on a visit to the Continent, taking Faraday with him as secretary and assistant in his experiments. This tour, which extended over a year and a half, was of the greatest advantage to Faraday, as he was brought into contact with the most distinguished scientific men of the day. Although the Continent was then closed to all English travellers, Sir Humphry and his party were allowed to pass freely, and everywhere the great chemist was received with distinction by the scientific men of the cities he visited. It was during his residence at various places abroad that Davy carried on his investigations into the properties of iodine, and on his return he entered upon the researches on flame which ended in the invention of the Davy Lamp.

On his return from the Continent with Davy in 1815, Faraday was again engaged at the Royal Institution as assistant in the laboratory at a salary of thirty shillings per week. At this time Mr. Brande was the Professor of Chemistry, Sir H. Davy being the Honorary Professor. In the beginning of the following year Faraday delivered his first lecture before the members of the "City Philosophical Society," of which he had become a member while yet a bookseller's apprentice. This was followed by five other lectures, all on chemical subjects. Other chemical lectures to the same society were given by Faraday in the following years. One delivered on the 19th of February, 1817, was printed under the title, "On some Observations on the Means of obtaining Knowledge, and on the Facilities afforded by the Constitution of the City Philosophical Society." In 1817 Faraday also began to publish papers in the scientific journals.

In 1821, after Faraday had been engaged for eight years as laboratory and lecture-room assistant in the Royal Institution, and as private assistant to Davy, he was appointed Superintendent of the Royal Institution. In the same year he married. This year is marked also by his discovery of the rotations about magnets of wires conveying currents. Among notes of subjects for future investigation, we find entered in his book for this year the following:

"Convert magnetism into electricity."

To ultimately accomplish this was the great glory of Faraday's scientific career. We see that the subject occupied his thoughts as early as 1821, but the work was really entered upon ten years later, and the account of it occupies the first part of his "Experimental Researches,"—eight large folio volumes of manuscript, in which he has detailed all his experiments, beginning in 1831 with paragraph numbered "1," and continued paragraph by paragraph, numbered, until he arrives at No. 15,389 in the year 1856. We shall have to devote several pages to the account of Faraday's great discoveries of magneto-electricity and induced currents. Leaving this account to follow these necessarily brief notes of the life and labours of this truly great man, we shall here quote a paragraph in which Dr. Bence Jones, in his "Life and Letters of Faraday," refers to what Faraday had accomplished by 1830, that is, before he had entered upon his famous electrical researches.

"If Faraday's scientific life had ended at this time, when he finished his higher scientific education, it might well have been called a noble success. He had made two leading discoveries, the one on electromagnetic motions, the other on the condensation of several gases into liquids. He had carried out two important and most laborious investigations on the alloys of steel and on the manufacture of optical glass. He had discovered two new chlorides of carbon; among the products of the decomposition of oil by heat he had found the bicarburet of hydrogen, or benzol; he had determined the combination of sulphuric acid and the formation of a new body, sulpho-naphthalic acid; and he had made the first experiments on the diffusion of gases, a subject which has become, by the researches of Professor Graham, of the utmost importance. According to the catalogue of scientific papers compiled by the Royal Society, he had had sixty important scientific papers printed, and nine of these were in the 'Philosophical Transactions.' From assistant in the laboratory of the Royal Institution he had become its director. He had constantly laboured in the great theatre, and he had probably saved the Institution by taking the most active part in the establishment of the Friday evening meetings."

For five and twenty years from the time when, in 1831, he began his "Experimental Researches," Faraday's scientific career extends. After the first ten years, the excessive strain upon his powers made it necessary for him to task his brain less continuously, and for a few years after 1839 the electrical researches were discontinued. During this period of comparative repose he visited Switzerland and the Rhine. In 1845 we find Faraday resuming his experimental researches in electricity, his principal discoveries at this period being the magnetization of light, "the magnetic condition of all matter," and atmospheric magnetism. The series of electrical researches begun in 1831 came to an end in 1855. It began with his two grand discoveries of the production of electric currents by magnets, and the induction of currents by currents; then (to use the enumeration given by Dr. Bence Jones) it continued with terrestrial magnetic induction, the identity of the electricities of the machine and of the battery, the investigation of conducting power; then came electro-chemical decomposition, the induction of a current on itself, static induction, the nature of the electric forces, the electricity of the gymnotus, the source of electricity in the voltaic piles, the electricity of steam, the magnetization of light, the illumination of magnetic lines of force, new magnetic actions and the magnetic condition of all matter, the crystalline polarity of bismuth, the possible relation of gravity to electricity, the magnetic and diamagnetic conditions of bodies, including gases, atmospheric magnetism, the lines of magnetic force, etc., etc. "The record of this work," says Dr. Jones, "which he has left in his manuscripts, and republished in his three volumes of 'Electrical Researches' from the papers in the 'Philosophical Transactions,' will ever remain as his noblest monument,—full of genius in the conception; full of finished and most accurate work in execution; in quantity so vast that it seems impossible one man could have done so much."

The laboratory investigations at the Royal Institution ceased after the close of the series on electricity. But after 1855 Faraday's powers began to show signs of decline. His last experimental investigation was made in 1862, to determine whether the spectroscopic lines were affected by polarized light under magnetic influence. On the 20th of June, 1862, he gave his last lecture at the Royal Institution, of which for thirty-eight years his discourses had formed the animating principle. Faraday quietly passed from this life while seated in a chair in his study at Hampton, where the Queen had placed at his disposal one of the houses on the Green. The plain stone which marks his resting-place in Highgate Cemetery has this simple inscription:

MICHAEL FARADAY.
BORN 22ND SEPTEMBER, 1791.
DIED 25TH AUGUST, 1867.

The singular nobleness of Faraday's character was as striking as his extraordinary genius for scientific research. Those who knew him personally all speak of his kindness, his simple-mindedness, his intense love of truth. Some sentences from the *éloge* of Faraday pronounced by H. Dumas, the distinguished French chemist, before the Académie des Sciences, may here be transcribed (as quoted in Dr. Gladstone's "Michael Faraday"), for they well express the sentiments of all who knew the man. "I am certain that all those who have known him would wish to approach that moral perfection which he attained to without effort. In him it appeared to be a natural grace, which made him a professor full of ardour for the diffusion of truth; an indefatigable worker, full of enthusiasm and sprightliness in his laboratory; the best and most amiable of men in the bosom of his family; and the most enlightened preacher amongst the humble flock whose faith he followed. The simplicity of his heart, his candour, his ardent love of truth, his fellow-interest in all the successes and his ingenuous admiration of all the discoveries of others; his natural modesty in regard to what he himself discovered; his noble soul, independent and bold, —all these combined gave an incomparable charm to the features of the illustrious physicist. I have never known a man more worthy of being loved, of being admired, of being mourned. Fidelity to his religious faith, the constant observance of the moral law, constitute the ruling characteristics of his life. Doubtless his firm belief in that justice on high which weighs all our merits, in that sovereign goodness which weighs all our sufferings, did not inspire Faraday with his great discoveries; but it gave him the straight-forwardness, the self-respect, the self-control, and the spirit of justice which enabled him to combat evil fortune with boldness, and to accept prosperity without being puffed up. There was nothing dramatic in the life of Faraday. It should be presented under that simplicity of aspect which is the grandeur of it. There is, however, more than one useful lesson to be learnt from the proper study of this illustrious man, whose youth endured poverty with dignity, whose mature age bore honours with moderation, and whose last years have just passed gently away, surrounded by marks of respect and tender affection."

That such a man as Faraday would receive innumerable scientific honours in the shape of medals and fellowships and honorary memberships of learned societies will be understood as a matter of course. It may, however, be stated that more than seventy scientific societies of their own accord elected him into their bodies. The academical degree of doctor was conferred upon him by several universities, and he was decorated with several foreign distinctions. The number of his various titles and tokens of honour was not far short of a hundred. He was at one time offered the highest scientific distinction in England —the Presidentship of the Royal Society: he declined it, preferring, as he said, "to remain plain Michael Faraday to the last."

The first experiment in which Faraday succeeded in producing electricity from magnetism is described in his note-book: He had made a ring of soft iron $\frac{7}{8}$ths of an inch thick, and 6 inches in diameter. He wound many coils of copper wire round the ring, the coils being separated from each other by interposed twine and calico. There were two separate lengths of wire, A and B, Fig. 291, each containing about 60 or 70 feet, and all the coils were wound in one direction. The extremities of the coil B were connected with a copper wire, which passed just over a magnetic needle at some distance. When the ends of the coil A were connected with a battery, P, an effect on the needle was perceptible: it oscillated, but soon settled in its original position. On breaking the

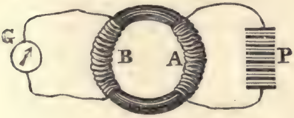

FIG. 291.

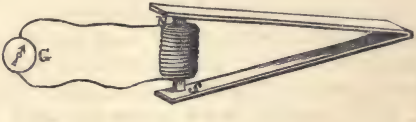

FIG. 292.

connection of the coil A with the battery, a disturbance of the needle again took place. Here the iron ring became an electro-magnet while the current flowed, and in the act of becoming and of ceasing to be a magnet, the ring so influenced the coil B that momentary currents were induced in that circuit. Faraday's next step was to place the experiment in such a form that no voltaic battery was used, the conversion of magnetism into electricity being distinct and direct. The experiment is represented in Fig. 292. A coil of insulated copper wire was wound round a cylindrical piece of soft iron, N S, and the ends of the coil were connected with a galvanometer, G, placed at some distance. The iron was then put between the contrary poles of a pair of bar magnets as shown in the figure. *Every time the contact between the iron and the magnets was made or broken* the effect on the galvanometer indicated that *a momentary current passed through the wire.* The current which appeared on breaking the contact was in the reverse direction to that which appeared on making the contact. In both cases the effect was momentary, and the continuance of the magnetism in the iron cylinder was not attended with any electrical effects. Faraday reasoned from this that a current would be produced in a coil if a magnet were introduced into the centre of the coil. Accordingly he took a coil made of about 200 feet of copper wire wound round a hollow cylinder, A B, Fig. 293, and having connected its extremities with a galvanometer, D (placed at a greater relative distance than shown in the figure), he put one end of a cylindrical bar magnet within the coil. While the bar magnet was being quickly thrust in the whole length the galvanometer needle moved; it then returned to its former position, the bar magnet being at rest within the coil. But while the magnet was being drawn out, the needle moved again, but in the opposite direction. These effects were repeated as often as the magnet was put in or taken out of the coil, and thus a wave of electricity was produced by the mere approximation, and another wave by the recession of the magnet, but no effect from the continuance of it in one position. Faraday soon succeeded in

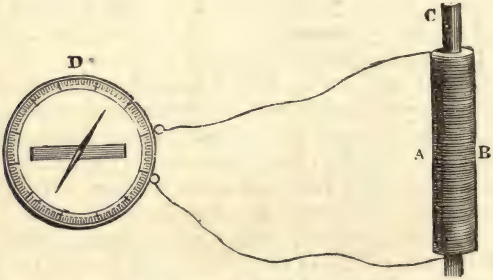

FIG. 293.—A MAGNET PRODUCING A CURRENT.

obtaining a spark by the electricity developed according to this principle from a natural magnet. These, together with certain other discoveries, were communicated by him to the Royal Society, in a paper read on the 24th of November, 1831.

It will be obvious that, as the proximity of the pole of a permanent magnet converts a piece of soft iron into a magnet, we may obtain a succession of currents by taking a soft iron cylinder wound round with insulated copper wire, and causing the pole of a magnet to approach and recede. But as the currents thus formed are in opposite directions alternately, they would, in general, neutralize each other's effect. But there are various expedients by which the direction of currents can be instantly reversed, and by employing some of them in connection with the movements of the magnet, it is easy to send currents in one direction only through a conductor external to the coil. H. Pixii, soon after Faraday's discovery had been announced, constructed an apparatus in which a large horse-shoe magnet was made to revolve beneath soft iron armatures covered with silk-covered copper wire. He was thus able to produce chemical decompositions, give shocks, exhibit sparks, and cause divergence of the gold leaves of an electroscope. An

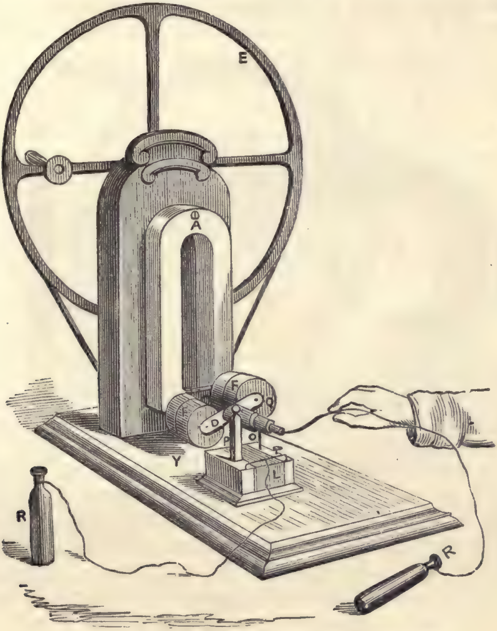

FIG. 294.—A MAGNETO-ELECTRIC MACHINE.

improvement of Pixii's apparatus was devised by Saxton, who made the armature move while the magnet remained fixed. The apparatus then assumed the form shown in Fig. 294, where A is a steel horseshoe magnet, and F F are the two armatures. At P is the contrivance for gathering up the currents in one direction. The wheel E, on which is fixed a handle for turning, is connected by an endless band with a small pulley on a spindle, which passes between the two coils, and to which they are attached by the cross-piece D. R R are brass handles used when the machine is employed to administer shocks.

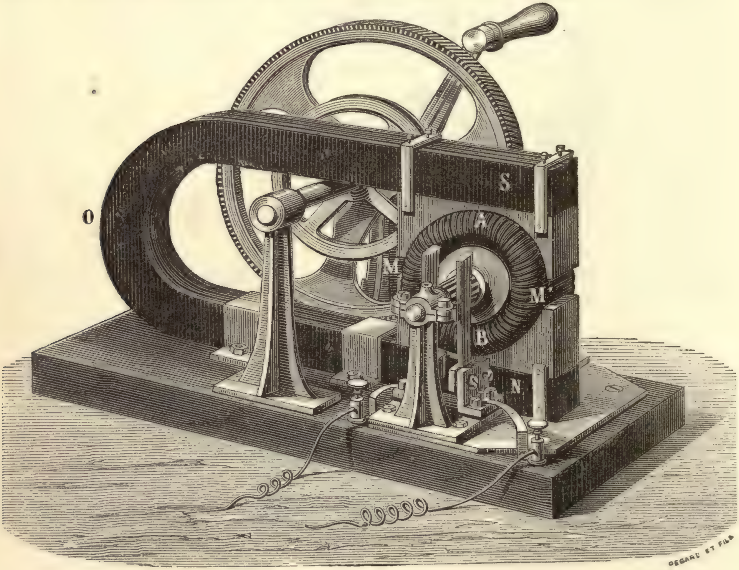

FIG. 295.—GRAMME MACHINE.

Many ingenious forms of powerful magneto-electric machines have been constructed in more recent times. Mr. Holmes in 1862 exhibited such a machine driven by steam power, and applied the currents it produced as a source of light. Mr. Holmes' machines were successfully set in action at some of our lighthouses, as the source of the electricity for the electric lamp. Every one who has of late years crossed the narrow part of the English Channel at night must have noticed the brilliant lights of the South Foreland, and the splendid beam from Cape Grisnez on the French coast: all these lights, be it remembered, are the immediate fruit of Faraday's discovery. Mr. H. Wilde, of Manchester, invented a powerful form of magneto-electric machine, in which the armatures are wound parallel to their axis of revolution, and in which part of the current is diverted to produce electro-magnets for its own generation. Mr. Wilde found, in fact, that the slight magnetism retained by iron suffices to generate a current which excites a greater magnetic power, and this in its turn gives rise to a still greater current, and so on. A still later form of the magneto-electric machine is the invention of M. Gramme, of Paris. A small table model of a machine of his invention is shown in Fig. 295. Its great peculiarity is the armature, which consists of a ring made of iron wires, with a large number of separate coils of insulated copper wire disposed radially, as seen in M A M′. The currents from these coils are collected at the proper point, so that the result is a regular and continuous current in one direction. N O S in the figure is a permanent steel magnet, composed of several plates bolted together, and the space between the poles and the annular armature is filled with blocks of soft iron. For use on the large scale the Gramme machines are constructed with several electro-magnets, and are driven by steam power. Magneto-electric machines, and not galvanic batteries, are the means by which current electricity is now generated for the production of light for lighthouses, for electro-plating, etc. The examples we have given of such machines will, it is hoped, suffice to enable the reader to observe for himself how all these later contrivances are essentially nothing but modifications of the simple arrangements which were devised in the laboratory of the Royal Institution in 1831 (see Figs. 291 and 292) by the illustrious discoverer to whose labours we once more revert by stating another of the great laws of electricity which he was the first to discover.

As early as 1825 Faraday tried whether one electric current would induce another current in an adjacent wire. He enters in his notebook the outcome of the experiment—"no result." In 1831 he finds a result, and he finds why he had before overlooked it. When two copper wires insulated from each other run for some distance side by side, a current passed through one causes a current in the other, if the extremities of this last are joined so as to form a complete circuit. But this induced current lasts but a moment; it occurs *only at the instant the battery current is established, and at the instant the battery current is interrupted.* In the first case the induced wave or current is in the *opposite* direction to the battery current; but in the second case, that is, when the battery current is stopped, the wave of induced electricity is in the same direction as the battery current. During the continuance of the battery current no current or other electrical phenomena are manifested by the wire forming the induced circuit. Faraday, however, believed at first that the wire of the induced current was, while the battery current continued, in a peculiar condition, to which he gave the name of the "electro-tonic state." But as no evidence could be obtained of any property other than that belonging to the wire itself, the idea was soon abandoned.

As the establishment of a current in the neighbourhood of a parallel closed circuit is equivalent to suddenly bringing a current near that circuit, Faraday tried the effect of moving an already established current up to a closed circuit. Fig. 296 shows the form of the experiment. H is a coil of insulated wire connected with the galvanometer at G through the wires A A; H′ is a coil through which the battery current is flowing from the wires P B, B N. When the coil H′ is held in the hands, and *is being* brought near H, a current flows through H and affects the galvanometer. Similarly, while H′ is receding from H a current flows, but in a direction opposite to that of the former one.

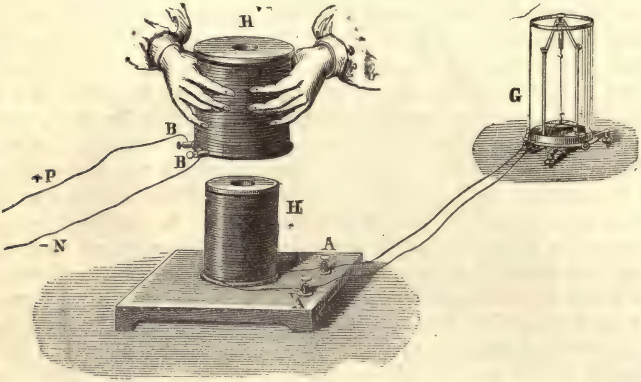

FIG. 296.

The direction of the current in H, on approximation of H′, corresponds with that of the induced current on the *establishment* of the battery. The current on recession corresponds with that which occurs on the cessation of the battery current.

Soon after this discovery of voltaic induction, coils were made provided with means of rapidly interrupting the primary or battery current, so that the effects of the induced currents should be continuous. Masson, in France, used as an interrupter a toothed metal wheel, with a spring touching each tooth as it passed. This was in 1836, and by 1848 several improvements had been introduced into the arrangement by Bréguet. A few years afterwards the voltaic-induction machine was perfected by RUHMKORFF, a skilful electrical instrument maker of Paris; and it has become an instrument of science itself prolific of new discoveries. It is now well known as *Ruhmkorff's Coil;* and about the coil and its inventor it behoves us to say a few words. M. Ruhmkorff was, as his name suggests, a native of Germany, where he was born in the early part of the present century. He came to Paris to learn the making of philosophical instruments, and afterwards established himself in that business on his own account. He was a workman of extraordinary intelligence and perseverance, and though himself not a professor of science, he gave to science in 1851 one of its most valuable instruments of investigation, and the best embodiment of the principle of voltaic induction discovered twenty years before by Faraday. He made the induction circuit with a far greater number of turns than had before been used, using fine wire very carefully insulated by gum-lac between the successive layers. The centre of the coils in Ruhmkorff's machine is occupied by a bundle of iron wires. Fig. 297 represents a large Ruhmkorff's coil. The inner coil which conveys the battery current is formed of rather thick copper wire; the outer coil is of fine covered wire very carefully insulated. The length of wire in the outer coil is much greater than that of the inner, amounting in the larger coils to 15 or 20 miles. The apparatus on the left is for the purpose of automatically interrupting and renewing the battery current with rapidity. It is, in the instrument represented in the figure, worked by a small separate battery, which sets the small electro-magnet into activity; and this acts upon an armature attached to a rocking lever, from which two platinum points descend into two small glass cups containing mercury covered with a stratum of alcohol. At each movement the points dip into the mercury, and this establishes a communication by which the current from the powerful battery is conveyed to the inner or inducing coil. This little apparatus, known as *Foucault's Interrupter*, was devised for the purpose of avoiding certain inconve-

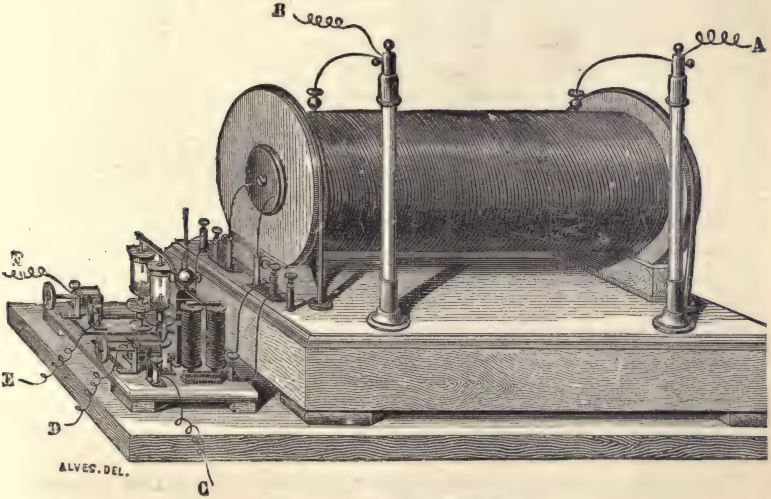

Fig. 297.—Ruhmkorff's Coil.

niences caused by sparks at the point where the interruption takes place. In the smaller Ruhmkorff coils a much simpler arrangement for breaking and making contact suffices, and no auxiliary battery is needed. At L there is a little arrangement for reversing the direction of the battery current. Its action will be readily understood by an inspection of the actual apparatus, but a description would be tedious. The end of the outer coil terminates at the brass caps A, B, mounted on glass pillars and provided with screws for attaching wires. The induction coils first made did not give a spark longer than $\frac{1}{5}$th of an inch; but when M. Fizeau attached a condenser to the inducing coil, sparks of much greater length were attainable. The induction coil is therefore now always constructed with a condenser, which is commonly contained in the hollow base of the instrument. The condenser is formed by pasting a sheet of tinfoil on each side of a piece of varnished silk or paper of considerable area. The compound sheet is folded up or divided into pieces which pack into the base of the instrument, and the tinfoil on one side is in communication with one pole of the battery coil, while that on the other side is connected with the other pole. The large inductive coils will give between the poles a constant stream of sparks several inches in length, attended by a loud cracking noise. There is a word of explanation required as regards the direction of the induced current. We have seen that an *inverse* induced current should occur when the battery current is established, and a *direct* current when it is broken. Now, for certain reasons which it would occupy too much space to explain here, the *direct* induced current has much greater energy than the inverse one; so that if the induced circuit be broken at any point, the sparks which appear there are due to the *direct current only*, the inverse current not having the power to leap across the interval.

The induction coil has been a boon to science. It supplies, without trouble or attention, a continuous stream of electricity of *high tension*, like that of the common machine, but far greater in quantity. By its aid we obtain the intensely ignited metallic vapours (for sparks are nothing else), which yield their characteristic lines in the spectroscope, and by passing the discharge through a nearly exhausted tube, we have the means of producing the spectrum of the residual gas. To give the spectra of gases is not the only property of the Geissler tube traversed by the induction current. There are, indeed, no more beautiful effects presented to the eye in any scientific experiment than those of the illuminated Geissler tubes. In these tubes the ignited gas assumes, under certain circumstances, a curious *stratified* appearance, of which the annexed Fig. 298 will give some notion. No very satisfactory explanation of this appearance has yet been advanced. It is highly probable that a complete examination of the many curious phenomena presented by Geissler's tubes may yet open to our view some of Nature's deepest secrets.

The great prize of 50,000 francs (£2,000), which was instituted by the late Emperor of the French (Napoleon III.) in 1852, for the most useful applications of the voltaic battery, to be awarded every five years, was not awarded to any person in 1857, or at all until 1864, when it was very deservedly obtained by M. Ruhmkorff for his induction coil.

We have already given a brief explanation of the two greatest of Faraday's great discoveries in electricity and magnetism, viz., magneto-electricity and the induction of currents by currents. The applications, scientific and industrial, of these principles are innumerable, and in both directions new applications and exemplifications are continually produced. Within the last few years, for example, Graham Bell's *Speaking Telephone* (page 546) has excited a great deal of popular interest. What, indeed, can appear more wonderful than that a man should be able to *hear* the very words which are spoken 100 miles away, and should even be able to recognize the voice of the speaker? The marvel is greater when the extreme simplicity of the telephone apparatus is seen; for of all instruments for communicating intelligence at a distance, the telephone, which reproduces the actual sounds of spoken words, is the simplest in construction. Its action depends upon the principles which have been explained in the present chapter, viz., magneto-electricity and electro-magnetism.

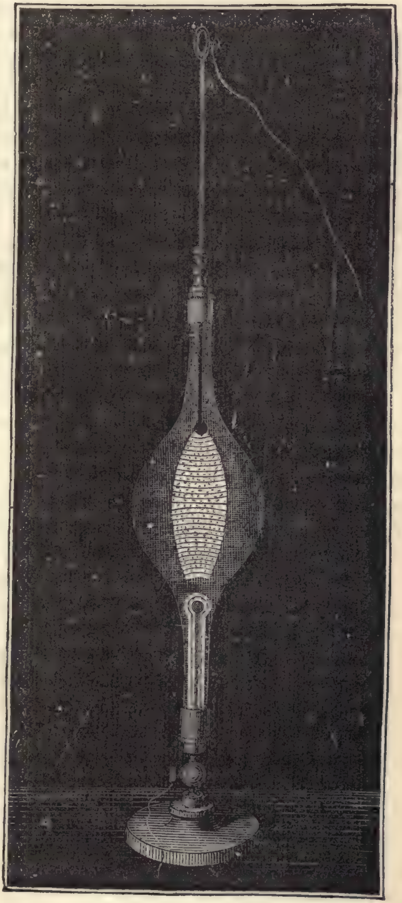

FIG. 278.—THE STRATIFIED DISCHARGE.

An account of all Faraday's electrical and magnetic discoveries would be equivalent to a treatise upon a great part of these sciences themselves, and this it is not our aim to attempt. We must, however, refer to his discovery of *Dia-magnetism*, because it showed magnetism to be a universal property. It was formerly supposed that only iron, cobalt, nickel, and compounds of these metals, exhibited magnetic properties. Towards the end of the eighteenth century the question was raised whether all bodies might not be affected by magnets; and some of the observations made about this time were sufficiently remarkable, but they failed to attract attention. Thus, it was found that a piece of bismuth is *repelled by both poles* of powerful magnets. Coulomb in 1812 noticed that slender wires of gold, silver, etc., or slender rods of glass delicately suspended between the poles of a powerful magnet, placed themselves at right angles to the lines joining the poles. In 1828 it was announced that certain substances, bismuth especially, repelled the poles of delicately-suspended magnetic needles. Up to 1845 these observations remained merely isolated facts, and they attracted but little notice. In the year just named Faraday discovered the influence of magnetism on polarized light. Thus, for example, when through a certain kind of glass occupying the centre of a coil a ray of polarized light is passed, the plane of polarization is, when the current is turned on, deflected in the same direction as the current. This led Faraday to examine the action of the magnetic forces on the glass itself, which was composed of lead with boric and silicic acids. He suspended a bar of the "heavy glass" by a silk fibre between the poles of a magnet. The glass bar was by no means indifferent to the magnetic forces, but instead of coming to rest in the direction of the line joining the magnetic poles, as a piece of iron would have done, it assumed a direction at right angles to this. We have here Coulomb's observation repeated. But Faraday extended his experiments to all kinds of substances, whether solid, liquid, or gaseous, and the result was that he was able to announce the law of magnetic action as affecting *all* substances whatever. All bodies admit of division into two classes: first, *magnetic* bodies, which are attracted by the magnet, and settle in the direction of the line joining the poles; second, *dia-magnetic* bodies, which are repelled by both poles and are turned across the line joining the poles. The first division contains (in addition to iron, nickel, cobalt, and manganese) several metals whose magnetic properties had not been suspected before Faraday's investigation, such as platinum, palladium, etc. The compounds of these metals are generally also magnetic. Among gases oxygen gas is strongly magnetic. In the dia-magnetic division are bismuth and certain other metals, the non-metallic elements, such as sulphur, phosphorus, carbon, etc. The same is the case with nitrogen, hydrogen, carbonic acid, and many other gases. Water, alcohol, essential oils, and most other liquids not being solutions of metallic salts are dia-magnetic. Animal substances come under the same class.

Very powerful magnets must be used in order to make manifest the general magnetism and dia-magnetic properties of all bodies. It is by means of electro-magnets of the largest dimensions, actuated by strong battery currents, that most of the facts have been ascertained. No doubt it was owing to the comparative weakness of the dia-magnetic forces that the phenomena were so long overlooked. The great importance to science of these discoveries consists in this: that whereas formerly magnetism was known only as an exceptional property of iron and of two or three other metals, it must now be recognized as a common property of all matter. The march of science is ever towards greater generality, hence the significance of the step by which a certain property is raised from being possessed by only particular substances into a universal property differing but in degree and direction from one body to another.

A theory the counterpart of that of Ampère, which so beautifully embraces all the facts of magnetism, electro-magnetism, and magneto-electricity, has been proposed in explanation of the facts of dia-magnetism. The currents of the inducing magnet are supposed in the case of dia-magnetic bodies to turn the currents in the *contrary* direction to themselves; but no satisfactory explanation has been offered of the cause of this inversion of the direction. Faraday, however, was of opinion that dia-magnetic bodies were destitute of *polarity*, the law of their action being the simple repulsion of their particles by the poles of a magnet. The question of polarity or non-polarity gave rise to much discussion, and it was not finally set at rest until after an investigation of the subject by Professor Tyndall (page 475), who, by elegant and conclusive experiments, demonstrated in 1856 the existence of both attractive and repulsive forces in dia-magnetic bodies.

The most notable steps in the progress of the science of electricity and magnetism (for now these are one) during the nineteenth century have now been described. They are these:—

1800. The voltaic pile (or galvanic battery) invented by Volta.
1820. Electro-magnetism discovered by Œrsted, Arago, and Ampère, and the mechanical actions between currents studied by the latter.
1831. Magneto-electricity and the induction of currents discovered by Faraday.
1845. Faraday investigates dia-magnetism and demonstrates the existence of magnetic properties in all matter.

It was a favourite idea of Faraday's that the various forces of nature, electricity, magnetism, heat, light, gravity, etc., are transmutable and, in a sense, identical. This theoretical notion was the starting-point of many of his experimental researches, and often when baffled by non-success, he would renew his attempts to discover the connection between the various forms of forces. That he did succeed in breaking down many of the partitions between this and that science, and that his discoveries tend largely to support the idea of unity in nature, is sufficiently obvious. A discovery was made in 1821 by Seebeck of Berlin, contributing another link to the bonds which connect electricity with other forces. Let A, Fig. 299, be a bar of antimony, and B a bar of bismuth, the metals being in contact at *h* and *c*. When heat is applied at *h*, or, to state the fact more generally, so long as the junction *h* is warmer than the junction *c*, a current of electricity will circulate in the metals in the direction shown by the arrows. A magnetic needle poised on a pivot within the circuits would indicate the current by its deflection. This was Seebeck's discovery. Not antimony and bismuth only, but any two metals whatever, will in their own degree produce the same effects. Later researches have shown that the transference of heat between any heterogeneous conductors suffices to set electricity in motion. Fig. 300 shows a galvanometer at A; S is a silver and P a platinum wire in contact at + ; it suffices to warm the junction of the wires by a spirit-lamp to obtain a strong deflection of the needle. If, instead of the silver and platinum wires between the terminals of the galvanometer, we make use of a single copper wire coiled or tied in a knot at +, we shall obtain a current on heating the wire at one side of the knot, and the reverse current on heating it on the other. Seebeck's discovery of *thermo-electricity*, besides its theoretical interest, had the effect of giving to science a delicate thermoscope, which in this work has been already described in connection with radiant heat (page 507).

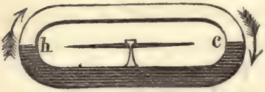

FIG. 299.

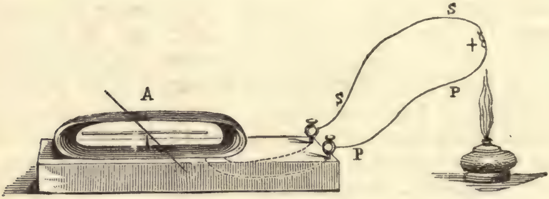

FIG. 300.

Electricity has arrived at the stage in which its laws resolve themselves into the statement of accurately-defined quantitative relations. The electrician must have, therefore, instruments for accurate measurement. These have not been wanting. An instrument far more convenient than the torsion balance (page 335) for measuring charges of static electricity is Sir W. Thomson's "attracted-disc" electrometer. Other instruments contrived by the same distinguished electrician will be found described in the books. The "mirror galvanometer" represented in Fig. 301 is a good example of the delicacy of modern electric

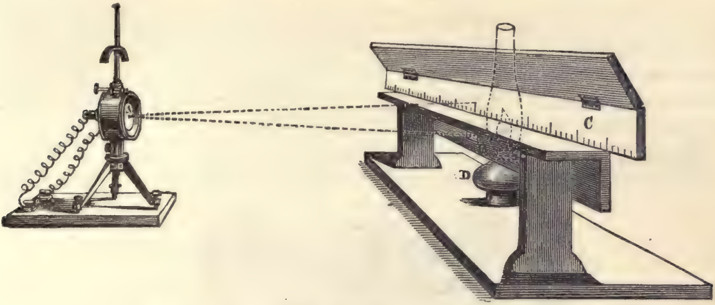

FIG. 301.—THE MIRROR GALVANOMETER.

instruments. The magnetic needle is reduced to a mere fragment of a steel watch-spring attached to the back of a circular mirror one-third

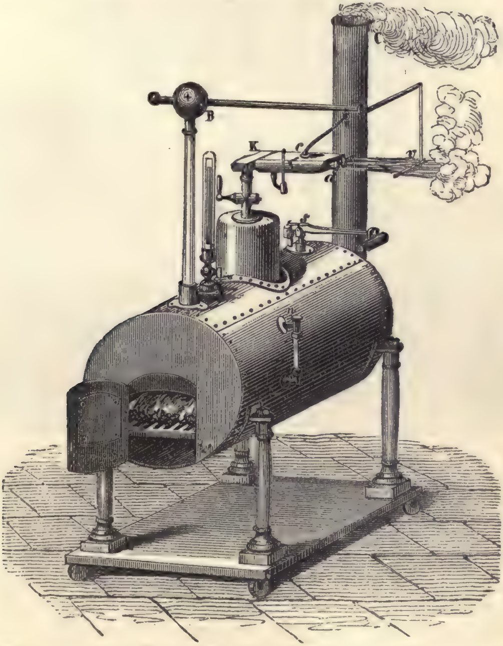

FIG. 302.—ARMSTRONG'S HYDRO-ELECTRIC MACHINE.

of an inch in diameter, made of glass as thin as paper. The needle is suspended by a single cocoon-fibre, in the midst of a coil of fine copper wire. The light of a lamp D passing through a narrow slit in a screen is reflected from the little mirror, and forms an image on the graduated scale C. Thus, the minute delicately-suspended magnet is provided in the beam of light with a weightless index-finger several feet in length, and the instrument will indicate currents which would altogether escape less refined methods of detection.

Besides the great discoveries which have been already treated of, numbers of others of much interest have been made, and many new forms of apparatus have been constructed, not only for the newer branches of electrical science, but in connection with that form of electricity which was the only one known up to the end of last century. Some of the newer forms of apparatus would appear very amazing to the older electricians. We have space to refer only to two of these, which at the time they were first constructed made some sensation in the scientific world.

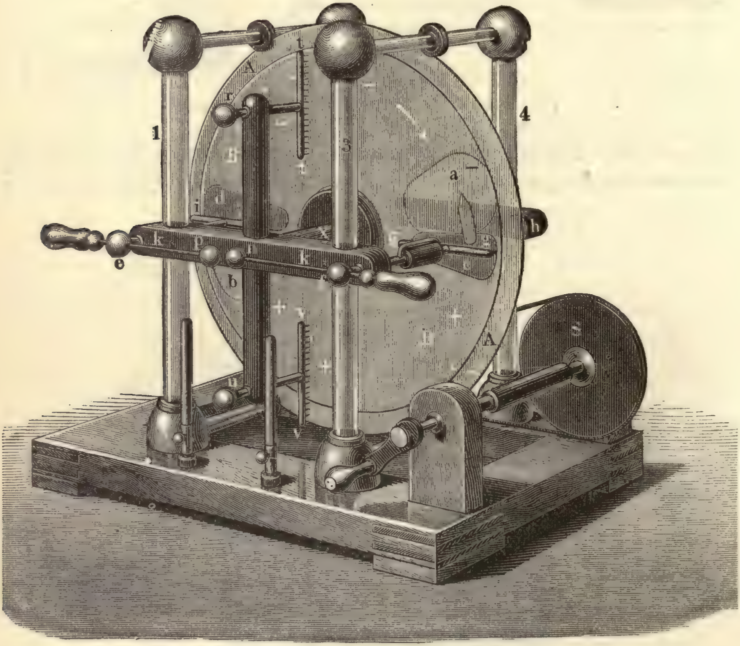

FIG. 303.—HOLTZ MACHINE.

In 1840 Mr. (now Sir) W. ARMSTRONG, of Newcastle-on-Tyne, found that under certain circumstances steam arising from a boiler under high pressure was charged with electricity. After some experiments had been instituted, the "*hydro-electric*" machine was constructed, as shown in Fig. 302. A strong iron boiler with an internal fire was mounted on four glass pillars. The steam was allowed to escape under a pressure of several atmospheres by a number of jets (*a*) of a peculiar construction. The steam was directed against a rod *v* connected with the insulated conductor B. The steam was positively and the boiler negatively electrified. A machine of this kind used to be exhibited at the Polytechnic Institution in London, where the boiler was 6½ feet long, and there were forty-six steam-jets. Sparks 22 inches in length could be drawn from the conductor. The electricity of the Armstrong machine is generated by the friction of particles of *water* against the sides of the wooden orifices from which the steam issues. *Dry* steam is found to produce no electricity: hence the tubes by which the steam escapes include an arrangement for obtaining a slight previous condensation of the steam.

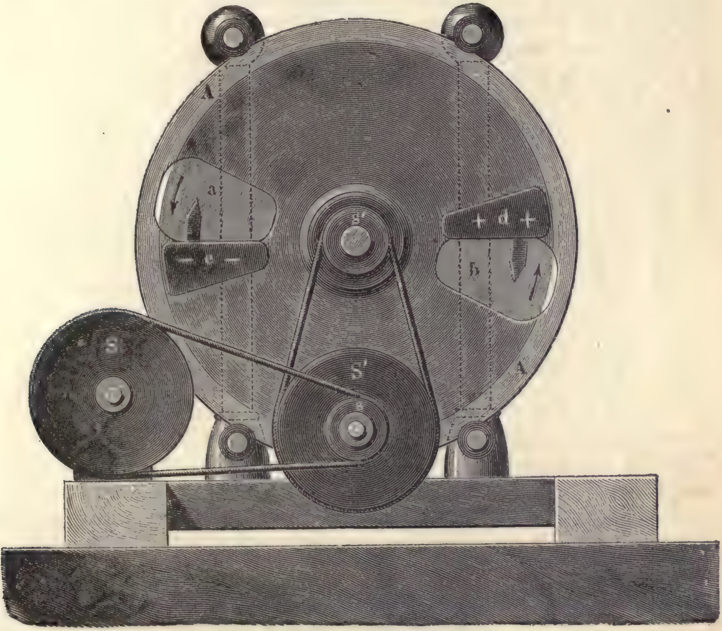

FIG. 304.

More recently some machines have been invented which supply electricity of high tension, that is, electricity like that of the common frictional machine, merely by the motion of a surface under induction. Figs. 303 and 304 represent one of the most effective of these machines, namely, the one invented by Holtz of Berlin, 1865. It consists principally of two circular plates of glass, of which one, A, is fixed and supported on insulators at four points. The other, B, is somewhat smaller, and capable of receiving a rapid rotation from the multiplying-wheels S S, S′ S′. In the fixed plate are two large holes, *a* and *b*. Adjoining these on the glass are two bands of paper, *c* and *d*, provided with pointed pieces which project into the opening in the opposite direction to that in which the plate B revolves. At certain positions opposite the movable plate are fixed the metallic comb-like rods *g g* and *i i*, *t t* and *v v*. When it is desired to set the machine in action, it is started by placing the knobs *p* and *n* in contact, and communicating a small charge of negative electricity to the paper armature C. By inductive action, negative electricity is expelled from B into *g g*, and the positive held by attraction until the revolving plate, having passed from the influence of C, the + electricity is set free, and is gathered up by the points in *v v*. The negative electricity received by *g g* passes through *n n* and *p*, which we have supposed to be touching *n*, into *i*, where by induction it charges the armature *d* with + electricity. This reacts on the revolving plate, in consequence of which – electricity is collected by *t t*. If after the plate has been turned a few seconds the knobs *p* and *n* are gradually separated, a stream of sparks will continue to pass between the knobs as long as the rotation is continued. A simpler machine subsequently invented by Holtz dispenses with both armatures and openings, retaining only the combs. The interest of these machines consists in the production of electricity by mere motion. There is no friction of surfaces, no chemical change; but the motion is opposed by the force of attraction between the + and – electricities, and therefore the electricity is gained only at the cost of the muscular force required to overcome the opposing attraction.

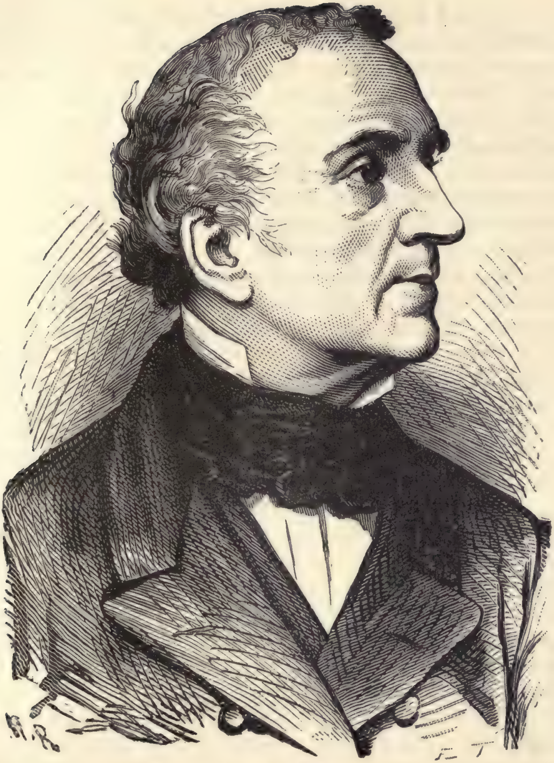

FIG. 305.—LIEBIG.

# CHAPTER XXI.

## CHEMISTRY OF THE NINETEENTH CENTURY.

JOHN DALTON was born in 1766 at a small village near Cockermouth in Cumberland, where his father pursued the humble vocation of a hand-loom weaver. Dalton's parents, like those of Thomas Young, belonged to the amiable sect of "Friends," perhaps more commonly known under the name of Quakers. His only school training he received from a young Friend of ability, who was acting as a schoolmaster in the neighbourhood. Dalton always entertained the highest esteem for this John Fletcher so long as he lived, and afterwards continued to hold his memory in honour. The Quaker training in school would offer regular habits and quiet self-restraint, which would enhance the value of its intellectual discipline, while the latter was much superior to that provided by other schools of the kind. Dalton, however, was by no means a quick boy, and attracted his master's attention only by his plodding industry. His perseverance carried him through arithmetic and navigation before he was twelve years of age, when the lad's abilities in a mathematical direction appeared to a rich Friend of the neighbourhood worthy of special encouragement and assistance. Young Dalton was, in fact, soon emboldened to open a school in his native village, on his own account, when he was scarcely thirteen years of age. At first his school was carried on in an old barn, but afterwards in the Friends' meeting-house. The scholars were of both sexes, and all ages between four and seventeen. But while the rustic Quaker lad was imparting the very rudiments of learning to these simple villagers, he continued to assiduously advance himself in higher knowledge. His nights were devoted to improving himself in classics, sciences, and mathematics. In 1781 Dalton joined his elder brother to carry on at Kendal a school where the elder brother had been previously engaged as assistant. The instruction which the Dalton brothers had to give in this school would appear to have been of the most elementary kind; but we find that already in his twentieth year John Dalton aimed at higher things than were embraced in the actual school course. In October, 1787, he issued a prospectus of public science lectures in Kendal. The announcement was made in these terms:—

"Twelve Lectures on Natural Philosophy to be read at the school (if a sufficient number of subscribers are procured) by John Dalton. Subscribers to the whole, half a guinea, or one shilling for single nights. N.B.—Subscribers to the whole course will have the liberty of requiring further explanation of subjects that may not be sufficiently discussed or clearly perceived when under immediate consideration; also of proposing doubts, objections, etc.; all of which will be illustrated and obviated at suitable times to be mentioned at the commencement."

The syllabus of subjects comprised mechanics, optics, pneumatics, astronomy, and the use of the globes. The same course of lectures was given again four years afterwards, when the fees for admission were only one-half of those named in the former syllabus. From this period it seems that the delivery of lectures at Manchester and elsewhere became one of Dalton's regular avocations and an important means of increasing his slender income.

At Kendal, Dalton devoted no little attention to the natural history of the district; but his favourite subject was meteorology. We find him regularly recording his observations of the barometer, thermometer, hygrometer, rain-gauge, etc. He constructs his own thermometers and barometers, and even supplies these instruments to his friends for the respective sums of five shillings and eighteen shillings. After having lived twelve years at Kendal, Dalton in 1793 removed to Manchester to enter upon the appointment of Lecturer of Mathematics and Natural Philosophy at a college which certain Nonconformists had recently established at Manchester. The emoluments of the office, apart from the "commons and rooms in the college," did not amount to more than fifty guineas a year clear money. The duties, however, required for their performance only twenty-one hours each week, and the time which might be requisite for the preparation of each lecture. The total number of students attending the "New College" in the first session of his residence there is mentioned by Dalton as twenty-six. A paragraph from a letter of his, dated "2nd mo., 20th, 1794," may be quoted as a little picture of a philosopher in his study.

> "There is in this town a large library furnished with the best books in every art, science, and language, which is open to all, gratis; when thou art apprised of this and suchlike circumstances, thou considerest me in my private apartments, undisturbed, having a good fire and a philosophical apparatus around me, thou wilt be able to form an opinion whether I spend my time in slothful inactivity of body and mind. The watchword for my retiring to rest is, 'Past 12 o'clock—cloudy morning.'"*

Soon after his arrival in Manchester, Dalton became a member of the Literary and Philosophical Society of Manchester, an association which has always exhibited no small degree of vitality, and whose records probably include a greater number of papers of high importance than those of any other scientific organization of a similar extent. On the 31st of October, 1794, Dalton read his first paper before the members of this society. Its title was "Extraordinary Facts relating to the Vision of Colours, with Observations," and in it he describes his own *colour blindness*. This phenomenon had before this time been little noticed; but it has since been a theme of much discussion and experiment. Interesting as are the facts connected with this subject, they must here be passed over. From October, 1794, to April, 1844, Dalton contributed to the journals of the Manchester society no fewer than 116 papers. He contributed likewise many papers to other scientific journals, and issued several independent publications, the most important of which was his "New System of Chemical Philosophy," the first part of which appeared in 1808, the second part in 1810. In 1800 Dalton was elected to the secretaryship of the Literary and Philosophical Society of Manchester, in 1808 he became the vice-president, and in 1817 he was made president, and filled the chair during the rest of his lifetime. All the various academical distinctions that can fall to the lot of a man of science were conferred upon Dalton from time to time after the publication of his great work. He received a pension at first of £150 per annum, which was afterwards doubled. Dalton died on the 27th of July, 1844, and Manchester honoured her great citizen by a public funeral, in which the procession was nearly a mile in length.

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* For the benefit of younger readers it may be explained that the last words refer to one of the functions of the old watchmen, who were superseded by our present well-organized police force. It was one of the duties assigned to these old worthies, when on their beats during the night, to proclaim aloud the hour and the state of the weather.

In one of his earliest papers (1802) on chemical subjects, Dalton mentions a case of combination in multiple proportion: "The elements of oxygen may combine with a certain portion of nitrous gas, or with twice that portion, but with no intermediate quantity." A little later he found that the constituents of olefiant gas and of carburetted hydrogen gas were in each case nothing but carbon and hydrogen, and also that for the same weight of carbon, carburetted hydrogen contains exactly twice as much hydrogen as olefiant gas. In the "New System of Chemical Philosophy" he takes the figures of Cavendish and Davy, by which the per-centage compositions of three different compounds of nitrogen and oxygen are expressed, and he shows that they agree with the doctrine of multiple proportions.

The Atomic Theory has so greatly influenced the progress of chemistry that it is desirable the reader should realize the nature of the data upon which it is founded. Perhaps no better illustration of the law of multiple proportions, and of the atomic explanation of that fact, can be given than the instance of the compounds of nitrogen and oxygen which Dalton himself gives. We shall give this in a tabular form somewhat similar to that set forth in Dalton's work; only, instead of using the figures he quotes, which are results of but imperfect analysis, we shall adopt those which the more exact and refined methods now in use place at the disposal of the chemist. Five different compounds of nitrogen and oxygen are known.

| | Name. | Composition in 100 parts by weight. | | Quantities of oxygen combined with a fixed weight of nitrogen. | | Atomic representation. |
|---|---|---|---|---|---|---|
| | | Nitrogen. | Oxygen. | Nitrogen. | Oxygen. | |
| 1 | Nitrous Oxide . . . | 63·64 | 36·36 | 28 : | 16 or 16 × 1 | ○<br>●● |
| 2 | Nitric Oxide . . . | 46·67 | 53·33 | 28 : | 32 or 16 × 2 | ○○<br>●● |
| 3 | Nitrous Anhydride . | 36·84 | 63·16 | 28 : | 48 or 16 × 3 | ○○○<br>●● |
| 4 | Peroxide of Nitrogen. | 30·44 | 69·56 | 28 : | 64 or 16 × 4 | ○○<br>○ ○<br>●● |
| 5 | Nitric Anhydride . . | 25·93 | 74·07 | 28 : | 80 or 16 × 5 | ○○<br>○○○<br>●● |

It will be observed that if we suppose the existence of atoms of nitrogen and of oxygen of invariable weights, the weight of each nitrogen atom being to the weight of each oxygen atom in the proportion of 14 to 16, the fact of multiple proportions in the compounds of these substances is obviously explained by the union of atoms in the definite groups represented in the last column of the table.

These are facts, and they were accepted as such by those who rejected the atomic theory, which Dalton had the merit of introducing, or at least of definitely applying to chemical facts. The idea of atoms, as we have seen, was familiar to the philosophers of ancient Greece—Leucippus, Democritus, Epicurus, and others. Modern philosophers, such as Descartes, Spinoza, Leibnitz, Newton had propounded the same conception under different forms. Nearer to Dalton's own time, Boscovich and others had indulged in speculations of the like kind. These speculations have in general no specific reference to the constitution of substances from the chemist's point of view. But an author, whose works Dalton had never perused, anticipated the Manchester chemist in expressing chemical combinations as the union of ultimate particles. This author was WILLIAM HIGGINS, who published in 1789 a treatise upholding the phlogistic theory. It is, however, certain that the atomic theory of chemistry was not established in this book. The conception is mentioned incidentally as applicable to a few cases, without being followed up or announced as a general law. It is the fact that no student of Higgins' work received from it the conception of the atomic theory.

The rise of the conception of the atomic theory may be traced to Dalton's study of the physical properties of gases. He gave out as early as 1803 a series of numbers representing the relative weights of the ultimate particles of certain chemical elements. Perhaps the best exposition of Dalton's theory will be a few paragraphs from the "New System of Chemical Philosophy." "When any body exists in the elastic (gaseous) state, its ultimate particles are separated from each other to a much greater distance than in any other state: each particle occupies the centre of a comparatively large sphere, and supports its dignity by keeping all the rest, which by their gravity or otherwise are disposed to encroach upon it, at a respectful distance. When we attempt to conceive the number of particles in an atmosphere, it is somewhat like attempting to conceive the number of stars in the universe: we are confounded with the thought. But if we limit the subject by taking a given volume of any gas, we seem persuaded that, let the divisions be ever so minute, the number of particles must be finite; just as in a given space in the universe the number of stars and planets cannot be infinite.

"Chemical analysis and synthesis go no further than to the separation of particles one from another, and to their re-union. No new creation or destruction of matter is within the reach of chemical agency.

We might as well attempt to introduce a new planet into the solar system, or to annihilate one already in existence, as to create or destroy a particle of hydrogen. All the changes we can produce consist in separating particles that are in a state of cohesion or combination, and joining those that were previously at a distance.

"In all chemical investigations it has justly been considered an important object to ascertain the relative *weights* of the simples which constitute a compound. But unfortunately the inquiry has terminated here; whereas, from the relative weights in the mass, the relative weights of the ultimate particles or atoms of the bodies might have been inferred, from which their number and weight in various compounds would appear, in order to assist and guide future investigators and to correct their results. Now, it is one great object of this work to shew the importance and advantage of ascertaining *the relative weights of the ultimate particles both of simple and compound bodies, the number of simple elementary particles which constitute the compound particles, and the number of less compound particles which enter into the formation of one more compound particle.*

"If there be two bodies, A and B, which are disposed to combine, the following is the order in which the combination may take place, beginning with the most simple, namely:—

"1 atom of A + 1 atom of B = 1 atom of C, binary.
1 atom of A + 2 atoms of B = 1 atom of D, ternary.
2 atoms of A + 1 atom of B = 1 atom of E, ternary.
1 atom of A + 3 atoms of B = 1 atom of F, quaternary.
3 atoms of A + 1 atom of B = 1 atom of G, quaternary, etc., etc."

In the course of the work the experiments are detailed which give support to these views. The author gives plates in which the ultimate atoms of the chemical elements are represented by arbitrary signs, consisting of circles distinguished by some mark or letter. The annexed examples will serve as specimens of Dalton's graphic representations of the ultimate particles.

It will be observed that Dalton speaks of C, D, E, F, and G as *atoms*. This is of course a departure from the original meaning of the word, which denotes something incapable of any division, whereas these binary, ternary, etc., atoms are divided when the compounds are resolved into their elements. No little confusion has at times arisen from this ambiguous use of the term *atom*. "Ultimate particle of water" would be a better expression for ○⊙ than "atom;" "*ultimate*," because by further division it would cease to be water, and become an oxygen atom ○ and a hydrogen atom ⊙.

The definiteness of theoretical view which Dalton's hypothesis gave to chemistry has proved of wonderful service to the science. It became the bond by which chemical theory is linked to that of other sciences; and since Dalton's time, it has entered into the very texture of the science, so that nearly all its generalizations are expressed in terms of this theory. It has, however, been contended that since the things that actually come under the chemist's observation are not atoms but the relative weights of the elements in a compound, and the reactions of substances under given circumstances, so the facts may be summed up in general statements without introducing hypothetical ideas at all. This is no doubt true; but combinations of *equivalents* of substances present vague ideas to the mind compared with the more definite conception of the atoms. These address themselves to our imaginations as images of concrete things.

Dalton conceived of his atoms as solid particles having some definite shape or figure, concerning which, however, he offered no speculations. But he pictured to his imagination a jar of gas as like a vessel filled with small shot, the shot-corns being all of the same size. The particles of the gas Dalton conceived to differ from the shot in that while the shot globules are uniformly hard throughout, the gas particles "are constituted of an exceedingly small central atom of solid matter, which is surrounded by an atmosphere of heat of great density next the atom, but gradually growing rarer, according to some power of the distance." Dalton's conception was, therefore, perfectly clear. There was the very small *solid* particle in the centre of an atmosphere of the more subtle substance, which the philosophers of Dalton's time called *caloric*. It was the repulsive force of the *caloric* which opposed itself to the tendency of the solid particles to approach each other, and the same force withstood the external pressure to which the gas might be subjected.

We have already seen that the doctrine of phlogiston was completely overthrown by Lavoisier. This doctrine seemed in its time to explain many phenomena of calcination, reduction, etc., and men firmly believed that *phlogiston* had a real existence. The *caloric* theory superseded phlogiston by better explaining a wider range of phenomena. *Caloric* was, to almost the middle of the present century, a very clear and definite conception in the minds of all scientific men. But the eighth decade of the century finds *caloric* relegated to the same *limbo* of things forgotten as *phlogiston*. The *caloric* theory has in its turn been replaced by another doctrine of far greater scope. We have already discussed this modern doctrine on page 530, so that it need not be more particularly alluded to in this place. The reader's attention is here drawn to the succession of doctrines held on the subject of heat, in order to prevent him from falling into the somewhat prevalent mistake of considering that atoms are *proved* to have a real existence. They are mental creations, like phlogiston or caloric, and hold their place in our conception for the same reason which once made of these last important scientific doctrines; that is, they enable us to form consistent representations in our minds of a certain range of phenomena. Very wide is the range of phenomena which the atomic hypothesis embraced; but whenever another hypothesis shall be found covering with equal or greater consistency a still wider range of phenomena, then the atomic hypothesis will be superseded. The atoms are not objective facts; they have never presented themselves to the senses. In Dalton's, and in all conceptions of the kind it may be seen that the mental process consists in transferring to the supposed existences those very properties which are observed in visible and tangible masses of matter. Thus in the passages from Dalton alluded to above, we have *shape*, *solidity*, *atmospheres*, *densities*, *elasticity*. It has been said that though with our present means of observation the *objective* existence of atoms is incapable of direct demonstration, yet it is not impossible but that by some immense extension of the power of our microscopes we might be able to behold the atoms. If this should come to pass, there would inevitably arise the very same questions as to the constitution of the atoms that we now ask ourselves about the constitution of matter. We should begin to speculate about the still invisible atoms of which the visible atom might be supposed to be constructed, and on which *its* properties would be supposed to depend. The process could never terminate. Atoms might be conceived within atoms:—

So naturalists observe a flea
Has smaller fleas that on him prey,
And these have smaller still to bite 'em,
And so proceed *ad infinitum*.

It has been laid down as necessary for a good theory that the laws by which the theoretical agency is supposed to act must be the same, or at least not greatly different from those according to which some agency analogous to the supposed one is *known* to act; or, on the other hand, that we must refer the phenomena to an agent known to be concerned in the action, and limit the theory to laying down hypothetical laws as to the agent's mode of action. That is, we are not at liberty to assume a wholly hypothetical agency acting according to laws inferred only from the phenomena to be explained. Now, in adapting the atomic hypothesis to the facts which new discoveries have from time to time brought to light, chemists have been obliged to attach other hypotheses to the original one. The original hypothesis in its simplicity has been found incapable of explaining the new classes of facts, and therefore for each of these classes an extra hypothesis has been added on to the original foundation, so that the theoretical structure of chemistry has somewhat of the aspect of an inverted pyramid, of which Dalton's hypothesis is the apex.

The theory of the existence of matter in particles of definite weight perhaps now derives its validity less from the simple laws of chemical combinations as propounded by Dalton than from its accordance with

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certain classes of facts which have since attracted the attention of chemists. Many of the laws relating to matter in the gaseous state are known, and the uniformities they present are very striking when compared with diversities presented by liquids and solids. Hence some have designated the gaseous as the *perfect* state of matter. The hypothesis of Avogadro, suggested by certain uniformities in the behaviour of different gases, has the beautiful simplicity which distinguishes many fertile theories. The deductions from it have an harmonious consistency in both chemistry and physics. Dalton rejected this hypothesis partly on account of difficulties arising from his ideas concerning the atmospheres of caloric.

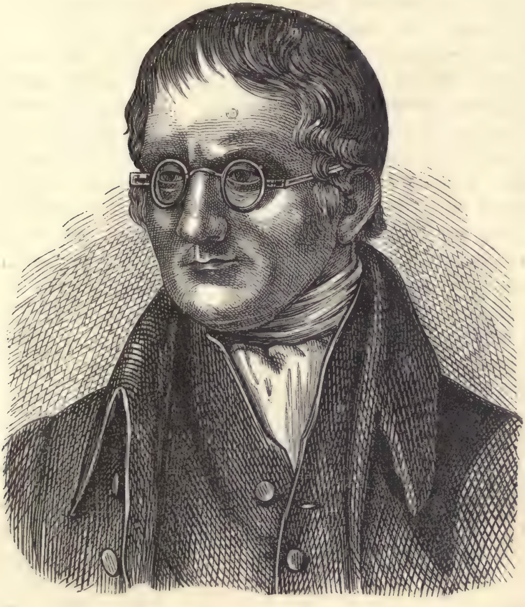


FIG. 306.—JOHN DALTON.

"In prosecuting my inquiries into the nature of elastic fluids, I soon perceived it was necessary, if possible, to ascertain whether the atoms or ultimate particles of the different gases are of the same size or volume, in like circumstances of temperature and pressure. By the size or volume of an ultimate particle I mean in this place the space it occupies in the state of a pure elastic fluid: in this sense the bulk of the particle signifies the bulk of the supposed impenetrable nucleus, together with that of the surrounding repulsive atmosphere of heat. At the time I formed the theory of mixed gases I had a confused idea, as many have I suppose at this time, that the particles of elastic fluids are all of one size; that a given volume of oxygenous gas contains just as many particles as the same volume of hydrogenous; or, if not, that we had no data from which the question could be solved. But from a train of reasoning . . . . I became convinced . . . . that the following may be adopted as a maxim till some reason appears to the contrary, namely:—Every particle of pure elastic fluid has the particles globular and all of a size; but no two species (of gases) agree in the size of their particles, the pressure and temperature being the same."

The difficulty which thus presented itself to Dalton was the case of the combination of one volume of one gas to one of another to produce a compound gas occupying two volumes. Each volume of the compound could be said to have at most only half the former number of particles. Let us suppose that one cubic inch of hydrogen gas contains 1,000 particles, and that one cubic inch of chlorine gas also contains 1,000 particles. By their union these gases would produce two cubic inches of hydrochloric acid gas. Therefore Dalton would say, each cubic inch of the compound gas can contain only 500 particles. But Avogadro's hypothesis is saved by the consideration that the *particles* of which it speaks may include several atoms. We have only to suppose that each *particle* of hydrogen contains *two atoms* of hydrogen, and that each particle of chlorine contains *two atoms* of chlorine. Then the combination will consist in the exchange of atoms among the particles. This may best be shown to the eye as in Fig. 307, where the squares represent the volumes of the gases, each containing four particles. After combination we see that each volume still contains four particles of the compound gas. This view is in perfect accordance with other chemical phenomena. In this way Gay-Lussac's laws (*see* page 530), which, it will be remembered, are not theories but facts, will be seen really to lend a powerful support to the atomic theory.

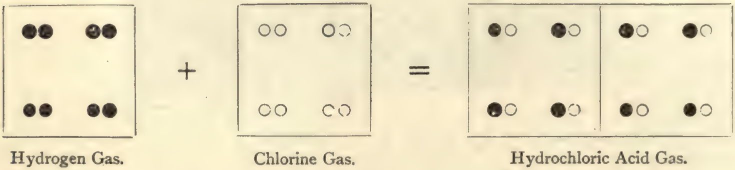


FIG. 307.

Dalton's life extended over more than three-quarters of a century, and during its course there appeared a chemist whose briefer span of existence did not deny him a more brilliant renown than even that of the famous philosopher of Manchester. Indeed, but few men of science attain a wider celebrity than fell to the lot of HUMPHRY DAVY (1778—1829). Davy was a native of Penzance, where, in the fifteenth year of his age, he became the apprentice of a surgeon and apothecary. His note-books, belonging to the period of his apprenticeship, show that he had entered upon a more comprehensive range of the deeper studies than persons of his age and position usually ever think of attempting; for the scheme of studies he drew up comprised not only all the branches of science appertaining to his profession, but also logic, physics, mathematics, history, rhetoric, geography, and theology. In languages he contemplated the acquisition of three dead languages, namely, Latin, Greek, and Hebrew, and three modern, namely, French, Italian, and Spanish. He also wrote poetry and essays on subjects relating to mental philosophy. The date at which he began the study of chemistry is known to coincide with the commencement of his nineteenth year. One of his first books was Lavoisier's "Elements of Chemistry," and the study excited a lively interest in his mind. He did not passively acquiesce in the doctrines of the French philosopher,

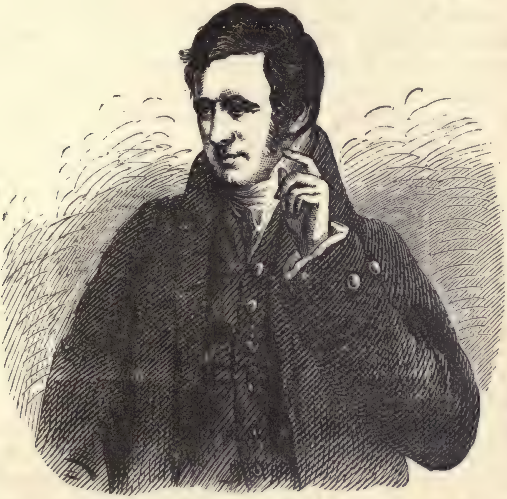


FIG. 308.—DAVY.

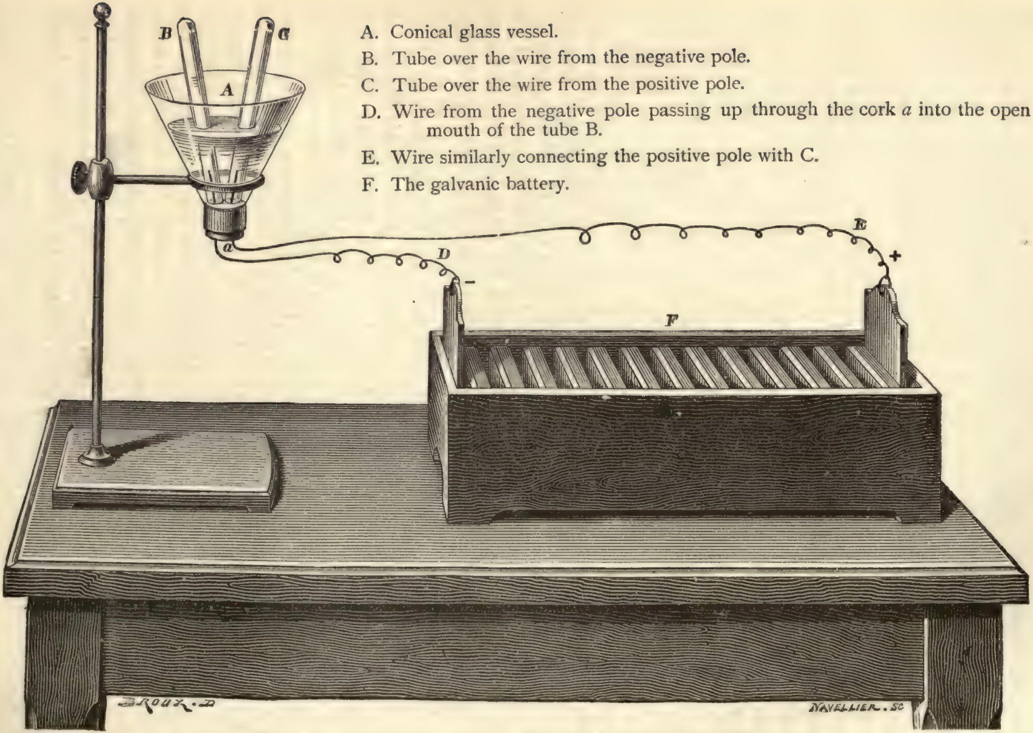
but speculated and conducted experiments on his own account. His apparatus consisted of wine-glasses, tea-cups, tobacco-pipes, and earthenware crucibles. So rapid was his progress in the new pursuit, that before he was twenty years of age he had elaborated certain theories of light and heat. Dr. Beddoes, a physician at Bristol, had shortly before established what he called the "Pneumatic Institution," where the properties of the different gases as regards their curative influences were to be studied and applied. Beddoes proposed that young Davy should undertake the superintendence of this institution, and the proposal was accepted. Davy's researches were speedily crowned with remarkable success. He discovered the peculiar property of *nitrous oxide* gas, which causes it to be ranked among the class of substances we now call *anæsthetics*, of which ether and chloroform are more recently discovered and well-known examples. In 1800 (*æt.* twenty-two) Davy published an account of his discoveries under the title of "Researches, Chemical and Philosophical, chiefly concerning Nitrous Oxide and its Respiration." This publication excited no little sensation among scientific men, and the genius of its author for experimental and philosophical research was fitly recognized by his appointment to the professorship of chemistry at the Royal Institution. In 1803, when he was scarcely twenty-five years of age, Davy was elected a Fellow of the Royal Society. Three years afterwards he delivered the Bakerian Lecture before the Society, and for several successive years delivered, in fulfilment of the same duty, discourses in which several of his most brilliant discoveries were announced. In 1812 Davy was knighted, in 1818 he was made a baronet, in 1820 he was unanimously elected President of the Royal Society, and held the office until 1827, when the state of his health compelled him to resign it. The following year he composed and published his "Salmonia," a treatise on fly-fishing. He was now seeking for some alleviation of his ailments in continental travel, but he continued almost to the last to occupy his mind with scientific and philosophical subjects. His last work was entitled "Consolations in Travel; or, The Last Days of a Philosopher." He died at Geneva on the 30th of May, 1829, at the comparatively early age of fifty-one.

Davy's researches on nitrous oxide and its physiological effects proved ultimately of great advantage, although not precisely in the way which was hoped for at the time. In Dr. Beddoes' institution it was intended that gases should be employed as curative agents, and on Davy's discovering by trial on himself the extraordinary effects of nitrous oxide, it seemed to some that the elixir of life had been found in the gaseous state. Davy's resolution and courage in first venturing to inspire a gas then reputed to be poisonous show his ardour for research; but though in this case they were justified by the event, some experiments of the like kind with other gases more than once placed his life in danger. The value to the world of these researches is that they made known the first *anæsthetic* that came into use in effecting surgical operations without pain. It is also the only quite safe one, and as such is still very much used in minor surgical operations, particularly by dentists. Davy's researches extended to the several combinations of oxygen and nitrogen, and were the means of giving chemists more exact knowledge of these compounds than had hitherto been obtained.

But the greatest of all Davy's chemical discoveries was that of the decomposition of the alkalies, soda and potash. This discovery was made in 1807 by help of a galvanic battery of about 250 couples. Davy first tried to decompose aqueous solutions of the alkalies, but the result was only the disengagement of hydrogen gas at one pole and oxygen at the other. It then occurred to him to pass the current through potash in a state of fusion. In this case he found some combustible substance was separated: he could not, however, obtain this substance until he had modified the form of the experiment. He then found at the negative pole small globules of a high metallic lustre. Some of these burnt with a bright flame almost as soon as they formed; but others remained, and became tarnished by a white film which formed on their surfaces. The lustrous matter was no other than the metallic base of potash, the existence of which had before been suspected. This metal received the name of *potassium*, and a few days after its discovery Davy succeeded in similarly isolating from soda the metal since called *sodium*.

Potassium and sodium are very light and very soft metals, in colour and lustre not unlike silver. Their surfaces are instantly tarnished by exposure to air, the oxygen of the air rapidly combining with the metal. A piece of either metal placed in contact with water decomposes the latter with evolution of hydrogen gas, and the result is a solution of the corresponding alkali. The hydrogen is inflamed by the heat attending the combination when the action takes place in the air. When a small piece of either metal is confined under water, a violent action ensues, and the hydrogen produced may be collected in appropriate vessels. In order to moderate the violence of the action in this experiment, Davy previously united the metal to mercury, and he found that the resulting *amalgam* permitted the hydrogen gas, liberated by the action of the potassium and sodium, to be conveniently collected and measured. The mercury is left pure and unchanged, as the only products are hydrogen, and a solution of the alkali, potash or soda, as the case may be.

The experiments of Lavoisier and others had proved that the constituents of water were oxygen and hydrogen. In these experiments the synthetical process only could be exhibited; that is to say, water was put together from its elements. But in 1800 Nicholson and Carlisle discovered a mode of effecting the inverse operation; that is



A. Conical glass vessel.
B. Tube over the wire from the negative pole.
C. Tube over the wire from the positive pole.
D. Wire from the negative pole passing up through the cork *a* into the open mouth of the tube B.
E. Wire similarly connecting the positive pole with C.
F. The galvanic battery.

FIG. 309.—DECOMPOSITION OF WATER BY ELECTRICITY.

to say, of splitting water up into its elements. This was accomplished by the then newly-found agency of the galvanic battery. The experiment is probably familiar to most of our readers, as in some such form as that shown in Fig. 309 it is frequently exhibited at popular lectures. A wire from each pole of the battery F passes at *a* through the bottom of the vessel A, which contains water acidulated with a little sulphuric acid. Over each a tube entirely filled with the same liquid is inverted at the commencement of the experiment. When the battery connections are made, a stream of minute bubbles of gas is seen to rise from each wire. The gas gradually displaces the water in the tubes, and the gas rising from the negative pole is found to be pure hydrogen, and that from the positive pole pure oxygen. The bulk of the hydrogen is always twice that of the oxygen. As Dumas' experiment for accurately determining by synthesis the ponderal composition of water is regarded by chemists as classical, a representation of the apparatus is given in Fig. 312, and this, with the description, will enable the reader to understand it without further details.

Now, in the experiments on the electric decomposition of water, made at the beginning of the present century, chemists were perplexed in finding that even when water supposed to be quite pure was exposed to the action of a *current* of electricity, the liquid about the negative pole always showed traces of alkali, while at the positive pole traces of acid appeared. As it had been proved in other ways that water contained only oxygen and hydrogen, the constant appearance of acid and alkali at the respective poles induced certain chemists to imagine that some substances other than oxygen and hydrogen might be present in water, while other chemists held that the acid and alkali were in some way produced by the action of the current. Davy tried to eliminate any effect due to the material of the vessel holding the water by using small cups of agate. Still acid and alkali appeared at the poles. He substituted conical vessels of pure gold; but still indications of acid and alkali were perceptible. Davy then placed his gold cups under the receiver of an air-pump. When the electric action was made thus to take place in a space free from every trace of nitrogen, the water in neither of the connected gold cups showed any trace of acid or of alkali. Thus was it conclusively proved that water chemically pure is decomposed by electricity into nothing but the two gases oxygen and hydrogen.

Davy proposed a theory of the relations of electricity to the elementary bodies very similar to that which was soon afterwards more clearly announced by Berzelius. The chemical origin of the electric force of the galvanic battery was another of Davy's views.

By the agency of electricity in the form of *currents* from galvanic batteries of many pairs of plates, Davy was able, in 1807, to decompose several of the "*earths*," so as to obtain their metallic elements. In this way he separated calcium, the metal of lime; barium, the metal of barytes; magnesium, the metal of magnesia, and others. It was in order to enable Davy to pursue experiments which were yielding results of so novel and surprising a kind, that the members of the Royal Institution had a galvanic battery constructed on a scale of then unprecedented magnitude, and with this some of his later results were obtained.

Another investigation of Davy's of no little importance to chemistry related to the substance we now call *chlorine*—the name proposed for it by Davy. Chlorine was discovered by Scheele (page 352), and was some years afterwards carefully examined by Berthollet, who concluded it to be a compound of muriatic acid (page 379) and oxygen. This view was accepted, and for twenty years chlorine was known as "*oxy-muriatic acid gas.*" Davy in his turn submitted "*oxy-muriatic acid*" to a very searching examination, in which he was led to the discovery of new compounds. He declared that the gas was an elementary (or undecompounded) substance, combining with substances to form bodies analogous to the oxides. These substances are now called *chlorides*, as, for instance, in the case of common table salt, which chemists designate *chloride of sodium*, or *sodium chloride.*

Soon after Davy's researches on chlorine he attempted without success to isolate fluorine. But at this time an accidental discovery made by M. Courtois, a manufacturer of saltpetre at Paris, added to the chemist's inventory a new substance belonging to the same small peculiar group of elements as chlorine and fluorine. Courtois found that the metallic vessels he used in preparing soda from seaweeds became much corroded. He investigated the cause, and found it in a substance separable without much difficulty from the ashes, a substance which in the solid form resembled plumbago in appearance, and which, when heated, gave rise to a vapour of a magnificent violet colour. This was *iodine*, for such was the name Davy gave it when he, like other eminent chemists, had examined it. His conclusion was decided that iodine was an undecomposable substance similar in its general chemical behaviour to chlorine and fluorine.

Many pages might yet be filled with the mere statements of Davy's chemical investigations and discoveries. Perhaps the selection we have made will suffice to convey to the general reader some notion of the extension chemistry owes to Davy. There is, however, one discovery—or, rather, invention—of Davy's which is so familiarly identified with his name as to deserve mention here. If it has not contributed to the structure of chemical science, it has contributed directly to human welfare. It need hardly be said we here refer to the *Safety Lamp.* The "Researches on Flame" which resulted in the invention of the "Davy lamp," constitute an elegant model of scientific investigation. We may see in these papers how Davy was led to adopt the now well-known cylinder of wire gauze, in which he enclosed an ordinary oil-lamp, as represented at Fig. 311. A piece of common wire gauze held in a candle or gas-flame, exhibits the fundamental principle of the Davy lamp. The combustible gases can be ignited on one side of the gauze, which does not permit the flame to pass through. The original papers show that this is owing to the power of the wire to rapidly abstract heat from the ignited gases. The fact of claims for prior or independent invention of the safety lamp having been advanced in favour of Dr. Clanny and in favour of George Stephenson, the eminent engineer, does not detract from the merit of Davy's scientific treatment and successful solution of the problem. The safety lamp is a striking instance of science bearing "fruit" in the Baconian sense.

In a preface to a separate publication (1818) of his researches relating to this subject, we find Davy expressing indebtedness "to Mr. Michael Faraday for much able assistance in the prosecution of my experiments." Some notice of Faraday's scientific career has been given in the chapter relating to electricity; but his discoveries in chemistry were neither few nor unimportant, and some of these will be noticed further on.

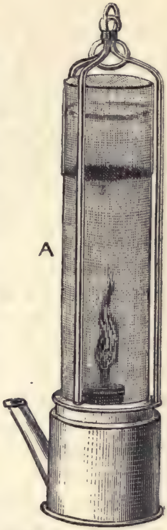


FIG. 311.—DAVY'S SAFETY LAMP.

The atomic theory of Dalton, like perhaps every other theory when first propounded, met with some opposition. The French chemist Berthollet, to whom the science is indebted for many valuable researches, called in question the fact of definite proportions in combinations. Berthollet's own investigations into "chemical affinity" are held to be classical. They were conducted to discover the operation of a special force called "chemical affinity," which was supposed to govern the combinations of bodies, and they brought to light a number of valuable truths. Berthollet considered that certain forces, such as cohesion and elasticity, opposing the action of affinity, might in certain cases more or less neutralize each other, so as to leave the action of affinity unimpeded. Combination of elements might then take place in any proportions, which would be determined only by the masses of the substances taking part in the action. Dalton's law of combination in unvariable proportions is of course inconsistent with these views, and therefore Dalton's facts and theory were opposed by all the weight of Berthollet's authority. Dalton's views, however, were with equal vigour supported by another Frenchman, PROUST (17 . .— . . . .), who answered Berthollet's cavils by bringing forward more exact analyses of oxides and sulphides in confirmation of the law. The dispute lasted from 1801 to 1808, and resulted in the establishment of the Law of Definite Proportions.

A new proof of the validity of the laws of definite proportions was in the meantime being discovered by the genius of a young philosopher, who, like many other brilliant men of science of his country at this epoch, had been trained at the Ecole Polytechnique. This was JOSEPH LOUIS GAY-LUSSAC, who was born at St. Léonard, 1778, and died 1850. The facts which exact experiments enabled him to announce not only confirmed the atomic doctrine, but gave a new extension to chemistry. Indeed, it is not too much to say that Gay-Lussac's laws, interpreted by aid of a certain hypothesis (Avogadro's), are the foundation of the whole fabric of the theoretical chemistry of the present day.

Before Gay-Lussac various experimenters had sought to determine the proportion *by measure* in which oxygen and hydrogen gases combine to form water. The results had been somewhat discrepant. The volumes of hydrogen which combine with 100 volumes of oxygen were variously given as 190, 198, 205, etc. Gay-Lussac, working in conjunction with Humboldt, found that 100 volumes of oxygen required *exactly* 200 volumes of hydrogen; that is, the gases united in the exact proportion of 1 to 2. This result, obtained in 1805, was sufficiently striking to induce Gay-Lussac to carefully examine other cases of gaseous combination. He found that hydrogen unites with chlorine 1 volume to 1 volume; that to form nitrogen protoxide 2 volumes of nitrogen gas are united to 1 of oxygen; and so on. In every case examined the gases were combined in exact integral volumes having a simple ratio to each other. Hence the law:—

"*There is a simple ratio between the volumes of two gases which enter into combination.*"

Gay-Lussac also found that the volume of the product formed by the combination of gases always had itself, when in the gaseous state, a simple ratio to the volume of its constituents. At the temperature of 212° F., 1 volume of oxygen would unite with 2 volumes of hydrogen, and the product would be exactly 2 volumes of steam, *i.e.*, of the gas of water. One volume of hydrogen united to 1 of chlorine produces exactly 2 volumes of hydrochloric acid gas, and so on in other cases. Hence the second of Gay-Lussac's laws:—

"*There is a simple relation between the volumes of gas which enter into combination and the volume of the product taken in the gaseous state.*"

It is a curious circumstance that Gay-Lussac himself should suppose as he did that these laws were reconcilable with the opinions of Berthollet as to the variability of combining proportions. And, again, it is equally curious that Dalton, whose atomic theory these facts so strongly supported, should have doubted of their correctness. Gay-Lussac's laws, taken in connection with Dalton's theory, indeed, placed in the hands of chemists a means of controlling their determinations of the atomic weights. Thus, if according to the atomic theory 1 atom of hydrogen combines with 1 atom of chlorine, and if equal volumes of chlorine and hydrogen do as a matter of fact unite, it follows that a volume of hydrogen contains the same number of atoms of hydrogen as an equal volume of chlorine, and hence the weights of hydrogen and chlorine atoms will be proportional to the weights of equal volumes of the gases. The specific gravities (or densities) of the gases would therefore have indicated how many times the atom of chlorine exceeded in weight the atom of hydrogen; but these inferences were not made at the time. Yet shortly before Gay-Lussac published his discoveries, an Italian chemist named AMADEO AVOGADRO (17 . .—18 . .) suggested, as a means of accounting for certain properties of gases, an hypothesis which involved the relation in question. The properties he sought to explain were, that all gases expand in the same degree by heat, and follow one law (Boyle's, page 231) of relation between volume and pressure. Avogadro's supposition was that in the same volume of different gases, whether simple or compound, the same number of particles are contained, the gases being taken, of course, under the same circumstances of pressure and temperature. This idea appears to have attracted no attention at the time, and though it was revived by Ampère in 1814, it was not until many years afterwards that Avogadro's hypothesis met with the recognition it deserved. In applying the hypothesis, not a little confusion has arisen from confounding the *particles* or *molecules* of a body with its ultimate atoms. Every molecule of any given elementary gas may contain two particles, or three, or four, or any greater number of atoms, or it may consist of but a single atom, without affecting the statement of Avogadro's proposition. The same is true of the molecules of a compound gas, except only that here each molecule must necessarily comprise at least two atoms.

The scientific reputation of Sweden has been ably sustained in the present century by the labours of the celebrated chemist, JACOB BERZELIUS, who was born at Wafnersunda in West Gothland, 1779, and died at Stockholm in 1848. His career was marked by an industry truly astonishing, and he worked to the last. He was rewarded for his labours with every academic and public honour which could be bestowed upon a man of science, and he obtained also wealth and position, which less frequently fall to a philosopher's lot. The chemical work of Berzelius is distinguished rather by its extent and accuracy than by depth or brilliancy of theoretical conceptions. Nevertheless, the discoveries he made were neither few nor unimportant. It was he who first imported exactness into the methods of analysis, and fitted it to become the instrument of fresh discoveries. It was he who first made known several of the rarer elements, and separated silicon from its oxide. He contributed perhaps still more to the progress of the science when he placed in the hands of chemists an instrument so effective as the *Symbolic Notation*, which is to the chemist what the symbols of algebra are to the mathematician.

The notation of Berzelius was, like all other inventions and discoveries, an evolution. The old alchemists used symbols, some of which may be seen on the bottles in the chemists' shops to this day. Dalton more rationally indicated his non-metallic atoms by distinguishing marks enclosed in little circles and the atoms of the metals by placing the initial letter of each within the circles. This mode of representation, excellent for simple combinations, was cumbrous and unwieldy when many atoms had to be indicated. Berzelius dispensed with the circles, and made the initial letter of the Latin name of each element the symbol of the atom. H represented an atom of hydrogen, O an atom of oxygen, S an atom of sulphur, Sb an atom of antimony (*stibium*), K an atom of potassium (*kalium*). A compound was indicated by the symbols of its constituents placed side by side, thus: KO stood for oxide of potassium, and when several atoms of the same element were to be grouped together, Berzelius, instead of repeating the symbol, wrote a small figure after it: thus, instead of SOOO for sulphuric acid, he wrote $SO^3$ or $SO_3$. Another device was the employment of dots to express *oxygen* atoms in combination: thus, $\dddot{S}$.

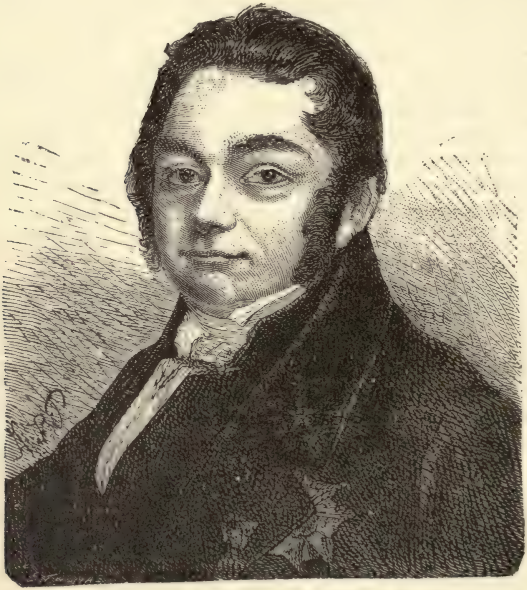


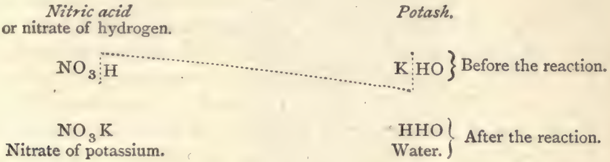
FIG. 312.—JACOB BERZELIUS.

Berzelius has stamped the nomenclature of chemistry with the impress of another idea, namely, the electro-chemical theory. The decomposition of water and other compounds by the galvanic battery need not be described in this place. Suffice it to say, as an example, that when a hydrochloric acid is made to form part of the galvanic circuit, it is decomposed, chlorine being liberated from the positive pole, and hydrogen from the negative pole. Similarly with other binary compounds,—one constituent appears at one pole, the other at the other pole. If sulphate of soda is the subject of experiment, sulphuric acid separates round the positive pole, while soda collects about the negative. Berzelius adopted the following hypothesis: he conceived that every atom had two points or poles, at one of which a quantity of negative electricity might collect, at the other a quantity of positive; and these quantities not necessarily being equivalent, but one or the other kind predominating in certain atoms might impart to them distinctive properties. Hence he divided bodies into electro-negative (*e.g.*, chlorine) and electro-positive (*e.g.*, hydrogen) by the supposed predominance of one or the other electricity in the atom. The classification by these names has, as a matter of convenience, been more or less retained to the present day; but the theory itself has lost the importance formerly attached to it.

A most valuable part of the scientific work of Berzelius was the preparation of a table of the *equivalents* of the elementary bodies, far more accurate than that which Wollaston had published in 1813. The numbers in Wollaston's table of equivalents (a term first introduced by him) were more correct than the *atomic weights* given by Dalton. But Berzelius extended the table, and by more exact methods of analysis than had before been practised, he arrived at numbers very closely representing those which still more refined researches have since given. In fixing the figures which express the combining proportions, any standard whatever that is otherwise convenient may be selected. Thus Wollaston called oxygen 10, while Berzelius adopted 100 as the figure for oxygen. Wollaston's number for hydrogen was 1·25, because he considered that water was composed of 1 equivalent of oxygen + 1 equivalent of hydrogen. Berzelius's number for hydrogen was 6·25, or only half the proportionate value of Wollaston's. The reason was the Swedish chemist's desire to make his system accord with Gay-Lussac's laws regarding water as made up of 1 atom of oxygen united with 2 atoms of hydrogen. The atomic weight of hydrogen is to the atomic weight of oxygen in Berzelius's system as 6·25 : 100, for this is the ratio of the specific gravities of the two gases. But he considered that hydrogen *always* entered into combinations in double atoms, and thus $H_2 = 12{\cdot}5$ is the *equivalent* of hydrogen. Nitrogen, chlorine, bromine, and iodine were also considered by Berzelius to have *equivalents* consisting of *two atoms*. In Dalton's system the "*atomic weights*" were really the proportionate weights in which bodies combine; in that of Berzelius the proportionate combining weights are called *equivalents*, and the relative weights of equal volumes of the elementary gases are their *atomic weights.* We see, then, that Berzelius sought to reconcile the law discovered by Gay-Lussac with the atoms of Dalton and the equivalents of Wollaston. He did this by supposing that hydrogen, nitrogen, and chlorine enter into combination only in pairs of atoms.

As it would be beyond the scope of this work to trace in detail the successive modifications of the atomic theory, which have been made in order to embrace new classes of facts as they have come into view, it may be proper to notice here, for the sake of a connected survey, the developments of Dalton's theory, which are of the greatest importance to modern chemistry, and at the same time admit of popular treatment.

Davy and Berzelius assigned to the atoms certain electrical properties, and proposed to explain chemical actions by the force of electrical attractions and repulsions. The decompositions which the then newly-invented galvanic battery effected were in many respects very striking, but in none more so than in the decided manner in which the results appeared to confirm Lavoisier's ideas concerning the constitution of salts. These were, as the reader may remember, that salts are the result of the direct combination of acids and bases (page 371). It was found that the galvanic current always decomposed a salt into an acid and an alkali. Sulphate of soda being the subject of experiment, sulphuric acid invariably appeared at the positive pole, while the base or alkaline soda showed itself at the negative pole. Similarly the neutral substance saltpetre was resolved into nitric acid and potash. In the symbolic notation of Berzelius, saltpetre, *i.e.*, nitrate of potash, was thus represented $N_2O_5+KO$. The first part of the symbol represents a molecule of the substance then called nitric acid; the second part shows the oxide of potassium. The notion of salts as thus formed by the *simple conjunction* of an acid and a base reigned supreme for many years. In process of time other views were adopted. Davy suggested that *hydrogen* plays an essential part in the constitution of acids, as in an acid of iodine which he examined, in which iodine and hydrogen were the only constituents. The researches and views of a group of distinguished French chemists, especially DUMAS (*born* 1800), LAURENT (1807—18..), and GERHARDT (1816—18..), all men of admirable genius, recast the theory of chemical constitution into new forms. The numbers representing the atomic weights were in certain cases changed from those used by Berzelius. It may be necessary to remind the reader that those numbers are *inferred* from the combining proportions, and where there are several of these, other circumstances determine the choice. The French chemists also insisted upon regarding the molecules of compound bodies as formed of a system of atoms not necessarily arranged in the two groups of acid and base. According to these newer views, by far the greater number of chemical actions resulting in production of salts and other compound bodies consist not in the simple conjunction of groups of atoms, but in an *exchange* between already existing larger groups. This will be understood by comparing with Berzelius's symbol for saltpetre, the following symbolic representation of what takes place in the contact of nitric acid with potash.



*Nitric acid* or nitrate of hydrogen. — *Potash.*

$NO_3 \vdots H$ ⋯⋯ $K \vdots HO$ } Before the reaction.

$NO_3K$ Nitrate of potassium. — $HHO$ Water. } After the reaction.

By the dotted line we here indicate that the hydrogen and the potassium atoms change places, thus forming the products shown in the second line of symbols, viz., nitrate of potassium and water. The elimination of water in such reactions is a fact which was long overlooked.

Another development of the atomic theory is the doctrine of "*compound radicals*;" that is, groups of atoms are conceived as performing all the functions of single atoms by entering into and leaving combinations. Thus a group formed by the union of one atom of carbon with one of nitrogen will perform the function of a single atom of chlorine, forming a parallel series of compounds. The compound radical (CN) is named *cyanogen*, and as we have hydrochloric acid, HCl; potassium chloride, KCl, etc., etc., so we have hydro-cyanic acid H(CN); potassium cyanide, K(CN), etc., etc. The number of compound radicals now recognized is extremely great; indeed, there is no limit to the number that may be supposed.

About the end of the seventeenth century we find some chemists considering apart the substances derived from the mineral, animal, and vegetable kingdoms respectively. About the beginning of the present century it became customary to divide the study of chemistry into two great divisions, called respectively "organic" and "inorganic" chemistry. The provinces assigned to these branches of the science will be understood (from Berzelius' definition of organic chemistry, 1827) as the chemistry of animal and vegetable bodies, or of those substances which are found under the influence of the vital forces. It was supposed that an impassable barrier existed between the mineral and organic substances in this respect: that whereas the former could be formed from their ultimate elements by the chemist in his laboratory, the latter, the organic substances, could arise only in the living bodies of plants and animals, where the vital actions were supposed to control the chemical tendencies in some peculiar manner. Lavoisier had announced that all organic substances contain carbon as one of their constituents, and subsequent analyses completely confirmed this. In 1828, however, a German chemist named Wöhler discovered a method by which a certain organic product called *urea* could be synthetically produced from its elements in the laboratory. This fact was sufficient of itself to break down the barrier which had been supposed to separate the two provinces of chemistry. But it was not until many years afterwards that other facts of this kind were discovered. Some progress was made in 1845 and 1846, and in 1850 Professor Frankland (*born* 1825) found a means of combining certain alcohol radicals with zinc, and this again showed the continuity of the two arbitrarily separated departments of chemistry. Before this a distinction had been made which superseded the older idea of the supposed necessity of the intervention of peculiar vital forces in the production of organic compounds. Organic chemistry was defined as embracing the study of *all compounds of carbon.* There were obvious advantages in studying these compounds apart, on account of their great number and of certain peculiarities in the chemical behaviour of carbon. The distinction, however, is now held to be quite arbitrary, although convenient. After Frankland's discovery of the organo-metallic compounds, methods of forming by artificial synthesis many complex organic compounds were made known by the labours of several distinguished chemists. From what has been already accomplished, it is reasonable to believe that in the progress of the science it may come to pass that all organic compounds will be obtainable by the processes of the laboratory. A striking instance of what can be effected by such processes may be cited in the discovery in 1869 of a mode of artificially preparing *alizarine*, the tinctorial substance in madder—the well-known Turkey red dye. Artificial Turkey red is now manufactured in large quantities from *anthracene*, a compound of hydrogen and carbon obtained in the distillation of coal-tar, and thus the necessity for cultivating the madder-plant is obviated.

The study of the carbon compounds was the means of introducing many new theoretical ideas into chemistry. An account of these theories in connection with the facts they were invented to represent would carry us far beyond our limits, and without a discussion of the facts they would be empty and unintelligible to the general reader. We shall not attempt even a statement of modern chemical theory, but notice only one or two points merely as an illustration. Hence we pass over the conflict of "nucleus," "type," "substitution," and other theories, to which the discoveries of organic chemistry especially gave rise. But it will be incumbent upon us to refer to the labours of the great master of organic chemistry—Justus von Liebig.

Justus von Liebig, born at Darmstadt in 1803, died 1873, was one of the greatest scientific men of his age. He did for organic chemistry that which Lavoisier did for inorganic: he may be said to

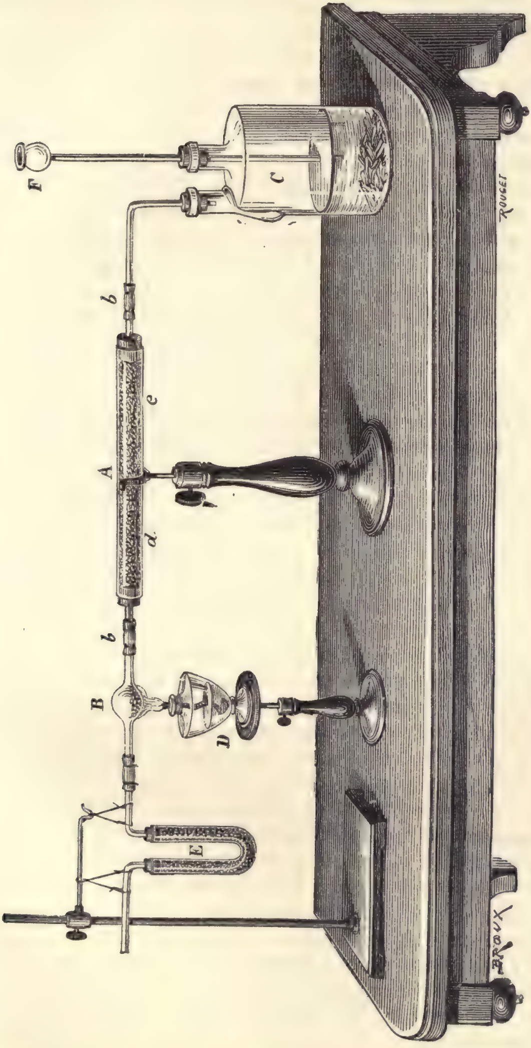


FIG. 312.—DUMAS' SYNTHESIS OF WATER.

C is a bottle in which hydrogen gas is generated from fragments of zinc previously introduced, and diluted sulphuric acid poured in by the funnel-tube F as required. The gas passes on through the tube *e d*, which is filled with a substance capable of making it thoroughly dry; it then comes into contact with oxide of copper contained in the bulb B and heated by the spirit lamp D. The oxygen leaves the copper and combines with the hydrogen, forming water, which the stream of gas carries on into the tube E, containing chloride of calcium for absorbing the water or aqueous vapour. The loss of weight of B gives the quantity of oxygen, the gain of weight of E the corresponding quantity of water.

have created the science. His influence on the progress of chemistry and the allied branches of science was exerted in several modes. He was the first to establish a school of experimental chemistry, his laboratory at Giessen being the prototype of the educational laboratories of the present day, and many of our most eminent chemists were his pupils and assistants. He made many important discoveries, adding in fact a new domain to the province of chemistry. He invented new processes and new apparatus; he expounded lucid theories of combination; his investigations made possible new manufactures; he extended the resources of the agriculturist; he carried chemistry into the field of physiology.

Liebig kept several terms as a student at the University of Bonn, but obtained his degree of Ph.D. at Erlangen when he was nineteen years of age. Being desirous of pursuing the experimental study of chemistry at Paris, which was then the great centre of chemical science, he was provided by the reigning Grand Duke of Hesse-Darmstadt with the means for several years' residence in the French capital. A number of chemists of high genius were then cultivating this science with brilliant success on the banks of the Seine. There among others were Gay-Lussac, Thenard, Chevreul, and Dulong. By the influence of his countryman, Alexander von Humboldt, young Liebig obtained admittance into the laboratories of Gay-Lussac, Dulong, and Thenard. In 1823 Liebig published his first paper, relating to certain fulminating compounds, and in the following year a paper appeared on the same subject under the joint name of Liebig and Gay-Lussac. In the same year Liebig was, on Humboldt's recommendation, appointed Professor of Chemistry in the little University of Giessen, where, from a very insignificant foundation, he raised the most famous school of practical chemistry in Germany.

The researches on fulminates which Liebig instituted in conjunction with Gay-Lussac proved that these compounds had the same per-centage composition as another class of substances called *cyanates*, without, however, being identical with them. An example of the way in which this is theoretically possible will be seen in a list of some *polymeric* hydro-carbons on a subsequent page. This led Liebig to the study of the cyanates and the allied compounds, the *cyanides*. The starting-point of the group is a well-known substance of extensive use in the arts, called the yellow prussiate of potash. Liebig's researches explained and improved the mode of preparing this substance, and he further devised a simple and cheap process for obtaining cyanide of potassium from the yellow prussiate. This method is now followed out on the large scale, for cyanide of potassium is very extensively used in photography and in electro-plating. These cyanogen compounds formed the starting-point of a long series of theoretically interesting new compounds, with which Liebig endowed organic chemistry. Another very interesting group of substances studied by Liebig in conjunction with Wöhler was the *benzoic* compounds, which form the pivots, as it were, upon which turn a vast series of compounds. Liebig investigated the nature of many organic acids with signal success. The derivatives of alcohol formed another subject of inquiry; but as the statement of results and methods of inquiry in such investigations would be intelligible only to those specially versed in the study of chemistry, we must refrain from continuing the long list of Liebig's studies. Two of the many new substances prepared by him may be named, however, because they have been heard of by everybody as the anæsthetics most largely used by medical men at the present day. These are *chloral*, with its hydrate, and *chloroform*. Liebig did not at the time know that these substances were destined to become potent "for the relief of man's estate," as we know them now. It was not until 1847, or fifteen years after its discovery, that Sir James Simpson, the eminent surgeon of Edinburgh, used chloroform for the first time as an anæsthetic. Chloral was not applied in medicine until a still later period, when thirty-five years had elapsed since its discovery. Liebig adopted and strenuously advocated the theory of "compound radicals," which was at first accepted also by Berzelius; but when the latter found that some of Liebig's compound radicals contained oxygen, he opposed the radical theory as incompatible with his own electro-chemical theory. A long controversy was then begun between Liebig and Berzelius. One of the radicals, the existence of which was suggested by Liebig, was *ethyl*, which he conceived entered into the composition of alcohol and of ether. His ideas have won their way, and modern chemists have universally accepted the theory of *ethyl* and similar radicals.

The investigations of Liebig on physiological chemistry, especially his researches on the nutrition of plants and animals, gave rise to the modern scientific system of cropping the soil, by repairing artificially the drawn-off phosphates, supplying nitrogenous materials, and so on. The now very extensive industry of the artificial manure manufacture is of Liebig's creation; and the like may be said of whatever scientific principles guide the feeder of cattle in selecting the food for his stock. Those conversant with agriculture best know the vast impulse it has received in recent times by the scientific practice which was first advocated and made possible by the Giessen Professor of Chemistry.

No science ever received from a single device a greater impulse than did organic chemistry by Liebig's method of analysing organic substances into their ultimate elements. The substance to be analysed is mixed with oxide of copper in a tube of hard glass, which is heated by charcoal in a convenient apparatus. The hydrogen of the organic substance is converted into water, and as the products of the combination making their exit from the tube first pass through a tube containing a substance capable of absorbing all the moisture, the increase of weight received by the tube is the weight of water produced in the experiment. The carbonic acid gas which has been formed by the union of the carbon of the organic substance with as much oxygen as it can take up, passes on, and is absorbed in a second tube containing a solution of potash. The tube designed by Liebig for this purpose, simple piece of apparatus though it be, shows the hand of a master. It admirably answers the conditions of exposing of a large surface of liquid to the gas, of agitating of the whole as each bubble of gas passes, and of guarding against loss of the liquid from the agitation caused by the passage of gas in either direction. Liebig's combustion apparatus for determining the quantities of carbon and hydrogen in organic substances is also distinguished by great simplicity in its construction and management, and to the facilities it has afforded, organic chemistry is to this day indebted for the vast development which a few decades have sufficed to bring about. In addition to the creation of organic analysis, chemists have also to thank Liebig for the invention of the general plan of the regular analysis of mineral substances which is followed in our laboratories. Liebig's literary labours were not confined to his purely scientific papers, numerous as these are. He (with his friends Wöhler and Kopp as coadjutors in the later years) regularly issued a record of the progress of chemical discovery. Of this publication, called "Liebig's Annals of Chemistry," one hundred and sixty-five volumes appeared between 1832 and 1873, the time of Liebig's death. He was joint author of a large Dictionary of Chemistry and of the "Handbook of Organic Chemistry," in which that science for the first time exhibited an orderly and comprehensive outline. Then there are his several very important works on agricultural and physiological chemistry: the "Familiar Letters on Chemistry," of which several editions have appeared in England, may probably be known to the reader. These letters were originally published in a German newspaper, and were intended to impart to general readers from time to time such results of Liebig's inquiries or meditations on experimental science, agriculture, physiology, or general topics, as might be available.

Liebig's character was specially distinguished by his eagerness for truth before all things; for this he would without reluctance abandon cherished theories, and cordially acknowledge his mistakes when convinced that his former views were untenable. He esteemed the actual results of experiments more highly than the most plausible speculation, and although his own speculations were often most happy, he would rely conclusively upon no other foundation than experiment. His distinguished pupil and friend, Professor Hofmann of Berlin, says of him, "During his long contest with Gerhardt, Liebig never loses the sure foundation of experiment. He has not to retract a single statement as to facts, though he does not deny having committed mistakes in interpretation; but these he happily compares to the broken crockery found in the corners of the best-regulated houses in which a good deal of work has been going on." In kindliness of character and in simplicity of life Liebig appears to have resembled his great English contemporary, Faraday.

Faraday, whose career and whose discoveries in another department of science have been presented in a previous chapter, began as a chemist. He was first engaged as assistant to Davy in the laboratory of the Royal Institution in 1813, and afterwards in the same place and in the same capacity he was again engaged with Professor Brande. Faraday's purely chemical discoveries bear no comparison with the number and importance of those which give him an imperishable fame in the history of electricity. One principle, however, of no insignificant general interest in chemical science, he did establish. It had been always assumed, before his experiments were made, that a vapour was something different in its nature from a gas. Faraday showed that the difference is merely a question of temperature and pressure, and in his skilful hand, many supposed permanent gases were reduced to the liquid, and some even to the solid, form. It will afford an illustration of the beautiful simplicity and rare skill which characterize the appliances by which Faraday was accustomed to obtain his results, if we explain his method of operation in some of his experiments on the condensation of gases. His apparatus consisted of a tube of very strong glass, into which the materials for generating the gas were introduced, so as to occupy a small space at one end, as shown at A, Fig. 313. The other end of the tube C was then sealed up, and the tube was then bent in the middle B by the application of heat. The portion of the tube B C was immersed in a freezing mixture while heat was applied at the extremity A, in order to liberate the gas. The gas, under the combined effect of cold and the pressure exercised by itself, was in most cases condensed into a liquid, occupying the end C of the tube. The means of measuring the amount of pressure within the tube was equally simple. A globule of mercury was introduced into a tapering capillary glass tube *d e*, and the spaces it occupied successively in different parts of the tube were marked thereon in black varnish; in other words, the tube, narrow as it was, was thus divided into spaces of equal capacity. The globule was left in the wider end of the tube, and the narrower end *e* was sealed up. The compression of the included air, as indicated by the position of the quicksilver, thus became, in accordance with Boyle's law (p. 231), the measure of the pressure on the included gas. Faraday succeeded in liquefying, and in some cases solidifying, all the supposed permanent gases except six, namely, hydrogen, oxygen, nitrogen, carbonic oxide, nitric oxide, and marsh gas, which long resisted all the efforts made to reduce them to the liquid state; but in quite recent times M. Cailletet has succeeded in liquefying most if not all of these. The condition, liquid or gaseous, therefore, in which a substance exists, is known to be determined by the circumstances of temperature and pressure.

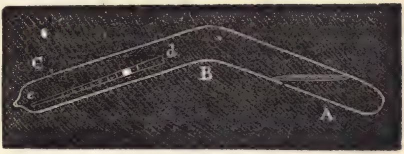


FIG. 313.

In prosecuting these experiments Faraday in 1825 condensed small quantities of certain vapours from gas prepared by the distillation of oil. These proved on examination to be compounds of hydrogen and carbon, and among them was one which long afterwards became of much scientific interest, and the starting-point of large chemical industries. This was *benzine* or *benzol*, the parent of all the now well-known aniline colours, to which reference will again be made in the sequel.

Here we may refer to the experiments of Dr. Andrews of Belfast (1862), who was the first to demonstrate the continuity of the liquid and solid states. When carbonic acid gas was liquefied in a tube by pressure, and the tube was gradually raised to the temperature of 88° F., the demarcation between the liquid below and gas above became fainter, and at length disappeared, the whole space being then filled with a uniform fluid, which on a slight diminution of either temperature or pressure exhibited a peculiar appearance of flickering *striæ*. At the temperature of 88° F. carbonic acid was therefore capable of

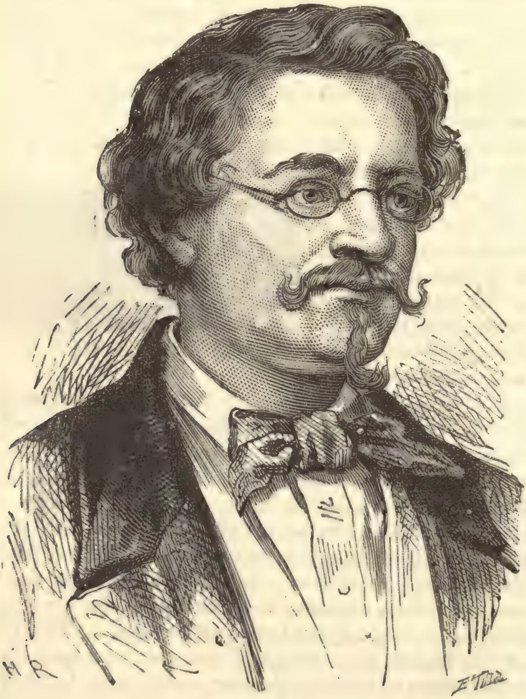


Fig. 314.—Hofmann.

existing under pressure in a state in which it was neither a liquid nor a gas, but something intermediate. Dr. Andrews found that other substances assumed the same condition, but each at a particular temperature.

The name of the eminent pupil of Liebig's, A. W. Hofmann, now Professor of Chemistry in the University of Berlin, whose words have been quoted in a foregoing page, furnishes us with the opportunity of illustrating the advantageous effects of scientific research on practical industry, though this theme is well-nigh worn threadbare; and it has become so much a truism, that successful manufacturers and traders now vie with each other in encouraging "scientific research," presumably for the prospect of having new fields for commercial operation, rather than for any abstract love of science as such. Liebig, in a letter to Faraday, says: "What struck me most in England was the perception that only those works which have a practical tendency awake attention and command respect; while the purely scientific works, which possess far greater merit, are almost unknown,—and yet the latter are the proper and true source from which the others flow. Practice alone can never lead to the discovery of a truth or a principle. In Germany it is quite the contrary. Here, in the eyes of scientific men, no value, or at least but a trifling one, is placed on the practical results. The enrichment of science is alone considered worthy of attention. I do not mean to say that this is better: for both nations the golden medium would certainly be a real good fortune." It may, therefore, be not improbable that chemical symbols and chemical theories may have greater interest for some readers, if it is shown that by their aid discoveries creating entirely new branches of industry have been effected.

Faraday, engaged in a purely scientific research, discovered benzol in 1825. In 1826 Unverdorben, by distilling indigo, obtained a substance he called *crystalline*, because its compounds readily crystallized. In 1834 Runge found in coal-tar a substance he called *cyanol*, because in certain reactions it yielded blue colorations. In 1841 Fritzsche, by distilling indigo with potash, obtained a substance he called *aniline*, from *anil*, the Portuguese name for indigo. About the same time Zinin obtained from a derivative of benzol a liquid to which he gave the name of *benzidam*. In the meantime, since Faraday's discovery in 1825, something more had been found out about benzol. Mitscherlich in 1834 had obtained it by distilling *benzoic* acid with lime—hence its present name. In 1843 Hofmann showed that *crystalline*, *cyanol*, *aniline*, and *benzidam* are one and the same substance. A little later he found that from coal-tar a considerable quantity of *benzol* may be obtained. It followed, therefore, that coal-tar could readily be made to furnish aniline. Aniline is a colourless oily-looking liquid; but by treating aniline with the proper chemical reagents, it is made to furnish a series of colouring matters which are marvellous for their number and the variety and intensity of their hues. Many of the derivatives of aniline were discovered and investigated by Hofmann and other chemists, and their chemical relations determined simply as a matter of science and without reference to practical applications. All these researches were directed by the light of theoretical views. Benzol was found to constitute the nucleus, or pivot, of an immense series of compounds united in relations which were embodied in a very ingenious and comprehensive theoretical view, for which science is indebted to Professor KEKULÉ of Bonn. A large number of interesting and useful substances belong to this extensive series, which has been named the *aromatic series* on account of the number of fragrant principles contained in it. In 1856 Mr. W. H. Perkin, of London, was endeavouring to artificially prepare quinine by the oxidation of aniline. He did not succeed in his aim, but he obtained the violet substance now known as "*mauve*." Its intense tinctorial power, combined with the beauty of its tint, struck Mr. Perkin, and he at once proceeded to obtain a patent with a view to the introduction of the new substance into commerce. He was successful in this, and "mauve" began to be manufactured on a large scale. Hofmann's researches into the aniline derivatives had pointed out their chemical relations, and it was now also seen that other colouring principles might be derived from aniline and made available as dyes. Hence the famous series of the coal-tar colours, mauve, magenta, etc., and all their congeners. Coal-tar, from having been a worse than useless by-product in gas manufacture, became the raw material of an extensive industry. It will be observed that science has here done more than replace natural by artificial production. She has produced substances which, so far as we know, have no existence as natural products. There are many thousands of substances not existing in nature which have been produced in the laboratory, and an unlimited number of such combinations are possible. So that the coal-tar colours are not otherwise remarkable than as an instance of this power of chemical science which is popularly appreciable. The processes by which a substance like benzol is made to yield highly complex bodies are such as would never have been discovered without the guidance of a theory representing at least a considerable number of the true relations of different substances. To discuss at large such relations or such processes would be foreign to our present purpose, which must be confined to indicating the form of the general principles of the science, rather than presenting the matter of the science itself. This paragraph may, therefore, be regarded as a digression for the purpose of suggesting, by an instance coming home to the general reader, the validity of modern chemical theory as an instrument of investigation.

We might mention scores of names of eminent men whose labours and discoveries have advanced and are advancing the boundaries of chemical science in every department. We are here, however, concerned only with such of the more general aspects of the science, as may be presumed capable of being made intelligible to a general reader. We shall, therefore, bring this chapter to a close by pointing out some methods of chemical research which are daily growing in importance, and appear to indicate the path along which the science is to advance.

If hydrogen and oxygen gases be mixed within certain limits of proportion, a flame applied to the mixture causes a sudden combination of the gases (attended with light and heat) in one definite proportion, viz., two volumes of hydrogen with one of oxygen. The excess above that proportion of either will be left unaltered. The explosion, as the sudden union of the gases is called, may be determined by an electric *spark* or by a piece of incandescent metal. The spark, indeed, operates only by the *heat* attending it. This combination of the gases is often effected in the *eudiometer* invented by Volta. Its construction and use will be obvious from a mere inspection of Fig. 315, which requires no explanation beyond the statement that the eudiometer is here used with mercury contained in a marble trough. We see that heat causes the explosive combination of a mixture of oxygen and hydrogen gases, especially when these gases are mixed in the proportions in which they

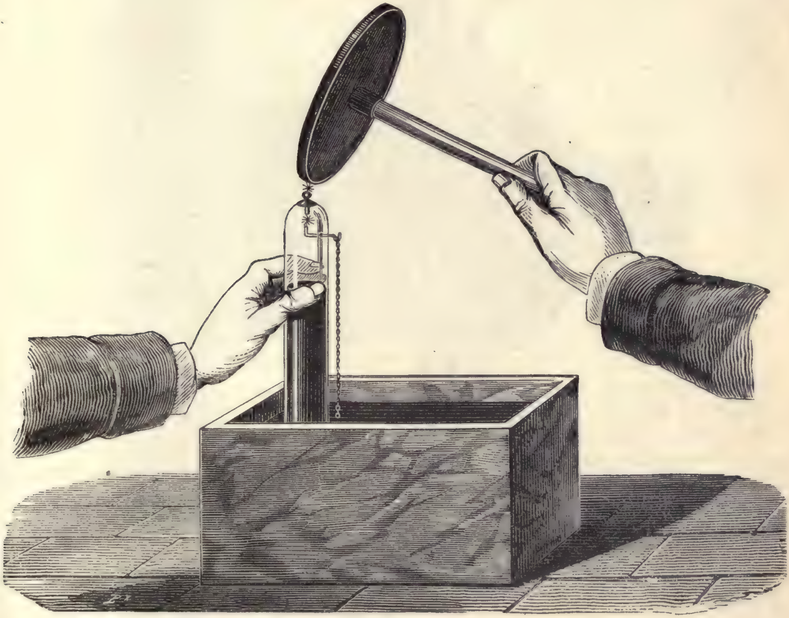


FIG. 315.

enter into the composition of water. Many years ago Mr. Grove (now Sir R. Grove) found that by passing steam through intensely heated tubes the inverse action occurred. The steam was decomposed into its elements, and a mixture of the dissociated hydrogen and oxygen could be collected. The quantity was, however, relatively very small compared to the bulk of the steam, and it is evident on consideration that the gases once separated must have become diluted, so to speak, with a large excess of water-gas, or undecomposed steam, in order to escape recombination. A modification of the experiment was afterwards devised which permitted the dissociated gases to be collected

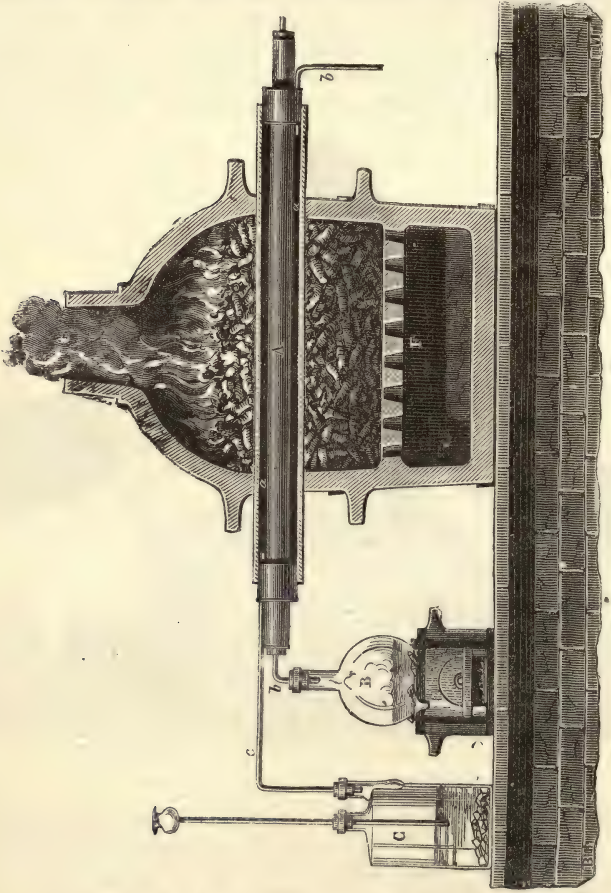


FIG. 316.

in larger quantities. The arrangement is shown in Fig. 316, where is seen a section of a furnace of a *glazed* porcelain tube passing through it. Within the outer is another tube, A, of *porous* earthenware. The space *a* between the inner and outer tube is connected by the tube *c* with the bottle C, which contains the materials for evolving carbonic acid gas, and at the other end is the exit tube *b*. When the furnace is in full activity steam is driven into the porous tube by boiling the water in the vessel B, this steam being decomposed by the heat; the hydrogen for the most part diffuses through the porous tube into the space *a*, and with the carbonic acid gas passes through the tube *b* into a solution of potash, which absorbs the carbonic acid and permits the hydrogen to be collected alone. The oxygen is collected chiefly from the central tube in the same manner. We see that in this experiment the porous tube is a filter, as it were, by which the hydrogen is removed from contact with the oxygen, and thus recombination in the cooler part of the apparatus is prevented.

This experiment seems to show that at a certain very high temperature steam would be completely decomposed into its elements, and that at that temperature the two gases might be mixed in combining proportions without combination taking place. If the temperature were allowed to decrease gradually, the gases would combine, passing gradually from a condition in which the vessels contained only a mixture of oxygen and hydrogen to one in which it contained nothing but steam, through every intermediate stage in which both steam and uncombined gas are present. These considerations show that chemical and thermic forces are definitely related to each other, and that the quantities of the several factors and products of a reaction may exist in the presence of each in proportions varying through an interval of time determined by the surrounding conditions, according to laws which it might be possible to discover. These laws would express the rate at which the chemical action takes place under the given circumstances; that is, instead of studying merely the final product of a chemical action, we should be studying the progress of the action—the forces in the very act of their operation.

The difference between examining the products only of a chemical reaction and examining the progress of the reaction itself, is analogous to that between *statics* and *dynamics* in mechanics. Hence the study of the laws of the progress of chemical reactions is termed *chemical dynamics*. This mode of investigating chemical phenomena promises to be prolific of results of great importance when it is fully developed.

Another method of investigation has been applied to the science by the eminent French chemist, M. Berthelot (who must not be confounded with the older chemist, Berthollet, page 379). In other departments of science the forces in operation are reduced to terms of mechanical effort or *work;* and it is by the same standard, or rather by its equivalent in heat, Berthelot measures the chemical forces which take part in reactions; and when the thermic values of these have afforded the requisite data, one principle will enable us always to define the results of any given reaction,—the principle of maximum work. The work is measured by the quantity of heat developed in a chemical action. In the estimation of this quantity, allowance must be made for merely physical changes, and the substances must be referred to one state. M. Berthelot, in attempting to construct a theory of thermo-chemistry in 1864, found so many voids and uncertainties in the data, that he undertook a prolonged and laborious series of experiments extending over sixteen years, the results being announced from time to time, and finally embodied in two large volumes recently published by him.

Now, as an illustration of the manner in which the atomic theory is applied in modern chemistry, we may take the doctrine of *series* in carbon compounds. A chemist named SCHIEL (1842) appears to have been the first to call attention to the fact that a certain class of compounds (*alcohols*) form a regularly progressive series, in composition and in properties. Gerhardt and Laurent made this the principle of a systematic arrangement of the carbon compounds. As an example, a series may be mentioned, the very numerous compounds of which consist of nothing but carbon and hydrogen. Some of these are gases, others are liquids or solids at ordinary temperatures. Experiment shows the relative quantities of the two elements contained in each compound, and the density of the substance, that is, as to how many times the gas, or the liquid or solid raised by heat to the state of gas, is heavier than pure hydrogen gas at the same temperature and pressure. From the former the *ratio* between the number of carbon atoms and the number of hydrogen can be inferred, the carbon being known from other considerations to be twelve times as heavy as the hydrogen atom. The density (or specific gravity) determines (by Avogadro's law) the actual numbers by which the ratio is to be expressed, for it tells how much heavier is the molecule of the gas or vapour than the molecule of hydrogen (page 613). These relations will be seen in the following table of a set of hydro-carbons.

| No. | Name. | Condition. b. p. = boiling point of liquid. | Experimental facts. | | | Formula. |
|---|---|---|---|---|---|---|
| | | | Composition by weight in 100 parts. | | Density compared with hydrogen = 2. | |
| | | | Carbon. | Hydrogen. | | |
| 1 | Hydrogen . . | Gas . . . | 00·0 | 100 | 2 | $H_2$ |
| 2 | Marsh gas . . | Gas . . . | 75·0 | 25·0 | 16 | $CH_4$ |
| 3 | Ethylic Hydride. | Gas . . . | 76·6 | 23·3 | 30 | $C_2H_6$ |
| 4 | Propylic ,, | Gas . . . | 81·8 | 18·2 | 44 | $C_3H_8$ |
| 5 | Butylic ,, | b. p. 1°C. . | 82·7 | 17·3 | 58 | $C_4H_{10}$ |
| 6 | Amylic ,, | b. p. 30° C. | 83·3 | 16·7 | 72 | $C_5H_{12}$ |
| 7 | Hexylic ,, | b. p. 61° . | 83·7 | 16·3 | 86 | $C_6H_{14}$ |
| 8 | Heptylic ,, | b. p. 90° . | 84·0 | 16·0 | 100 | $C_7H_{16}$ |
| 9 | Octylic ,, | b. p. 120° . | 84·2 | 15·8 | 114 | $C_8H_{18}$ |

There exists another set of hydro-carbons, distinguished from the foregoing and others by their own special chemical properties. Like the former, there are gases, liquids, and solids. These have all one and the same per-centage composition, and therefore by analysis alone they would be indistinguishable; but by the analytical results taken in conjunction with Avogadro's hypothesis (page 613) we are enabled to assign formulæ of these also:

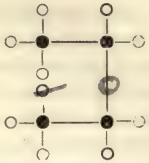
| No. | Name. | Condition. b. p. = boiling point of liquid. | Experimental facts. | | Formula. |
|---|---|---|---|---|---|
| | | | Composition by weight in 100 parts. | Density compared with hydrogen = 2. | |
| 1 | Methylene | Gas | 14·29 : 85·71 | 14 | $CH_2$ |
| 2 | Ethylene | Gas | ,, | 28 | $C_2H_4$ |
| 3 | Propylene | Gas | ,, | 42 | $C_3H_6$ |
| 4 | Butylene | b. p. 35°. | ,, | 56 | $C_4H_8$ |
| 5 | Amylene | b. p. 55°. | ,, | 70 | $C_5H_{10}$ |
| 6 | Hexylene | b. p. 39°. | ,, | 84 | $C_6H_{12}$<br>etc. |

It will readily be seen that the formula of each compound in both the series differs from that below it by having $CH_2$ less; so that the first series may be expressed by this *general formula*, $C_nH_{2n}+_2$; and the second series by $C_nH_{2n}$. There are other series of hydro-carbons, and the general formulæ of these themselves constitute a still more general series. Thus:

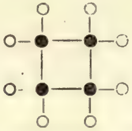
(*a*) $C_nH_{2n}+_2$.
(*b*) $C_nH_{2n}$.
(*c*) $C_nH_{2n-2}$.
(*d*) $C_nH_{2n-4}$.
(*e*) $C_nH_{2n-6}$, etc.

The law of this series will be sufficiently obvious. These regular mathematical progressions are very striking, and induced chemists to seek for some theoretical explanation. This is found by attributing to each carbon atom the power of fixing one hydrogen atom at each of *four points*, and of linking with other carbon atoms also at similar points of attraction. Each carbon atom may roughly be imagined to have *four* hooks or bonds, by each of which it may link on to a hook of another carbon atom, or may fix one atom of hydrogen. We shall now *graphically* represent the atomic constitution on this hypothesis, the four-carbon member of each of the series (*a*), (*b*), (*c*), (*d*), and (*e*), and leave the reader to follow out the like relations in other cases. The black circles are supposed to represent carbon atoms, and the open circles hydrogen atoms, the four bonds of each carbon atom being represented by the four straight lines attached to each circle.

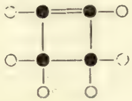
(*a*) $C_4H_{10}$



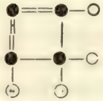
(*b*) $C_4H_8$



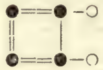
(*c*) $C_4H_6$



(*d*) $C_4H_4$

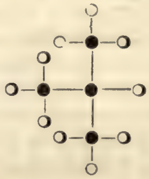


(*e*) $C_4H_2$



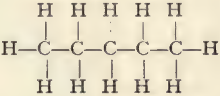
The foregoing also illustrates the notion called the *valency* or *atom fixing* power of atoms. Each carbon atom has four bonds or points of attachment, or the power of fixing four atoms having each but one bond. Of this last kind are hydrogen, chlorine, potassium, silver, etc., and these are spoken of as *monovalent* elements; while oxygen, etc., are *divalent;* and so on up to *hexavalency.*

There is another subject which may here be illustrated. If we consider with a little attention the representation of the atoms of the group $C_4H_{10}$, it will be seen that there is another mode in which the atoms may be linked, namely, thus:



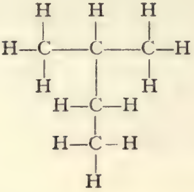
$C_4H_{10}$

In the first arrangement no carbon atom is joined to more than two other carbon atoms; in the second, one of the carbon atoms is directly connected with three others. If we take the next higher member of the (*a*) group, $C_5H_{12}$, we shall find on trial that *three* different arrangements are possible; viz., ($\alpha$) one in which no carbon atom unites with more than two others; ($\beta$) one in which one of the carbon atoms unites with *three* others; and ($\gamma$), one in which one of the carbon atoms unites with *four* others. We here represent these three modifications as before, only substituting for the black and white circles the literal symbols of carbon and hydrogen respectively, which is the more usual mode of exhibiting these "*graphic* formulæ," as they are called.

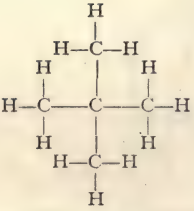


$$\begin{array}{ccccccccccc} & & H & & H & & H & & H & & H & & \\ & & | & & | & & | & & | & & | & & \\ H & - & C & - & C & - & C & - & C & - & C & - & H \\ & & | & & | & & | & & | & & | & & \\ & & H & & H & & H & & H & & H & & \end{array}$$

$C_5H_{12}$ ($\alpha$)



$C_5H_{12}$ ($\beta$)



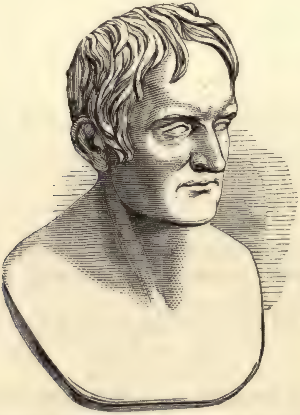
$C_5H_{12}$ ($\gamma$)

Or, writing the symbols in a more condensed form, while retaining the same notion of their construction, we may exhibit them thus:

($\alpha$) $CH_3 . CH_2 . CH_2 . CH_2 . CH_3$; ($\beta$) $CH_3 . CH_2 . CH(CH_3)_2$;
($\gamma$) $C(CH_3)_4$.

Now, chemists are acquainted with a vast number of instances in which substances of more or less different properties have precisely the same per-centage composition, and have also in the state of vapour the same specific gravity; that is, their molecules are formed of the same number of atoms of the same kind. The difference of their properties can, therefore, only be accounted for by a *different arrange-*

*ment* of the atoms constituting the molecule. Such substances are said to be *isomeric*. We have just seen the theoretical possibility of two isomeric hydro-carbons $C_4H_{10}$, and of three $C_5H_{12}$'s; and so we might deduce the possibility of four different $C_6H_{14}$'s, six $C_7H_{16}$'s, and so on. Now, it is a fact that in several cases all the isomeric compounds foreseen by the theory have been recognized as actually existing substances. In other cases only some of the *isomerides* have yet been prepared. The facts of isomerism thus lend a very efficient support to the atomic theory. For example, there are two hydro-carbons known, each of which contains 82·77 per cent. of carbon and 17·23 per cent. of hydrogen, and in the state of vapour is just thirty times as heavy as hydrogen gas under the same circumstances of temperature and pressure. These facts show that the molecular formula for both is $C_4H_{10}$. They are, however, different substances, being prepared in different ways, and possessing distinctly different properties in certain respects, although in general chemical character they resemble one another. For instance, $CH_3.\ CH_2.\ CH_2.\ CH_3$ is a liquid which boils at 34° F., while $CH(CH_3)_3$ boils at 5° F. Of isomeric $C_5H_{12}$'s three have been prepared. Of the next member of the series, $C_6H_{14}$, only three modifications are known; it is presumed, therefore, that one remains yet to be discovered.



DALTON (from the Statue by Chantrey).

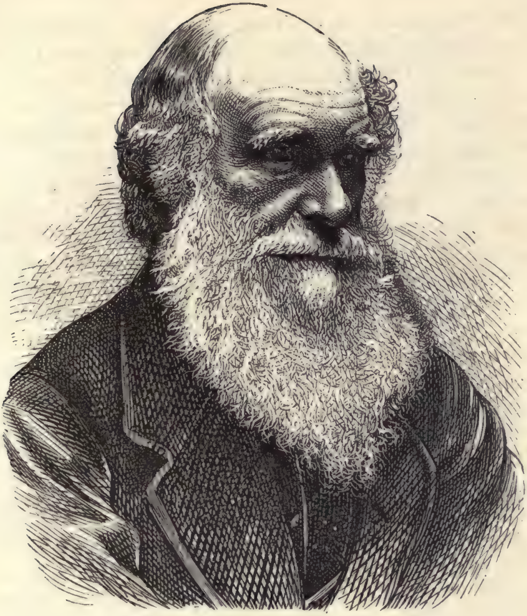


FIG. 317.—CHARLES ROBERT DARWIN.

# CHAPTER XXII.

## NATURAL HISTORY SCIENCES OF THE NINETEENTH CENTURY—ZOOLOGY, BOTANY, GEOLOGY, AND METEOROLOGY.

AS we have already indicated, it is only the wider generalities of the sciences included in the above title that can come within the scope of the present volume. If we consider the science of living things alone, we come upon studies that have divided themselves into a hundred departments, which have severally taxed the lifelong energies of men gifted with the clearest intellects and the keenest faculties of observation. It would be obviously impossible to place before the general reader the details of even one of those explorations of organic nature which have given form to the biological science of the present day. To the multifarious and complex phenomena presented by the world of living things some clues appear to have been attained in the physiological principles established by the labours of the present century, and by the working out into now received theories of certain ideas which originated, or, at least, were first clearly enunciated, by a few philosophers about the close of last and beginning of the present century. Some of these ideas have been placed before the reader in Chapter XV. (page 390), where Lamarck's "*Philosophie Zoologique*" is referred to in connection with our notice of that author and his classification of the invertebrata. ETIENNE GEOFFROY ST. HILAIRE (1772—1844), another great French naturalist, who has also been referred to in Chapter XV., had by the close of last century put forward on the transformation of species views essentially the same as those of Lamarck. But whereas the latter considered that the use or disuse of organs was the agency of the transformation, St. Hilaire conceived it to be effected rather by changes in the surrounding circumstances; as, for example, when he thinks that birds may have originated from lizard-like reptiles by a diminution of the carbonic acid in the atmosphere, in consequence of which a more active oxygenation and higher temperature of the blood quickened the creatures and caused their scales to become feathers, etc. St. Hilaire maintained that throughout the animal world there existed a general plan of organization, the mere modification of which in this or that direction gave rise to the different races of animals. This unity of plan or *homology* may be illustrated by the identity of structure which is found beneath the superficial differences in the arm of a man, the fore-leg of a horse, the wing of a bird, and the paddle of a whale. In the earlier part of the century commenced many warm controversies on such topics, and we find Cuvier opposing the homological views of St. Hilaire with all his might. In the year 1830 a debate was carried on between these two naturalists on the unity of animal organization before the Académie des Sciences, and was continued for five sittings. An account of this debate, with remarks upon the points raised, was the very last production of the great German poet GOETHE (1750—1832), written but a few months before his death. Goethe had in the course of his life devoted not a little attention to science, and written on such subjects as osteology, meteorology, geology, etc. Perhaps but for his unfortunate theory of colours the ideas Goethe had himself put forth on certain points of natural history would sooner have commanded attention, as they tended powerfully to support the notion of the unity of organic nature. The existence of an intermaxillary jaw-bone in the human subject was one of Goethe's own discoveries, inferred from the law of homology, and confirmed by actual observation after the examination of a great number of human skulls.

There is another kind of homology to which Goethe was one of the first to draw attention as a law of animal organization. It is the relation of similarity in the parts placed one behind another in the same individual. An instance so familiar as to be recognized by every one is the similarity of the legs and arms. The correspondences of

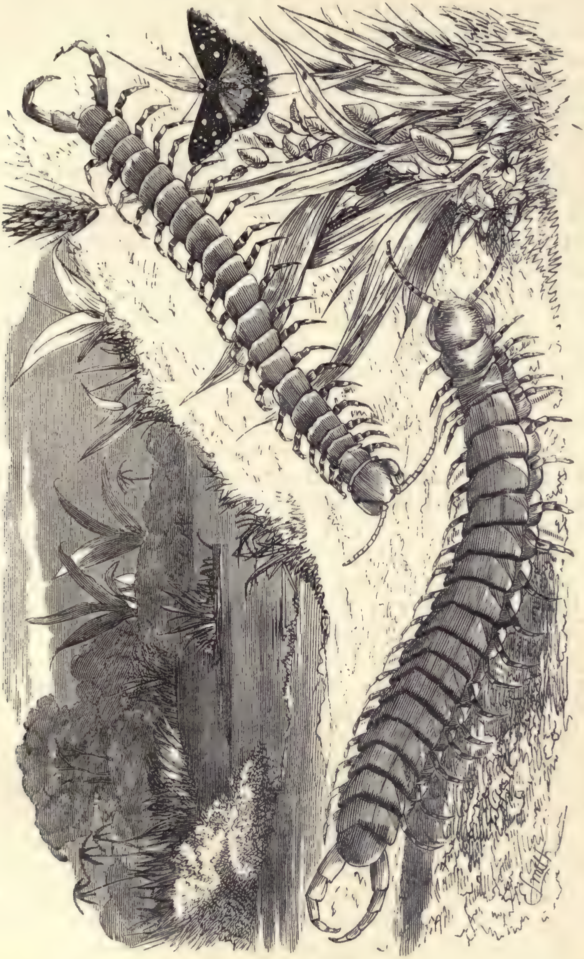


FIG. 318.—SCOLOPENDRA ANGULATA (Lower figure). SCOLOPENDRA VARIEGATA (Upper figure).

the foot with the hand, of the ankle with the wrist, of the knee with the elbow, are too obvious to be overlooked. But very little knowledge of anatomy suffices to show that the resemblance is much deeper: that a single bone (the *femur*) articulates with the hip, and a single bone with the shoulder; two bones (the *tibia* and the *fibula*) form the leg below the knee; two (the *radius* and the *ulna*) enter into the structure of the fore-arm. The foot and the hand are each supported on a series of small bones, and consist of five similar pieces (the *metatarsal* and *metacarpal* bones) and of an equal number of toe and finger-joints. This kind of resemblance has received the name of *serial homology*. The homology of parts in a series may be strikingly illustrated by instances in which the similarly constructed parts are very numerous, as in the centipedes, two of which are represented in Fig. 318. The common lobster furnishes a case in which the *serial homology* may be traced under the modified forms. The six segments of the so-called tail, show it clearly enough; however, in the front part, the segments in the adult animal coalesce; but on looking at the under surface of the body we find a series of pairs of movable appendages taking on different forms. Such are the feelers, the jaws, the foot-jaws, the claws, the legs, and the swimmerets attached to the segments of the "tail." Now, although the segmentation is disguised in the fore part of the lobster, there are species of closely allied forms in which the segments remain more distinct. There is in general but little (except the two pairs of limbs) in the outward forms of vertebrate animals to suggest a serial homology; but the case is different when we contemplate the structure of the skeleton, and especially that of the back-bone. There the serial arrangement of *vertebræ* having a common conformation is conspicuous, and there are the similarly constructed pairs of ribs. The skull appears to have no relation to any serial arrangements, yet Goethe had at the beginning of the present century arrived at the conception of the bones of the skull being formed by modifications of the parts of a series of vertebræ. The origination of this idea is, however, commonly attributed to LORENZ OKEN (1779—1847), who describes how in 1806, when one day rambling in the Hartz Forest, he came across the blanched skull of a deer, and the thought flashed into his mind, "*It is a vertebral column.*" These ideas were afterwards taken up and developed, more particularly the eminent English comparative anatomist, Professor OWEN (1804—. . . .), in his celebrated work entitled "On the Archetype and Homologies of the Vertebrate Skeleton," published in 1848. Oken's book on the "Philosophy of Nature" contains the first suggestions of other conceptions which have in more recent times been developed in biological science. For example, his Urschleim ("Primitive Slime") is the *protoplasm* of recent observers, and (under another name) he refers to the *cell* as the structural element of all organisms.

The doctrine of the descent of species by gradual transformation, which has exercised so much of the thought of the present century, may be traced in its rudimentary forms in the writings of several philosophers and naturalists of last century. ERASMUS DARWIN (1731—1802) in his "Zoonomia," 1795; KANT (1724—1804), the great German metaphysician, in a work published in 1790; Goethe, in various passages of his writings produced between 1780 and 1832; TREVIRANUS, of Bremen (1776—1837), in his "Biology" (1802); Oken, and Lamarck, appear to have all independently and nearly simultaneously arrived at similar conceptions of the unity of organic nature, and of its plasticity or power of moulding itself to the surrounding conditions. Such ideas were not generally accepted by naturalists until about twenty years ago, when, mainly by the labours of an English naturalist, they were raised to the rank of a comprehensive theory, and based upon a wide range of co-ordinated facts.

CHARLES ROBERT DARWIN, born at Shrewsbury in 1809, was the grandson of the author of "Zoonomia," "The Botanic Garden," etc. He studied at Edinburgh, and afterwards at Cambridge, where he graduated in 1831. In the same year he sailed as naturalist with Captain Fitzroy, in H.M.S. *Beagle*, on a five years' voyage of scientific exploration. Mr. Darwin published an extremely interesting account of this voyage, and various important papers and treatises on geological and zoological subjects. Soon after his return to England he took up his residence on an estate he had acquired near Bromley, in Kent, where he devoted himself to the solution of the problem that had presented itself to his mind during his voyage. For twenty-one years he was collecting facts and carrying on observations on several species of plants and animals which for this purpose he largely cultivated and bred on his estate. He had written in 1844 a preliminary sketch of his theory, but he refrained from publishing a single word concerning it until he had accumulated a store of facts upon which it might be firmly based. In 1858 Mr. ALFRED RUSSEL WALLACE, a distinguished traveller and naturalist, who had spent many years of scientific observation in the Malay Archipelago, and was then at Ternate, sent to England for publication an essay which showed that he had then arrived at the very same conclusions to which Darwin had come some years before. Mr. Wallace sent his paper for publication to Mr. Darwin himself, whose own views, however, had never in any form been made public. But on the solicitation of some friend, Mr. Darwin now published several extracts from his manuscripts written long before, and these were read before the Linnæan Society on the same evening as Mr. Wallace's essay. In 1859 Darwin's famous book "On the Origin of Species" was published, and announced as preliminary to a larger work in two volumes—"The Variation of Animals and Plants under Domestication," which afterwards appeared in 1868. It is in these works that Mr. Darwin expounds that "Theory of Natural Selection" which, at the present day, everybody has heard of as *Darwinism*. It was not Darwin who originated the Theory of Descent, or of the development of one species from another; that, as we have already seen, was clearly proposed by Lamarck and entertained by others. But it was Darwin and Wallace who first showed that real causes existed by which the transmutations may have been produced. It is impossible within our limits even to suggest the various classes of facts and illustrations which Darwin and Wallace have advanced in support of the Theory of Natural Selection.

This theory has, like others already noticed in these pages, met with abundant opposition. The literature of the subject, though extending over only twenty years, is extensive enough to occupy a whole library. As many of the works relating to "Darwinism" are very popular and accessible books, we shall here merely quote two brief and clear statements of the Theory of Natural Selection. The first is from St. George Mivart's "Genesis of Species," where it runs thus:—

> "Every kind of animal and plant tends to increase in numbers in a geometrical progression.
>
> "Every kind of animal and plant transmits a general likeness, with individual differences, to its offspring.
>
> "Every individual may present minute variations of any kind and in any direction.
>
> "Past time has been practically infinite."

Mr. Wallace presents a demonstration of the Origin of Species by Natural Selection in a tabular form nearly like the following:—

| PROVED FACTS. | NECESSARY CONSEQUENCE. (Taken in the next following deduction as a "proved fact.") |
|---|---|
| 1. *Rapid Reproduction of Organisms.*<br>2. *Total Number of Individuals Stationary.* | 3. STRUGGLE FOR EXISTENCE; the deaths on the average equalling the births. |
| 3. *Struggle for Existence.*<br>4. *Heredity with Variation*, or general likeness with individual differences. | 5. SURVIVAL OF THE FITTEST, or Natural Selection. |
| 5. *Survival of the Fittest.*<br>6. *Changes of External Conditions*, universal and unceasing. | 7. CHANGES OF ORGANIC FORMS, to keep them in harmony with the changed conditions. As these never recur, so the divergences of organic form are permanent. |

According to the Theory of Natural Selection, feline races did not obtain their retractile claws by their habits, or their efforts, or their desires, but because in the struggle for existence among less highly organized forms, those survived which were best able to seize upon their prey. This "survival of the fittest" is a very different explanation of the changes in organisms from that advanced by Lamarck (page 396).

The Theory of the Descent of animal and vegetable species from common and simple prototypes, as held by Lamarck and Goethe, when worked out and based on the Theory of Natural Selection, may be appropriately called the Theory of Development. Its opposite is the view which regards species as eternal and invariable, and this view has been stoutly defended against Darwin and his numerous supporters. Indeed, the unqualified and unopposed acceptance of any important theory would, as the reader of these pages must already know, be a new thing in the history of science. Nevertheless, the Development Theory must be considered as a good working hypothesis, now accepted by naturalists in general without a single eminent exception. How profoundly this theory has modified biological conceptions may be illustrated by contrasting the older definitions of *species* and *genera* with those of the present day. Linnæus defined his notion of species by saying, "There are as many different species as there were different forms created in the beginning by the Infinite Being." Cuvier, who thought a science of living things impossible unless based on the immutability of species, gave a more practical definition of species by

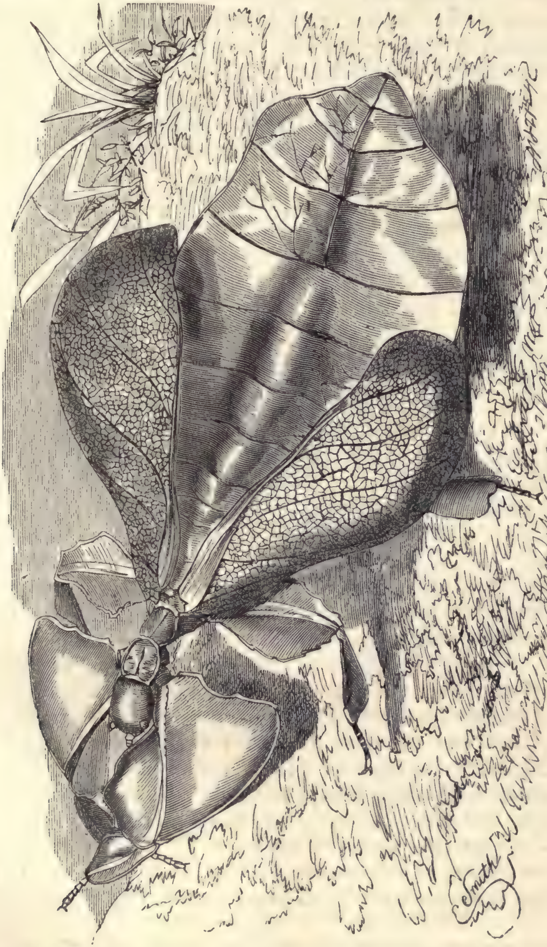


FIG. 319.—LEAF INSECT (Female of *Phyllium scythe*).

saying that "All those individual animals and plants which can be proved to have descended from one another or from common ancestors, belong to one species." Compare these definitions with that given of the species, etc., of plants on page 644.

The doctrine of Descent by Natural Selection has shed a light upon the interpretation of many groups of facts in natural history hitherto involved in the deepest obscurity. It has often been remarked that the colours of many animals are the same as that of their surroundings, and that the resemblances extend even to marks, spots, or textures. Good instances may be seen in any aquarium, as in the backs of flat fish, which so exactly reproduce the colours and general aspect of the gravel on which they lie, that the spectator must look attentively to distinguish the fish from the stones. These resemblances are in some cases carried to such a degree that the spectator is tempted to believe that some ingenious artist had made copies of the objects imitated. Striking examples of this "mimicry," as it has been termed, are presented by the "leaf-insects," of which there are many species. The aspect of these creatures in colour, markings, size, and texture, coincides with that of the leaves and twigs and moss among which they live. One species is represented in Fig. 319; but the reader who has never inspected the specimens to be seen in museums will not be able to appreciate the closeness of the imitation. Fig. 320 represents a creaure which is called "the walking-stick insect" (*Bacteria trophinus*), and will serve to show how nearly an animal can simulate a slender twig. Mr. Wallace says: "Some of these insects are a foot long and as thick as one's finger, and their whole colouring, forms, rugosity, and the arrangement of the head, legs, and antennæ, are such as to render them absolutely identical in appearance with dry sticks. They hang loosely about shrubs in the forests, and have the extraordinary habit of stretching out their legs unsymmetrically, so as to render the deception more complete."

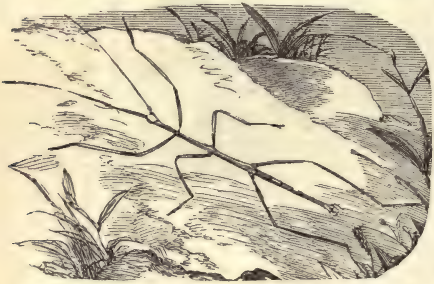


FIG. 320.—WALKING-STICK INSECT.

It is not alone through general philosophic theories that biological science has made the extraordinary advances which mark its history in the present century. The physiologist is now provided with instruments of precision by which to investigate his phenomena, and in physiological research experiment goes hand-in-hand with observation. Much of the great advance in this region of science has undoubtedly been due to the improvement of the microscope and its appliances. But these have been rendered effective by the discovery of methods of preparing tissues for examination by the use of chemical reagents which harden them, or impart colour, or reveal their structure. Much of what is known of the embryonic development of animals and plants has been the work of the present century. Harvey, Haller, Bonnet, Wolff (1759), and Oken, certainly contributed to found Embryology, but it is mainly by the laborious investigations of some distinguished Continental physiologists of the present century that embryology has become a great and important department of biology. In 1817 CHRISTIAN PANDER had made an elaborate study of the development of the chick in the egg, and his researches were continued and extended by a man whose name holds the highest place in connection with the subject. This was VON BAER, Professor in the University of Königsberg, who, after carrying on his observations for eight years (1819 to 1827), published in 1828 the first volume of his classical work entitled "The Development of Animals." The most remarkable general results of these researches which we shall here mention is the discovery of the universal process of "*yolk segmentation*," in which the *nucleus* of the primordial vesicle divides first into two cells, each of these again into two, and so on, the number of cells being successively 2, 4, 8, 16, 32, etc., until the embryo assumes under the microscope the appearance of a mulberry-like cluster of similar spherical cells enclosed within the original cell-membrane. At a later stage the cells are differentiated in their forms and arrangement. In their first stages all embryos have the same appearance; a little later the rudiments of the vertebræ are distinguishable in embryos of that class; but the development must make a considerable progress before the several embryos of mammals, birds, fishes, and reptiles can be discriminated by their structure.

Large additions to our knowledge of the globe and of its varied forms of life have been made by the expeditions for scientific exploration which have been fitted out at the expense of the different Governments, particularly in quite recent times. The deep-sea soundings and dredgings which have so much modified the views of naturalists and geologists, may be said to have been commenced in 1818 by Captain Ross in Baffin's Bay. The voyage of the *Challenger*, the most recent British enterprise for deep-sea exploration, will be familiar to most readers. The results of these voyages have been in many respects wholly unexpected, and they have especially extended and corrected the views entertained of the condition of the ocean and the distribution of animal life therein.

Passing over many researches of the highest importance to the avowed student of physiology, we might, had space permitted, have given some account of subjects so likely to command popular interest as the investigations which relate to the nervous system. Such are the doctrine of *motor* and *sensory* nerves established by—1st, the discoveries of CHARLES BELL, published in his "Anatomy of the Brain" (1811) and "Arrangement of the Nerves" (1821); 2nd, those of MAJENDIE; 3rd, those of JOHANNES MÜLLER; and the theory of *reflex action* due to the independent labours of the last-named physiologists, and of MARSHALL HALL (1832); and the more recent labours of FLOURENS, BROWN-SÉQUARD, FERRIER, and many others. Not a little general interest attaches also to the results of modern researches into the physiology of sensation by Young, PURKINJE, DONDERS, Müller, WEBER, Helmholtz, and others. We must be content to represent all these last by the brief reference made at page 445 to some of Young's work in this direction.

In the systematic Botany of the present century the chief name is AUGUSTIN PYRAMUS DE CANDOLLE (1778—1841), who was Professor of Botany at Montpellier in 1807, and at Geneva in 1816, and published his "Elementary Principles of Botany" in 1804. His system was a modification of that of Jussieu (page 388). It made its way but slowly. In England it was first brought into notice by ROBERT BROWN (1773—1858), who employed it in his "Flora of New Holland," published in 1810. This system, in which plants are grouped according to their general affinities of form, structure, and vital functions, is with slight differences that adopted at the present day by ENDLICHER, LINDLEY, HOOKER, BENTHAM, etc. In classification, the fundamental question is of course the relationships of varieties, species, and genera. The older views regarded variation as taking place within certain limits only; and it was supposed that varieties necessarily tended to revert to the original stock. At the present day, most naturalists regard variation as continuous, and hold that, under appropriate external conditions, the variations of the descendants from a common stock may accumulate to constitute the difference of species. The five following propositions embody Mr. George Bentham's definitions of *varieties*, *species*, and *genera* in the vegetable kingdom:—

1. Although the whole of the numerous offspring of plants resemble their parent in all main points, there are slight individual differences.

2. The great majority of the few which survive and propagate in their turn, are under *ordinary circumstances* those which most resemble their parent: thus the *species* continues without material variations.

3. There are occasions, however, when individuals with slightly diverging characters survive, and reproduce races in which these divergences are continued with ever increased intensity: hence the origin of *varieties.*

4. In the course of numberless generations, circumstances may increase the divergency so far that the varieties will no longer propagate with each other: thus the varieties become *species.*

5. Species have in turn become the parents of groups of species; that is, of *genera*, *orders*, etc., of a grade higher according to the remoteness of the common parent, and more or less marked according to the extinction or preservation of the primary or intermediate forms.

Vegetable Physiology had, at the close of last century, made but little advance as compared with systematic botany. What had been done for the former was chiefly due to the labour of Grew, Malphigi, and Hales. In 1815 TREVIRANUS drew attention to the development of the vegetable embryo; and in 1823 AMICI discovered the fertilization of the *ovule* by the *pollen-tubes*. This subject has since occupied a host of observers; the processes of cell-formation (*cystogenesis*) has been perseveringly studied, especially by French and German microscopists such as SCHLEIDEN, SCHWANN, MOHL. About 1790 Goethe, seeking to satisfy the scientific spirit which in the contemplation of endless diversity craves to discover unity, clearly enunciated in his "Metamorphosis of Plants" a law which, although it attracted little attention at the time, was found afterwards to be a new light for the botanist. It declares that the various parts of plants, calyx, corolla, stamens, pistils, etc., are in reality but leaves modified to adapt them to particular functions. It is meant not that these organs individually were at one time leaves and were afterwards transformed, but that they are constructed of the same parts and elements as the leaf, and are arranged upon the same plan. In other words, the leaf is taken as the type of all vegetable organs. The similarity of sepals and petals to leaves is often sufficiently obvious, and by careful examination is traceable in all cases; and the community of nature in petals and stamens is proved by the instances in which the same parts of a plant are developed, sometimes into petals and sometimes into stamens.

In 1875 Mr. Darwin published, under the title "Insectivorous Plants," an account of his examination of certain plants which have the singular habit of capturing insects. The subjects of observation were species of *Drosera* (Sundew), *Dionæa muscipula* (Venus's Fly-trap), *Pinguicula*, etc., and the most remarkable result of the investigation was that these plants derive their nutriment from the bodies of the insects they entrap. Some of them actually *digest* the insects by a process analogous to that which occurs in the stomach of an animal. Observations of similar habits have also been made by Sir Joseph Hooker on other classes of plants, and will doubtless be extended. The *Dionæa muscipula* mentioned above is one of the most wonderful plants in the world, from the rapidity and force of the movements by which the insects are caught. The lightest touch affecting any one of three or four minute filaments which project from the face of the leaf, suffices to make the lobes immediately close, their margins turning inwards. The mechanism of this movement, which is so like that of an animal, has not yet been made out. It is singular that chloroform and other anæsthetics have a certain effect on these plants. Another of Mr. Darwin's works which should be noticed here is the "Fertilization of Orchids," published in 1862, in which he points out wonderful relations between insects and flowers, which strongly support his Theory of Natural Selection, and explain many particulars of both insect and plant organization hitherto without meaning.

The discoveries mentioned in the last paragraph connect curiously the animal and vegetable kingdoms. A distinction long relied upon for discriminating animals from vegetables was that the food of the former is *organic* matter, while that of the latter is *inorganic*. Strange as it may appear to a reader unversed in modern biology, the scientific demarcation of these two kingdoms of nature has proved a hard, if not an impossible, task. It is, of course, only with the lowest classes of each that this difficulty occurs. Not a few classes of organisms have been shifted alternately from the one kingdom to the other, according to the characters relied on for their distinction. An eminent naturalist has proposed to provide a place for such organisms in a kind of neutral territory—the *Regnum protisticum*.

The continuity of nature would therefore appear to be unbroken as regards the two kingdoms of the organic world. Can a like continuity be traced between the organic and the inorganic? When recently the developmental theory of organisms came to the front, there appeared to be yet a link missing to complete the general doctrine of evolution. It was that connecting the organic world with the inorganic, and an old question was revived to be considered under new lights. It involves the principle that is called "spontaneous generation," or *abiogenesis*, which would assert that inorganic dead matter under suitable conditions is capable of evolving living organisms. Although "spontaneous generation" had been almost universally rejected by biologists, there were nevertheless many instances in which organisms made their appearance where the pre-existence of germs appeared to be impossible. A great many experiments of this kind were detailed in a work published by Dr. BASTIAN in 1871, which gave rise to much disputation. Renewed investigations were made by Dr. Roberts of Manchester, Professor Tyndall, and many others; and these went to prove that when certain precautions were taken to prevent the access of *germs floating in the air*, no appearance of organism or inorganic evolutions ever took place. Professor Tyndall propounded a theory of zymotic diseases as due to specific germs in the atmosphere. All these investigations are connected with the researches on *fermentation* of the eminent French chemist PASTEUR (*born* 1822). There are many different kinds of fermentation, and it was Pasteur's discovery that each kind of fermentation is dependent upon the presence of certain organisms which are different in each case.

Geology is a science in many ways peculiar. It is one of the newest of the sciences. It is continuous, by common matter and interdependence, so to speak, with many other departments of knowledge; with astronomy, with physics, with chemistry, with zoology, with botany, with mineralogy, with meteorology, with physical geography. According as the study is pursued under one or the other of its relations, so many branches of inquiry present themselves. The science might also be considered *descriptive*, *systematic*, or *theoretical*. First, as merely

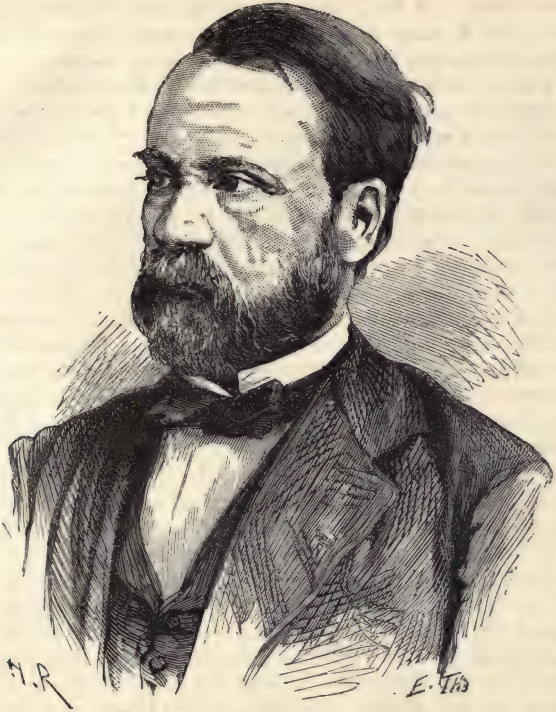


FIG. 321.—PASTEUR.

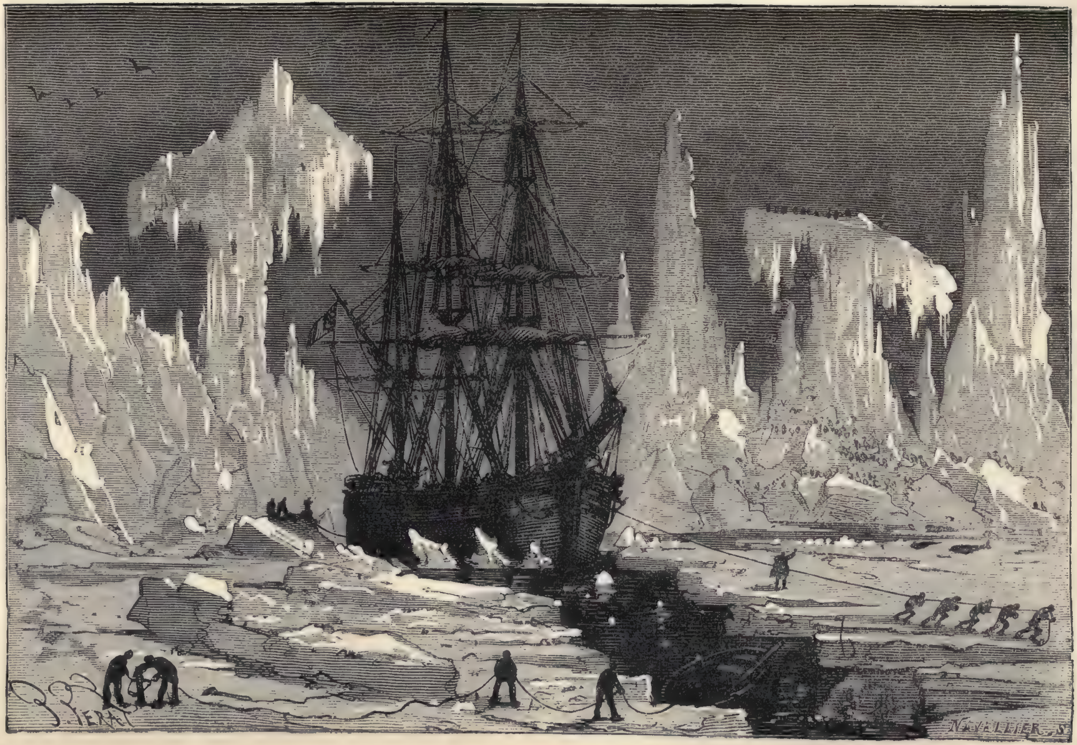
*descriptive* of the present actual state of things on the surface of our planet—the strata, their nature, organic remains, order of arrangement, the changes visibly going on by the action of waters, winds, volcanoes, etc. Second, *systematic* inquiry into the laws and principles of the actual arrangements and changes which can be arrived at by induction from observation and experiment. And third, *theoretical*, that most difficult but most fascinating department of the science, which undertakes to trace the past history of our globe and of the life upon it. This last problem is the subject of *theoretical* or *speculative* geology, and it is not surprising that the general principles on which its solution has been attempted have been very diverse. During the first part of the present century two antagonistic doctrines were contending for supremacy. One held that the changes which have obviously taken place in the earth's crust have been occasioned by great and sudden convulsions, such as upheavals or depressions of land, earthquakes, huge floods, etc. Cuvier was the most distinguished supporter of these doctrines. He held that the extinction of former species of vegetables and animals was due to great cataclysms, and that new species were afterwards created afresh, or, at least, that the distribution of land animals was repeatedly changed. The opposite doctrine is identified with the name of the late Sir CHARLES LYELL (1797—1875), the first edition of whose "Principles of Geology" appeared in 1830. Lyell's doctrine asserted that geological changes in past time have been continuous and uniform with those in progress at this day. Cataclysmic revolutions brought about by paroxysmal forces are regarded by Lyell as gratuitous assumptions. Much disputation was carried on for years between the *Catastrophists* and the *Uniformitarians*, but Lyell's views have finally been everywhere accepted. That all the changes which have ever occurred on the face of the earth have been produced by *the causes now in action*—that is, by forces distributed in similar proportion and acting with like intensity to those we now observe—is, it may be supposed, not the position contended for; but that these causes will account for the visible facts of geology if we admit a sufficiently extended time for their operation.

Among the more remarkable geological discoveries of the present century may be named that of evidence of epochs of intense cold having formerly prevailed over the now temperate regions of the earth. It was observations on existing glaciers that enabled AGASSIZ (1807—1874) to recognize over parts of Northern Europe and America traces of glaciers which once filled the valleys or extended in vast sheets over the land. Agassiz was appointed Professor of Natural History at Neuchâtel in 1832, and he soon afterwards published a great work on fossil fishes, in which he did for the extinct species of this class of vertebrates what Cuvier (page 402) had done for the fossil mammalia. In 1864 Agassiz was induced to accept a professorial chair in the United States, where he spent the remainder of his days in scientific work, greatly honoured by the citizens of his adopted country. It was of him that Longfellow wrote:—

And he wandered away and away
  With Nature, the dear old nurse,
Who sang to him night and day
  The rhymes of the universe.

And whenever the way seemed long,
  Or his heart began to fail,
She would sing a more wonderful song,
  Or tell a more marvellous tale.

The mechanism of glacier motion yet remains a puzzle to physicists and naturalists, notwithstanding the investigations of De Saussure, Forbes, Charpentier, Agassiz, Tyndall, Viollet-le-Duc, and others. Further research into the action of glaciers and icebergs, and the exploration of the vast regions of eternal frost which surround the Poles, would doubtless disclose many facts of the utmost importance to the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XV.—ICEBERGS.

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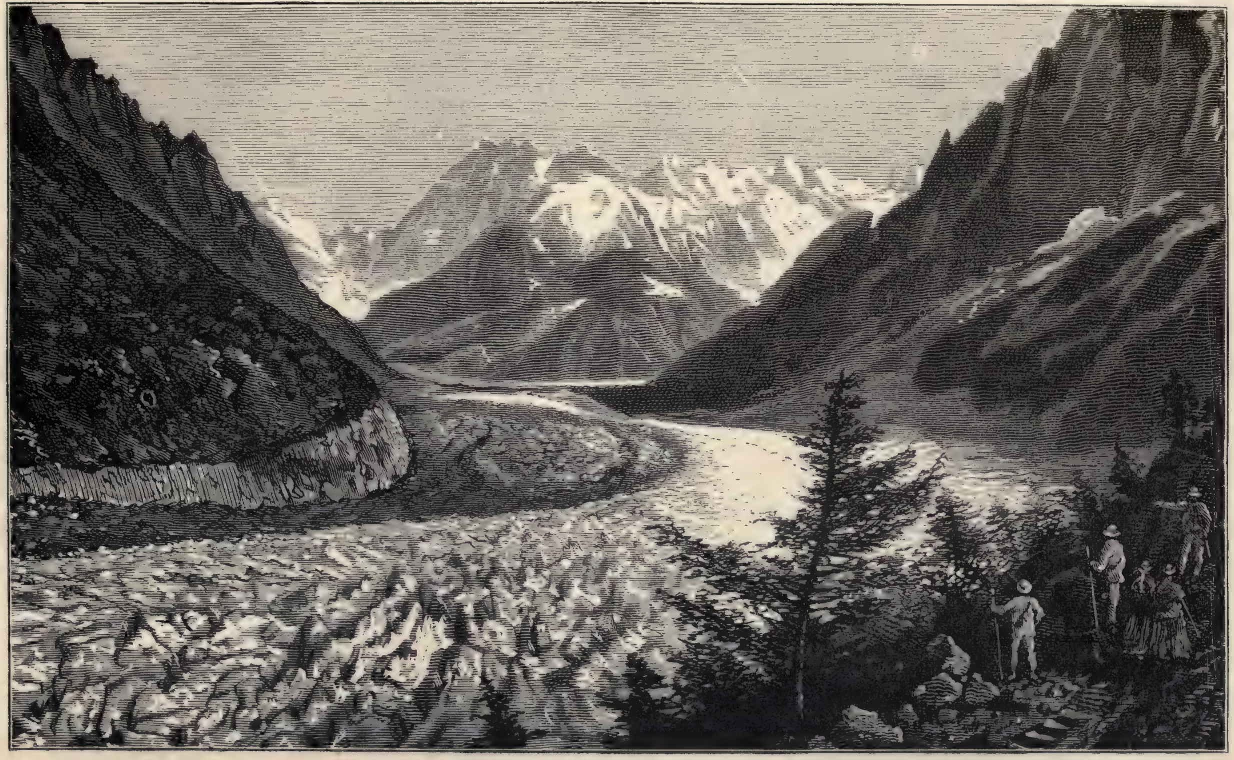

PLATE XVI.—Mer de Glace.

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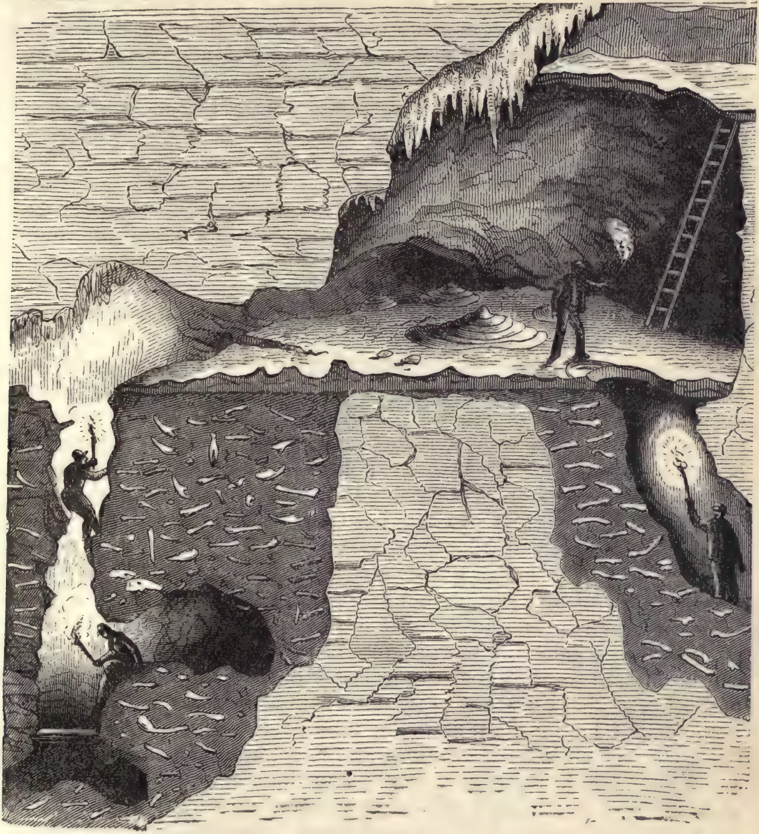

FIG. 322.—BONE CAVERN.

geologist. Science has gained by the many voyages in high latitudes which have taken place in the present century, and in the gallant attempts to reach the Pole itself which have been made in her interest.

Many bones of extinct species of mammalia have been met with in caves, and have received much attention from naturalists since Cuvier's time. These caverns, a typical section of which is shown in Fig. 322, generally occur in limestone rocks, and though the entrance is usually very narrow, they have been found to contain enormous quantities of bones of various animals, and in several cases traces of the human species, with whom these animals appear to have been contemporaneous. Some of these traces consist of the now well-known flint implements, the discovery of which attracted so much attention when it was announced in 1847 by M. Boucher de Perthes that he had found stone implements in the gravel-pits near Abbeville. Flint implements had, however, been found more than twenty years before this in a cave called Kent's Hole, near Torquay; but little notice had been taken of the circumstance. The reality of Boucher de Perthes' discoveries was discredited, and it was not until Mr. Prestwich visited the site in 1858 that geologists were aroused to the fact that evidence of the existence of mankind in a geological epoch had been obtained. Since that period flint implements have been found in great numbers in various parts of the world, and the inference that mankind existed on the earth at a very remote period seems to have been confirmed by other discoveries. Its correctness has, however, been called in question by some.

There is one branch of the general science of the earth which is almost universally interesting, and is in some degree studied by nearly every one. It is occupied with the investigation of the phenomena that specially belong to the aërial ocean at the bottom of which we live, and move, and have our being. This science seeks to ascertain the laws which govern storms and tempests, and to foretell their approach. That in late years great progress has been made in our knowledge of Meteorology is evident from the regular publication in our daily papers of scientific predictions of the state of the general weather for the next twenty-four hours in a certain number of districts into which the country is divided. The means by which it has been found possible to accomplish this, and generally the progress of meteorology in recent times, can hardly fail to possess a certain attractiveness, though the progress of discovery here does not present us with any of those springs and bounds that lend a quasi-dramatic interest to certain other paths of research. Countless observations and registrations of the readings of a few known indicating instruments, and the collation of an immense mass of facts, which have thus been gathered now by one person and now by another, do not offer any striking materials for these pages. And in truth meteorology has yet to make good its claim to be considered as an exact science. The subjects of meteorology are the motions, temperature, and other conditions of the atmosphere. It belongs, therefore, to the group of sciences which deal with the condition of our planet generally. To *foretell* the weather at any given place is no doubt an extremely useful application of the science, even when the prediction applies only to the next few hours. As knowledge increases, it will doubtless be found possible to extend the period over which the forecast applies, and give the forecast itself with increased confidence.

Meteorology is by no means a new study. Three centuries before the commencement of our era Aristotle wrote a treatise, τα μετέωρα, "the things above the earth." Nevertheless, scientific knowledge of the atmosphere cannot be said to date anterior to the period of the invention of the barometer and thermometer. These instruments, as first constructed, have been already described (pages 106 and 168). It is recorded that the height of the Torricellian column was daily observed by Pascal at Paris, by Perrier at Clermont, and by Descartes during the years 1649–50, and the readings were by agreement taken at the same hour of the day "in order to see if anything could be discovered by confronting them with one another." Here we see the synchronous mode of observation which plays so important a part in the modern system of meteorology set on foot by Pascal. Boyle (page 230) in 1665 observed the barometer in connection with the weather, and he added to its scale the words "fair," "set fair," etc., which are still commonly seen on the wheel-barometer. This last form of the instrument, however, was the invention of Hooke. The barometer has been constructed in various forms and modified according to the circumstances for which it is intended. In the marine barometer a part of the tube is made with a very contracted bore, in order to prevent the violent oscillations of the mercury, which the motion of the ship would otherwise cause. One of the best forms for stationary observatories is that represented in Fig. 323, and known as *Fortin's Barometer*. The glass tube is protected by being enveloped in a copper or brass tube C, in which two slits permit the height of the mercurial columns to be observed. On the edges of those slits the scale is engraved, and there is usually a sliding piece with a vernier (page 212), which enables the observer to read off the height of the mercury with great accuracy. The cistern A D B is cylindrical, the upper part D is of glass, and fits into a box-wood tube E, closed at the bottom with a piece of leather *b b*. This leather, by the action of the screw shown in the figure, can be raised or lowered in order that when a reading is about to be taken, the mercury in the cistern may be brought to a constant level. It is to this level that the graduations on the scale are referred, and it

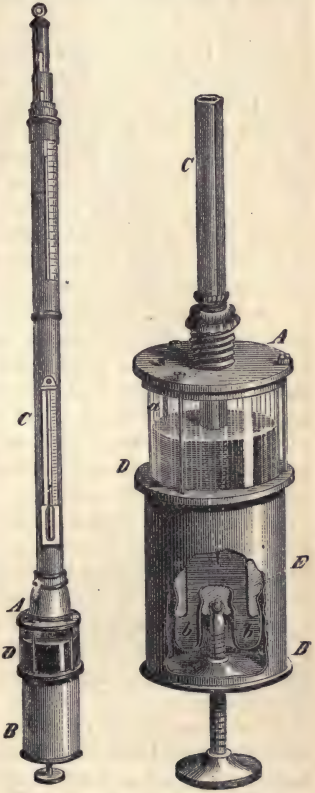

FIG. 323.—FORTIN'S BAROMETER.

is indicated by the point of the ivory pin *a* just touching the surface of the mercury. As the height of the mercurial column varies with the temperature, it is always necessary in exact observations to make such a correction as would give the height at some defined temperature, usually 0° C. A thermometer C is therefore attached to the barometer.

Barometric observations were long carried on, and the results registered at many stations, and these were compared with the state of the wind and weather at each place. Certain rules were sometimes deduced in which it was attempted to establish a relation between the barometer reading and the forthcoming weather at some of the localities. The reliance which could be placed upon these rules was, however, usually very small. When the more accurately constructed barometers had come into use, it was soon found that, independently of all other variation, the barometer has certain regular diurnal oscillations. It rises from 4 a.m. to 10 a.m., falls again until 4 p.m., rises between 4 p.m. and 10 p.m., and falls from the last-named hour to 4 a.m. In tropical countries these variations are so regular that the hour of the day might be determined from the maxima and minima readings within a possible error not exceeding the third part of an hour. An annual variation of the barometer has also been noted. Many interesting results of this kind have been obtained by self-registering instruments. Some of these are highly ingenious contrivances, giving a continuous record of the pressure of the atmosphere. Thus in the *barograph* used at Kew Observatory and other British meteorological stations, the height of the mercury is marked on sensitive photographic paper moved by clockwork night and day. At Greenwich the height of the column of mercury, exhibits on the average, variations the range of which may be thus indicated: over 132 days in the year the change is less than 0·1 inch, on 123 days it lies between 0·1 and 0·2, on 61 days between 0·2 and 0·3, on 27 days between 0·3 and 0·4, on 12 days between 0·4 and 0·5, on 6 days between 0·5 and 0·6, and on 4 days between 0·6 and 1 inch. Not, perhaps, oftener than once in ten years will it happen that the change of the barometer amounts to 1·25 inch within twenty-four hours.

While barometric observations were conducted independently, that is, without synchronous comparison, the hourly, daily, monthly, and yearly averages at each station were all the results the observations could show. A new era of meteorological progress was inaugurated with the system of comparing the barometrical readings taken simultaneously at different places, reported by telegraph, and collected together. But while barometers were still inaccurate in their construction, many investigators were comparing recorded results and reducing them to systematic expressions and actual laws: we might mention Sir John Ross, Sir John Franklin, and many others. At length Dovē broached his celebrated gyratory theory of storms. According to this theory, in all great storms there is a wind blowing *round* an area of comparative calm, which area has the lowest barometer reading, while, receding from this, the readings gradually indicate higher and higher pressures, until the limits of the storm are reached. It was in 1860 that the late Admiral Fitzroy, aided by the electric telegraph, first established a system of warnings of storms and inaugurated public weather forecasts in this country. In 1863 Mr. Galton published a work entitled "Meteorographica," in which for the first time we have charts of the weather, and it is to this author that we are indebted for the general features of the weather maps which the daily newspapers have made familiar to all. Mr. Galton pointed out that areas of high and of low barometric pressure are of vast extent, and that they present certain regular features. Their forms, however, are constantly changing, and the direction and rate of their movement vary greatly. The law which governs cyclones and the direction of the wind is very clearly laid down in "Meteorographica." We may first explain the meaning of some terms. If the barometer readings, taken at any one instant at a number of stations distributed over a large area of country, be compared, certain series of stations will be found where the readings have the same value, and if lines be drawn on maps through stations of equal pressures, these lines, which are called *isobars*, will be found to exhibit in general a certain parallelism, and not infrequently to form closed curves. This will readily be understood by a glance on a weather chart. Where a series of closed isobaric curves have values decreasing inwards, the middle point of the system is spoken of as the centre of a depression. If a line be drawn at right angles to a system of isobars, the difference of the barometric readings for two stations on this line, divided the distance between the stations, measures the *gradient*, which is directly as the difference, and inversely as the distance.

It might be supposed that difference of pressure is the cause of winds, and that the air would pass directly from the areas of high towards the areas of low barometric pressure; that is, that the direction of the wind would generally be at right angles to the isobars. So far, however, is this from being the case, that the opposite is the truth. "One universal fact is," says Mr. Galton, "that on a line being drawn from the locus of highest to the locus of lowest barometer, it will invariably be cut more or less at right angles by the wind, and the wind will be found to strike the left side of the line as drawn from the locus of highest barometer. In short, as by the ordinary well-known theory, the wind (in our hemisphere), when indraughted to an area of light-ascending currents, whirls round in a direction contrary to the movement of the hands of a watch; so conversely, when the wind disperses itself from a central area of dense descending currents, or of heaped-up atmosphere, it whirls round in the same direction as the hands of a watch."

Meteorologists have since reduced the general law of winds over all the world to this very concise form: *The wind blows along the isobars,* *In the northern hemisphere the barometric pressure is less on the left of the wind's course than on the right. In the southern hemisphere the pressure is less on the right of the wind than on the left.*

The observation of temperature is important to the meteorologist, and the improvements which in modern times have been made in the construction of thermometers have greatly facilitated the obtaining of correct data. There are some differences of opinion and of practice as to the proper conditions for finding the true temperature by a thermometer. The results are in general liable to be complicated with effects of radiation. As such questions do not here concern us, we pass them over to consider the principal improvement in all classes of thermometers. This consists in engraving the scale on the glass tube itself. For good instruments tubes are selected which have been filled many years before. The bore of the tube is gauged by causing a short column of the mercury to occupy various parts of its length successively. From the length of the little mercury column the capacity of tube is noted, and the corresponding allowance is made in the graduation. After the tube has been filled with the mercury and sealed, the first step is to immerse it in powdered ice, as shown in Fig. 324.

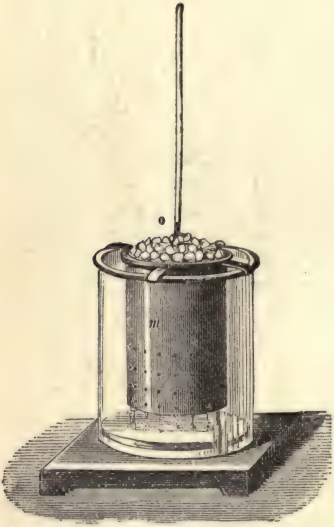

FIG. 324.

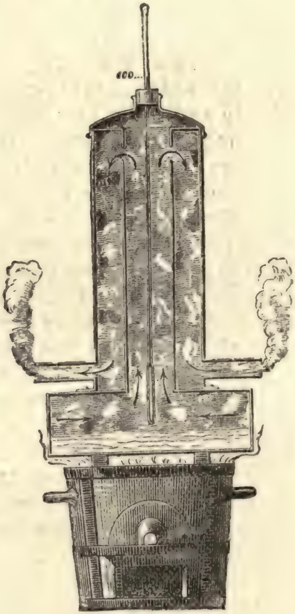

FIG. 325.

While the ice is melting, the mercury is stationary at a point which is marked by a scratch on the stem. The tube is then exposed to the vapour of water boiling in a metallic vessel when the pressure is at a certain standard (otherwise a correction is made). The apparatus used is represented in Fig. 325. In this way the upper fixed point is found and marked. The total internal capacity of the tube between the two fixed points is then divided into a number of points of equal capacity, and a dividing engine is used in marking the divisions in the tube.

There are several kinds of self-registering thermometers much used in meteorological observation. In Six's arrangement the tube is U-shaped, the bend of the U being occupied with mercury, separating two portions of spirit. The readings are taken from the positions of two small fragments of iron wire, which are moved by the motions of the mercury, and are replaced, after the reading has been taken, by means of a magnet. Rutherford's spirit thermometer, for registering minimum temperature, is a well-known instrument. Casella's is another ingenious instrument for the same purpose. Phillips, and also Negretti, devised maximum registering thermometers, which are much used in this country.

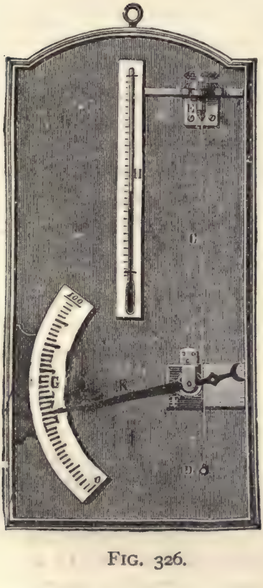

FIG. 326.

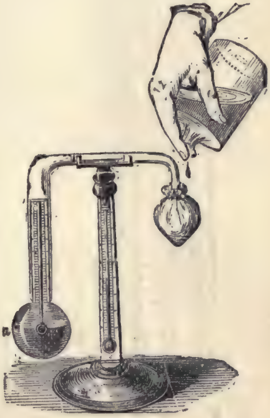

FIG. 327.

Next to pressure and temperature, the most important point to be ascertained as to the condition of the atmosphere is the amount of moisture it contains. The oldest instrument for indicating this condition is the hair hygrometer of De Saussure. It is represented in Fig. 326, and consists essentially of a hair C attached to a fixed support at E, and fastened below to an arm of the index K, which moves over the graduated arc G. The hair is kept in a state of slight tension by a small weight D. A thermometer B is usually attached to the apparatus. The scale on each instrument must be graduated by actual trial with air of known humidity, and, even after every precaution has been taken, little reliance can be placed upon the quantitative indications of the scale. It is, however, still much used in cold countries, especially in Russia. An instrument of much greater accuracy is the hygrometer invented by Daniell. It is represented in Fig. 327, and consists of a tube twice bent at right angles, and terminated by a bulb at each end. The bulb *a* is of black glass, and is half filled with ether, into which dips the bulb of a small thermometer enclosed in the bent tube. The other bulb *b* is covered externally by a piece of muslin. A thermometer is attached to the stand of the instrument to indicate the temperature of the air. In making an observation, ether is dropped upon the muslin, and the cold occasioned by the evaporation of the ether causes a condensation in *b* of some of the ether-vapour which fills the instrument. Thereupon some of the liquid in *a* evaporates, and by abstracting heat, soon so far cools the exterior of the bulb that the moisture of the external air condenses upon it. The moment this occurs the temperature of the enclosed thermometer is noted; again, at the instant the moisture disappears, the enclosed thermometer is read. The mean of the two readings is taken as the "*dew-point.*" From their observation the quantity of moisture present in the atmosphere may be inferred. Another instrument called the *psychrometer*, or the Wet and Dry Bulb Hygrometer, is shown in Fig. 328. It has two similar thermometers close to each other, the only difference being that one of them has the bulb covered with muslin, which is connected by some cotton wick with a vessel containing water. The evaporation which takes place from the moistened bulb lowers the temperature to a degree which depends on the quantity of moisture in the air.

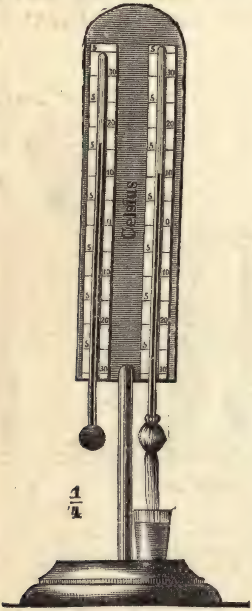

FIG. 328.

The measurement of the rainfall is perhaps the simplest of all the determinations which the meteorologist is called to constantly make.

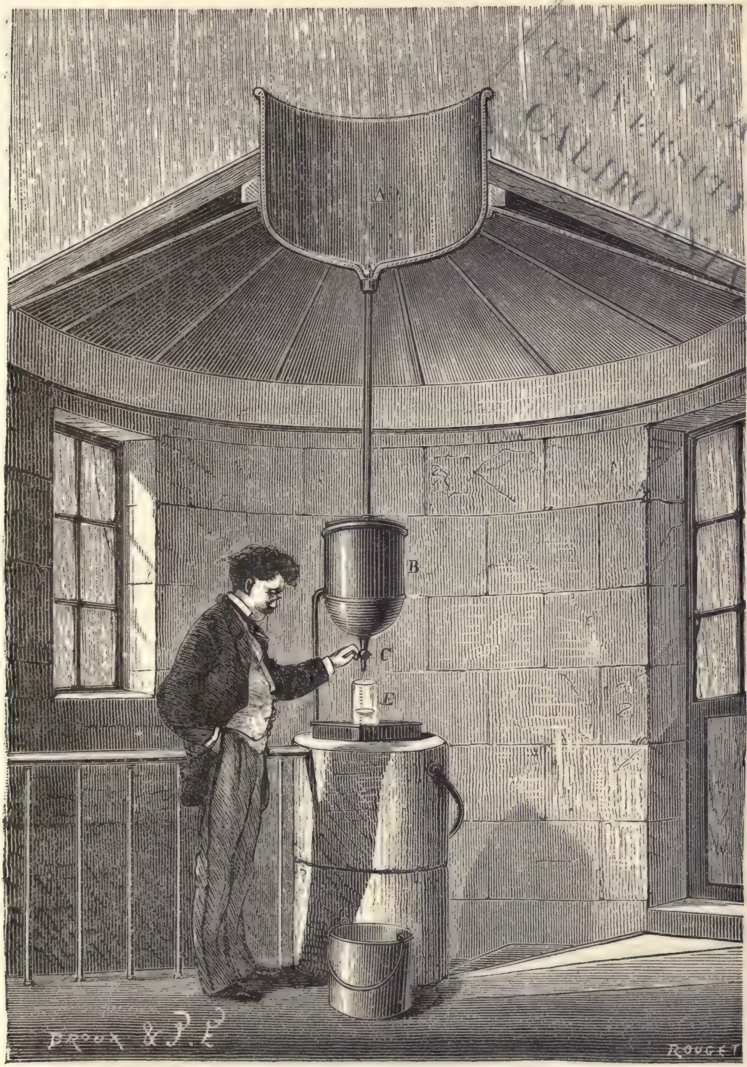

FIG. 329.—RAIN-GAUGE AT THE OBSERVATORY OF PARIS.

A common funnel placed in the neck of a common bottle would form a rain-gauge of a simple construction. Indeed, the majority of the rain-gauges in use consist merely of a funnel of known area, and a receiver of the collected water. The receiver is either itself graduated, or the water is poured off and measured in a graduated vessel. Fig. 329 shows the rain-gauge of the Observatory of Paris. A metallic collecting vessel A catches the rain, which a pipe conveys to the receiver B. It is from time to time drawn at C into the graduated cylinder E. In this way the rate of fall during a storm may be ascertained. The collation of the rainfall statistics has led to many interesting results. Thus a map of the rainfall of Great Britain shows at a glance that in the hilly districts of the western part of our island the rainfall is more than twice as great as in the eastern parts of the island. British meteorology is greatly indebted to the labours of Mr. G. J. Symons for the collection and comparison of rainfall statistics. He, years ago, placed himself in correspondence with a host of observers, mostly amateurs. The number of regularly observed rain-gauges in this country amounted in 1879, according to Mr. Symons, to about 2,200, by far the greater number of the observers being volunteers. "Nearly all the observations are made at 9 a.m., local time; and it often seems to me a remarkable illustration of self-denial and willingness to help science, that as 9 a.m sweeps across the British Isles from Lowestoft to the West of Ireland, no matter how wild the weather may be, forth go some 1,500 or 2,000 persons of all social ranks, from peer to peasant, to make their daily measurement of the amount of rain fallen."

The force of the wind is measured either by its pressure on a surface of known area, or by its velocity. The instruments for measuring wind pressure are in general of a simple construction. A plate is exposed perpendicularly to the wind, and the extent to which it is driven back against the resistance of a spring measures the pressure. This principle was first proposed by Bouguer. Another instrument consists of a rectangular plate hung by a horizontal hinge at the top. The angle which the plate makes with the vertical measures the force of the wind. The plan of using the revolving vanes of a windmill to measure the wind's velocity was first proposed by Wolff in 1743. The instrument now almost exclusively used is that devised by Robinson. It has four arms in the form of a cross, each bearing at its extremity a hemispherical cup. The theory of the instrument shows that the cups will be moved by the wind with one-third of its own velocity.

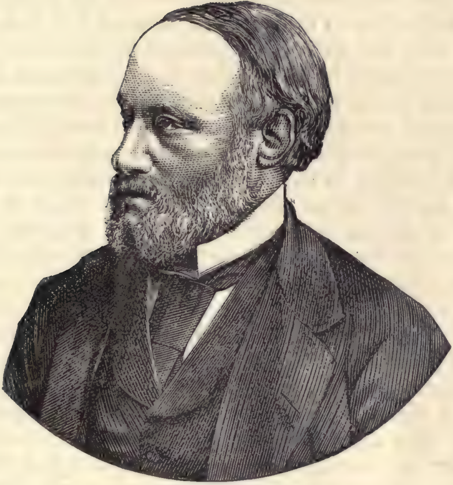

FIG. 330.—JAMES P. JOULE.

# CHAPTER XXIII.

## CONCLUDING REMARKS.

A FEW brief observations on some of the more general characteristics of the science of the present day, and a rapid retrospective glance at certain features of antecedent epochs, will serve to now bring this history to a close.

A review of the whole progress of science presents as one of the most striking circumstances of its history its unequal rate of growth at different periods. That vast inheritance of intellectual wealth which science now includes appears to have been almost entirely the creation of the last four centuries, and as this store continues to increase at an accelerated rate, the question arises whether we can assign any reason for these variations in the rate of its development.

We have seen that the ancient Greeks sought after those wide general laws which it is the object of science to attain; and yet it is admitted that in spite of all their grand intellectual genius they altogether failed in physical science. The cause of this failure has by some been attributed to their neglect of observation, by some to their not employing inductive reasoning, by others to their want of "distinct ideas" or of "appropriate scientific conceptions." It would seem, however, that facts of nature must have surrounded them as they surround us, and certainly the Greeks were far from being an unobservant race, while induction as a process of reasoning is simply the process of every-day life; and, indeed, the proceeding from particular to general truths was recognized by the Greeks as an important logical method. Nor, after glancing at the speculations which have been alluded to in our first chapter, will the reader be disposed to admit that the Greeks can be justly charged with a poverty of ideas, or with a want of distinct ideas. Yet according to the view of a late distinguished writer on the history and philosophy of science, it was the want of "distinctness" in the ideas of these Greek philosophers which caused their failure in physical science. But the same author still more strongly insists upon the "inappropriateness" of the ancient ideas as the barrier to any real progress in science. This phrase is, however, only another mode of *expressing the fact* of the failure, and is not an *explanation of the cause.* The truth appears to be that the ancient Greek philosophers eagerly grasped at generalization, suggested indeed by observation, but unsupported by the slow and laborious processes of experimental *verification;* and they applied these generalities as universal principles, satisfied with any conclusions which might by mental operations alone be clearly deduced. They trusted to merely verbal representations, instead of keeping in view the facts themselves, and they did not understand the importance of that part of the inductive process which is now so carefully performed, namely, *verification.* It is only by *verification* at every stage that induction proceeds safely. There must be verification of the facts and of the conclusions at each step of their ascending scale of generality. The art of scientific observation and discovery had, in fact, to be developed, and, like other arts, its growth was slow. But though there is none, or next to none, of the science of the ancient Greeks embodied in the sciences as they now exist, it must not be supposed that their efforts have done no service to the inductive sciences. They showed at least what were the best solutions that could be yielded by their method of attacking the problems of nature, carried out as it was under the most favourable conditions of intellectual power.

In the Alexandrian school we find observation and experiment in some measure taking the place of speculations as to what *would* happen according to certain assumed principles. Results of permanent value were thereupon attained, and still possess a place in our sciences. Such, for instance, are the "principle of Archimedes" (page 41) and some of his propositions concerning the centre of gravity, and in statics and dynamics. These form a contrast to Aristotle's speculations on mechanical subjects. It is not improbable that had the

Alexandrian school remained in its best condition for a few centuries, the arts of experiment, observation, and rigid induction would have fully developed themselves in its midst.

At Alexandria, Greek science came in contact with other influences. As we have seen, the Arabians were the people who passed on the torch of knowledge to later generations. In the Christendom of that period natural knowledge was despised and neglected. All philosophy and science was held to be false and empty. "To search," says a Christian writer of the fourth century, "for the causes of natural things; to inquire whether the sun be as large as he seems; whether the moon is convex or concave; whether the stars are fixed in the sky or float freely in the air; of what size and of what material are the heavens; whether they be at rest or in motion; what is the magnitude of the earth; on what foundations it is suspended and balanced; to dispute and conjecture on such matters, is just as if we chose to discuss what we think of a city in a remote country of which we never heard but the name." Another author observes that "it is not through ignorance of the things admired by them, but through contempt of their useless labour, that we think little of these matters, turning our souls to the exercise of better things."

The Arabian science was in the main derived from Greek sources, but was affected by a peculiar tinge of mysticism foreign to the true scientific spirit. Thus their chemistry merged into alchemy; their astronomy into astrology; their physics into magic; their mathematics into occult relations of quantity and number. These tendencies were transmitted to the science of the Middle Ages, which was also brought under the influence of Christian theology. This last, which was the engrossing subject of mediæval thought, claimed to be a complete philosophy of nature, of man, and of God. It is true that, directly, theology concerned itself little with any physical questions, except perhaps such as related to the creation of the world and the Scriptural cosmography as then understood. Natural science was, in fact, considered by the Christians as worse than useless, hence its study was in general left to Arabians and Jews, both races inheriting the traditions of the schools of Alexandria. But in alliance with Christian theology there arose in the Middle Ages the *Scholastic Philosophy*, which claimed the character of a universal science. In physical matters it fell into the same fallacy which had misled Aristotle and other Greeks, namely, a confusion of words with facts, of language with things. It assumed that by properly analysing and combining the notions derived from ordinary language, all knowledge could be attained. The details of this philosophy were also in a great measure borrowed from the Stagyrite. As at that time theology comprehended all philosophy, it is easy to understand how scholasticism became almost a part of religious belief, and thus imposed upon the progress of natural science those heavy shackles which were broken at length by the genius of such men as Francis Bacon and Galileo, but only after the supremacy of theology itself had been impaired by other causes. The divergence of science from theology—or rather from a theology which pronounces *à priori* upon the very questions which it is the function of science to investigate—has often been dated from the sixteenth century. In its operation and results this divergence has perhaps been illustrated sufficiently for the purposes of the present work in Chapter V. We unfortunately hear much in our own day about oppositions and conflicts between theology and science, and more particularly in connection with the development theory of organic species, many persons have declaimed against the pretensions of science. But not the acceptance of the doctrine of *Evolution* in its widest application could affect any vital proposition of theology, so profoundly as did the heliocentric theory of Copernicus when it dethroned the earth from its supposed central position in the universe. That ecclesiastics, Catholic and Protestant alike, denounced a theory which appeared to them impious and absurd, and refused to admit the discoveries by which it was confirmed, is a well-known fact; and that their opposition was not always confined to words and arguments, the case of Galileo sufficiently exemplifies. Now that the reality of those discoveries, and the truth of that theory, are admitted by all, who regrets the old notions of the universe, or feels that his theology would be better for a rehabilitation of the Patristic cosmography? Yet in the present century certain geological and biological speculations have been loudly denounced on theological grounds. Opposition of this kind is, however, rapidly dying out, for it is seen that truth can never be opposed to itself, that theology has nothing to fear from any results of physical science, and that its province does not lie among things that can be measured and weighed and dissected. Indeed, there were divines who prepared at once cheerfully to accept the development hypothesis, feeling that it was as reasonable to recognize the law of Natural Selection, acting in the production of varieties or species of organized beings, as to admit that the law of gravitation ruled the motions of suns and planets. The number of those is increasing who welcome such scientific generalizations as tending to exalt their views of the government of the universe, and, by clearing the ground of thorny questions and disputes concerning lower matters, to leave a wider space for enlarged spiritual views.

If we ask for some of the special causes which, beyond the wider influences that have operated in the general intellectual development, have within the last three or four centuries most powerfully contributed to the rapid progress of science, one of the most obvious is the growth of the arts of observation and experiment. These, like other arts, were acquired only in process of time. Nature presents us with complex aggregates of phenomena, from which the causes and effects that are to be studied must be disentangled from a plurality of causes and an intermixture of effects. The ancient philosophers, in their search after unity, assumed many things to be simple which we now know are really very complex. For example, they supposed that air was an element, and that its essential properties were few and its condition uniform. We now know that the atmosphere is a mixture containing nitrogen, oxygen, carbonic acid, aqueous vapour, ammonia, and many other vapours and gaseous matters, most of which are themselves complex substances; and that in it there float also liquid and solid particles of various kinds, ferruginous and other dust, organic fragments, ova of plants and of animals. Moreover, it is capable of many different conditions, and susceptible of infinite variations of these; the known conditions including its temperature, density, humidity, electrical state, etc., while there are doubtless other peculiarities yet to be discovered. The atmosphere is therefore the cause of phenomena in which its numerous different agents may be distinguished and the part due to each assigned, as in the phenomena of animal life, vegetable life, fermentation, putrefaction, so-called spontaneous generation, etc. This example will sufficiently illustrate how manifold may be the agencies which work in what simple observation would assume to be a single cause. Men had therefore to learn to discriminate the cause of a given effect from other agencies which might be present, and to trace the effects of a given cause amidst a complication of co-existing effects. The art of experiment, which has been so powerful in physical science, though it has its methods and rules, is not a method of interrogating nature essentially different from that of observation. The distinction has been happily expressed thus: — Observation is *finding* a fact, experiment is *making* one. In experiment some apparatus or appliances are always necessary, and nearly all observations and facts which are of value in establishing general laws are determinations of the quantitative relations between phenomena. Hence science advances by improvements in apparatus, and especially by increase of accuracy and delicacy in instruments of measurement. It is for this reason that we have in the foregoing pages noticed some of the chief steps in the improvement of instruments for measurement, such as the vernier, the sextant, the micrometer, the pendulum, the chronometer, the torsion balance, the galvanometer, etc. As instruments for the better determination of quantitative relations, we may consider also the invention of new methods in mathematics. Rapid advances in physical science were made when mathematics had provided such instruments as algebra, trigonometry, logarithms, the Cartesian geometry, and the infinitesimal calculus.

In glancing at some of the more general characteristics of the science of the present day, we may consider it first in its practical, and secondly in its speculative aspect. Nothing is more striking than the immediate and general application of scientific discoveries which is continually made to the purposes of every-day life. In this respect science has a relation to life very different from that which she presents in the lofty abstractions of the ancients. The advantage which society and civilization have gained from these applications is a theme which need not here be entered upon. Nor do we propose to adduce a series of examples of such applications, many of which will doubtless suggest themselves to the mind of the reader. In order to illustrate by a single case how science may serve men's ordinary needs, we place before the reader in Plate XVII. a representation of the manner in which the *Times* newspaper was daily made legible to the representatives of the press in Paris during the close investment of the city by the Germans in 18~~67~~70. Photographs of the sheet only a few inches square were taken on paper, and these were rolled up, tied under the wings of carrier-pigeons, and thus conveyed into the beleaguered city. These microscopic photographs were transferred to glass slides as transparencies, and by means of a magic-lantern illuminated by the electric light, the printed characters were projected on a screen whereon the whole newspaper could easily be perused by a number of persons simultaneously. The accumulation of ages of labour and ingenuity, which has made possible the modern arts and sciences concerned in this apparently impracticable feat, will be perhaps more strongly suggested to the reader's mind by the contrast of this scene with that depicted on page 3.

On the speculative side, the main characteristic of the science of the nineteenth century is that it brings into notice the unity and continuity of nature which underlie all the diversities of phenomena. This view is the point to which many lines of research have converged. Some of the more notable steps by which it has been reached have been named in the foregoing pages, as when Galileo perceived in the Jovian system a repetition of our own, when Newton showed that the same force which draws a stone to the earth keeps the planets in their orbits, when Kant and Laplace proposed the nebular hypothesis, when W. Herschel saw that the motions of the immeasurably distant stars are also under the control of gravitation, and when the spectroscope revealed that sun, and stars, and nebulæ are made of the materials for the most part identical with those of our earth. Then there are the discoveries and doctrines mentioned in the last chapter which go to support the unity of the organic world—the establishment of the identity of electricity, galvanism, and magnetism; and, lastly, the great scientific generalization of the nineteenth century, namely, the principle called the *Conservation of Energy* or *Persistence of Force*. This principle, which has been already mentioned in Chapter XIX. (page 524 *et seq.*), asserts that energy is as indestructible as matter; that the quantity of it existing in the universe is invariable, subject neither to increase nor diminution. The various agencies which it is in the province of physics to investigate—Light, Heat, Electricity, Magnetism, Chemical Affinity, Motion—are all so many manifestations of Energy,

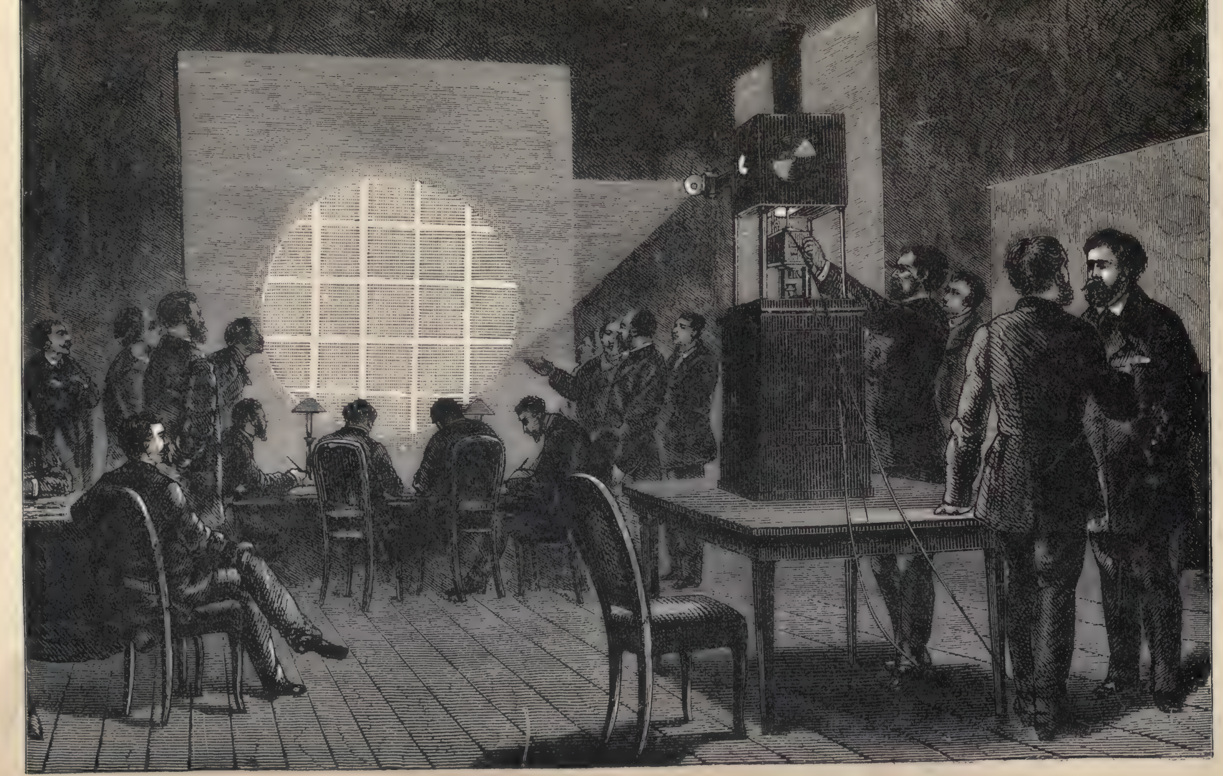

PLATE XVII.—APPLIED SCIENCE.

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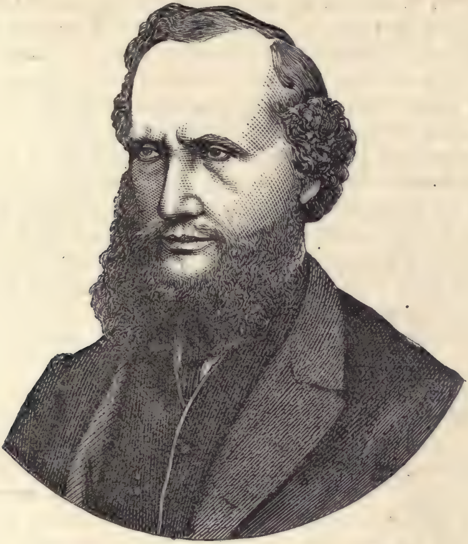

FIG. 331.—SIR WILLIAM THOMSON.

which, Proteus-like, appears now in one form, now in another. These physical agencies are correlated, so that no one of them can be said absolutely to be the essential cause of the rest, but any one of them may produce another. The transformation, however effected, always takes place in definite and invariable quantitative relations. Thus, so much heat will produce so much electricity; and this quantity of electricity will reproduce the original quantity of heat; and so on of the rest. The relations already described as existing between heat, mechanical energy, electricity, and magnetism, for the quantitative determination of which science is indebted to Mr. J. P. JOULE, of Manchester, may serve as examples of these transformations. It was, indeed, as we have already observed, the experiments of Mr. Joule which placed the doctrine of the transmutation and indestructibility of energy on a firm basis.

To Sir William Thomson, the distinguished physicist, some of whose beautiful electrical instruments were referred to on page 573, we owe some researches into the subject of the transmutations of energy which have conducted him to some very remarkable conclusions. Sir William has not been mentioned in the preceding pages in a manner which can adequately represent his signal services to science, especially to electricity, with which he is probably more profoundly conversant than any *savant* living. He was the son of an eminent professor of mathematics, and was born at Belfast in 1824, and graduated at Cambridge as second wrangler in 1845. The following year he was appointed Professor of Natural Philosophy in the University of Glasgow, and he still occupies that position. He has published many physico-mathematical papers on electricity, elasticity, the rigidity of the earth, and other subjects; and, in fact, in many departments of science, his extraordinary mathematical insight has been of the highest service. It is to his profound researches and elegant instrumental contrivances that the success of the submarine telegraph is mainly owing. On the completion of the Atlantic Cable in 1866, he received the honour of knighthood, and many marks of academical distinction have also been conferred upon him. Of all Thomson's investigations those relating to heat and energy are the most remarkable. He has shown that in all the transformations of energy, although in the strict sense there is no loss, yet a portion of the energy takes a form in which it is, so far as we know, no longer available for reproduction into other forms. For example, of the total heat supplied to a steam-engine, only a small fraction is converted into work, the rest being radiated or conducted away, part of the work reappearing as heat in the friction of the bearings of the machine. The heat thus lost without any useful effect is ultimately radiated into space. Now there is no known process by which the low-temperature heat thus diffused is ever again stored up in any form available as a source of energy. The quantity of heat thus diffused, if gathered up into a source of high temperature, would be ready for transmutation into work, electricity, etc.; but, so far as we know, it is in its diffused state like so much water which has descended from a height to the sea-level. This principle, which has been termed the *dissipation of energy*, asserts, then, that the energy of the universe is flowing down, as it were, from a higher to a lower level, and that throughout its varied manifestations, as heat, electricity, motion, chemical affinity, vital actions, etc., this constant degradation, which will ultimately bring all to the dead level of a uniformly diffused temperature, is in operation. This speculation leads to a result exactly the opposite to that of Hutton (page 413), for a process of degradation cannot be eternal. All the forces in our planet may be traced to energy now or in times past derived from the sun; and it is part of the general doctrine of the conservation of energy that the sun's light and heat are maintained by condensation of his material, by aërolites attracted into his mass, or in some similar manner—the process being one in which a state of equilibrium must at length be reached, and the sun himself become a dark rayless mass.

The whole tendency of scientific investigation is therefore to substitute a dynamical view of the universe for a static one. We are no longer permitted to look for stability and eternal endurance in any region of the visible world. Atoms and planets are alike in unceasing motion, but never twice traverse the same space. On our own habitation older races of men and of animals and of plants are silently disappearing, new races continually arising. Are they fixed, the everlasting hills? Our Laureate sings:—

> "The hills are shadows, and they flow
> From form to form, and nothing stands;
> They melt like mist, the solid lands,
> Like clouds they shape themselves and go."

Perhaps the daring image of a venerable living philosopher and poet will best illustrate the aspect in which science now views what appeared to men of a few generations ago the very type of permanence. *He compares the starry heavens to a flight of fire-flies.* And, indeed, could we but view the constellations diminished into the dimensions of a swarm of insects, and ages of duration proportionately shrunk into seconds, such in sober truth is the spectacle that the so-called fixed stars would present to our gaze. We now know positively that many, and we have reason for believing that all, of these far-distant suns are moving with inconceivable velocities. Their fixity is an illusion due to the comparatively brief space of man's existence. This mystic dance of the stars tells us that they also are borne hither and thither by vast all-pervading energies. To those deep and eager questionings which ask, Whence originated this stream of Energy? Whither is it bearing the Universe? and Wherefore? science has no answer. She, looking before and after, sees only obscurity, and must leave the questioner as

> "An infant crying in the night,
> An infant crying for the light,
> And with no language but a cry."

FINIS.

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